FARADAY'S DIARY

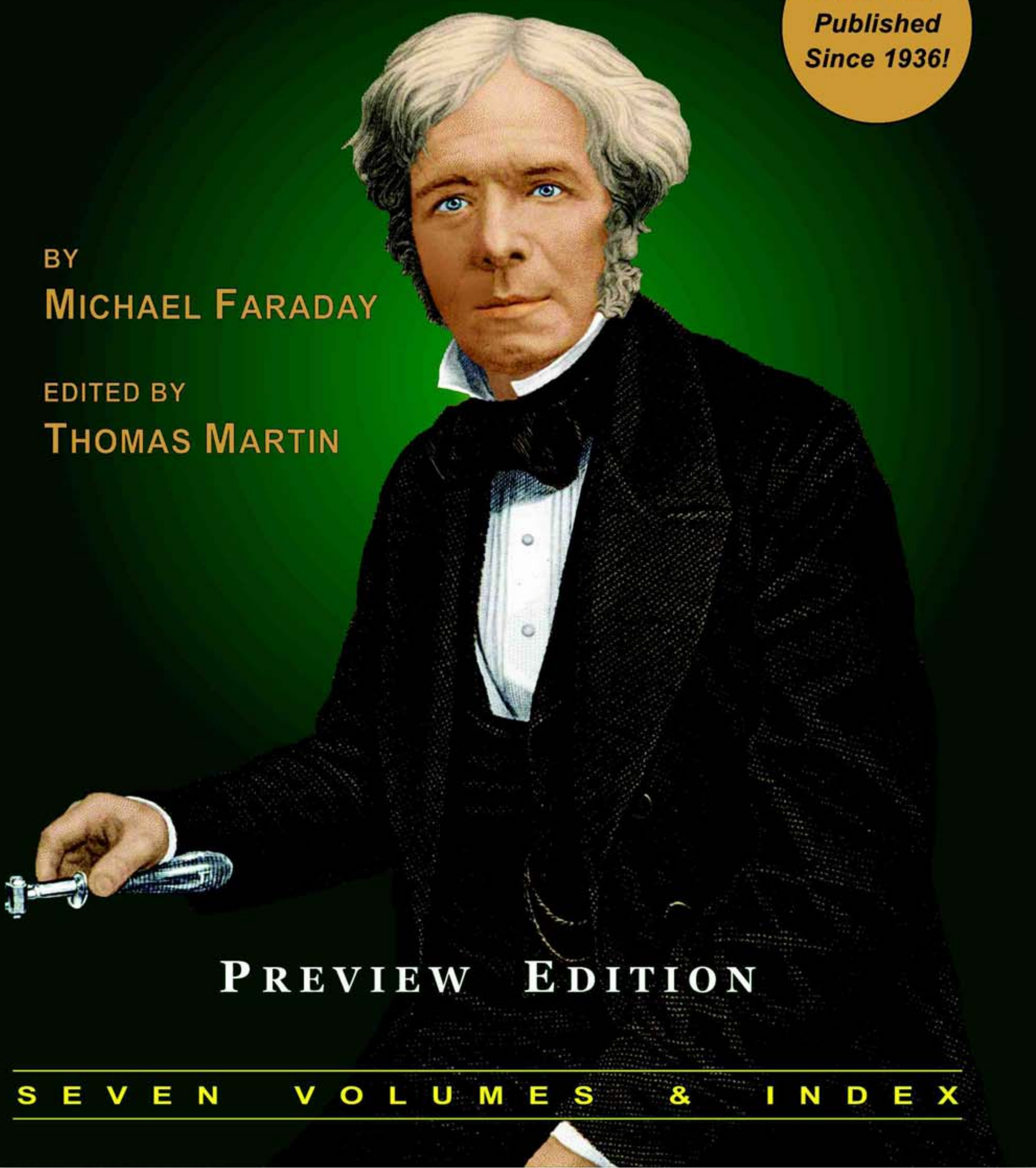
OF EXPERIMENTAL INVESTIGATION

1820 — 1862

First Time
Published
Since 1936!

BY
MICHAEL FARADAY

EDITED BY
THOMAS MARTIN



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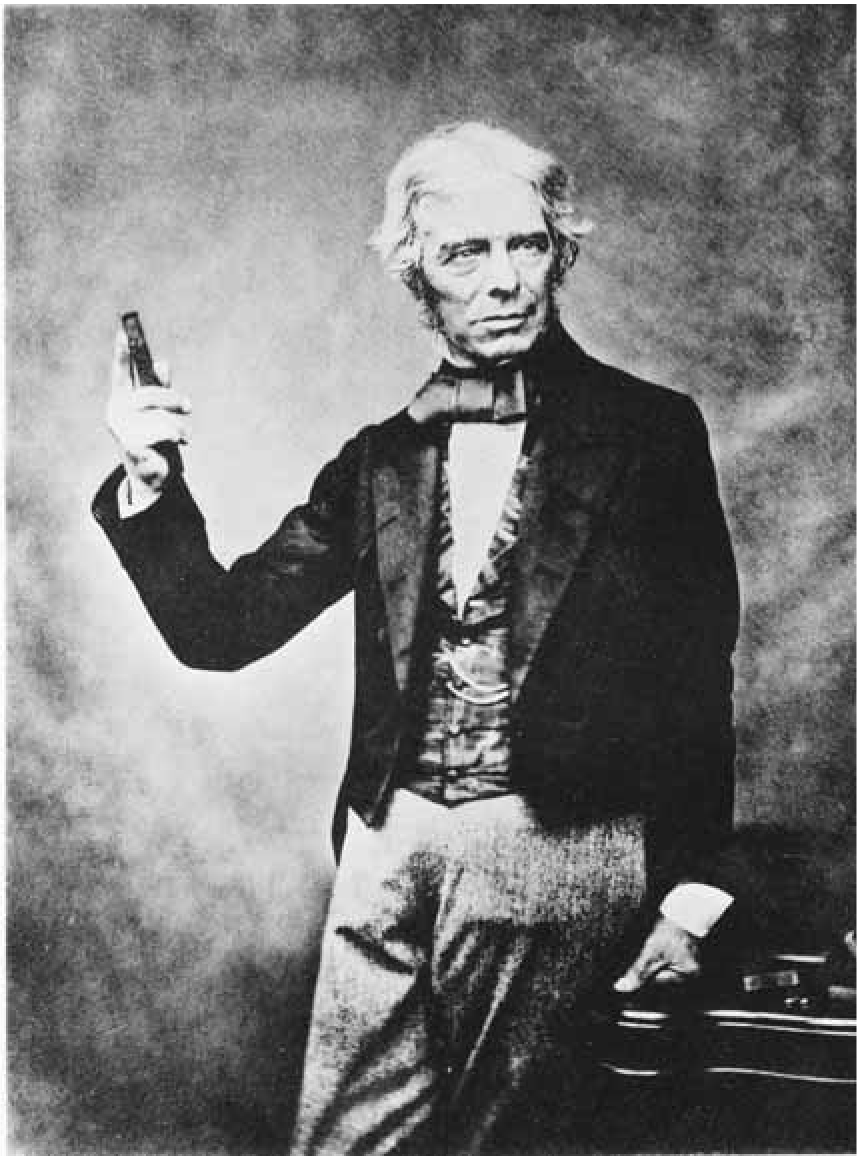
FARADAY'S DIARY

VOL. I



MICHAEL FARADAY

FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

made by

MICHAEL FARADAY
D.C.L., F.R.S.

during the years 1820 – 1862
and bequeathed by him to the
ROYAL INSTITUTION OF GREAT BRITAIN
Now, by order of the Managers,
printed and published for the first time,
under the editorial supervision of
THOMAS MARTIN, M.Sc.

with a Foreword by
SIR WILLIAM H. BRAGG, O.M., K.B.E., F.R.S.
Director of the Laboratory of the
Royal Institution

VOL. I

SECOND EDITION

SEPT., 1820 – JUNE 11, 1832

HR DIRECT

2008

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This second edition of Faraday's Diary (7 vols. with integrated index) is an unabridged republication of the 1936 first edition with the index volume (v.8) integrated into the seven main volumes of the manuscript; comprises the "Experimental Notes" of Michael Faraday during the years 1820-1862; includes first edition photographs and thousands of illustrations by Faraday; published for the first time since the original printing in 1936 under exclusive arrangement with The Royal Institution of Great Britain.

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FOREWORD

THE MANAGERS OF THE ROYAL INSTITUTION OF GREAT BRITAIN have had in their care for many years the manuscript records of the researches of Michael Faraday. They cover the years from 1820 to 1862, during which period he worked in the laboratories of the Institution. He was in the habit of describing each experiment, in full and careful detail, on the day on which it was made. Many of the entries discuss the consequences which he would draw from what he had observed. In other cases they outline the proposed course of a research about to be undertaken. Thus the Diary is far more than a catalogue of results. The reader is able to follow the advance, step by step, to final and fundamental conclusions. He sees the idea forming, its experimental realisation, and its employment as a foothold for the next advance.

The results of Faraday's researches were described by him in papers contributed, for the most part, to the Royal Society. They were collected by him in his well-known *Experimental Researches*. The demonstrations and arguments of the Diary have therefore been given to the world already. The main interest of the Diary lies, however, quite outside the range of propositions and experimental proofs. It centres round the methods of Faraday's attack, both in thought and in experiment: it depends on the records of the workings of his mind as he mastered each research in turn, and on his attitude not only to his own researches but also to scientific advance in general.

Faraday is generally held to be one of the greatest of all experimental philosophers. Nearly every science is in his debt: and some sciences owe their existence mainly to his work. The liquefaction of gases, benzene, electromagnetic induction, specific inductive capacity, lines of force, "magnetic conduction" or permeability, the dark discharge, anode, cathode, magneto-optics, electrochemical equivalent; all these terms suggest fundamental researches which he made, and many of them were called into existence in order to describe his discoveries. It is extraordinary that a man who did such excellent work, should also have excelled in his description of it.

Moreover, the industrial applications of Faraday's discoveries are enormous in their magnitude and variety.

The Managers believe therefore that the publication of this Diary will be widely welcomed, by all who are interested in the growth of a knowledge which has had so large an influence upon the shaping of the modern world, both in thought and in practice, and by those who would know more of the man who wrote it.

The portion of the Diary reproduced in this present volume describes Faraday's work from Sept. 1820 to June 1832. The reproduction of the whole Diary will require six volumes more. The volume contains several descriptions of great interest. In March 1823 he records his experiments on the liquefaction of gases. In 1825 he isolated and examined "bicarburet of hydrogen", now known as benzene: the entries from April to June give a full account of his determination of its chemical composition and reactions. A number of other chemical investigations of various kinds occupied most of his time until 1831. On August 29 of that year he began the long series of researches on electromagnetism which formed the principal work of his life. The first day's experiments showed him the true character of a long sought for effect: it was to be the reciprocal of that which had been demonstrated by Oersted eleven years before and had afterwards been extended by Ampère. An electric current could produce magnetism. Faraday discovered on that day how magnetism could be made to produce an electric current. The work of the next few months cleared the position rapidly: and an entry of March 26 in the following year shows that Faraday had grasped "the mutual relation between electricity, magnetism, and motion".

W.H.B.

THE manuscript which is now generally known as Faraday's Diary has been in the possession of the Royal Institution since its author's death in 1867. The Minutes of the Managers for November 4th, 1867, record the bequest to the Institution by Professor Faraday of his "Experimental Notes", and a marginal entry in the Minute Book, which is evidently a quotation from the actual bequest, giving the date January 16th, 1855, describes the MSS. as consisting of "Various philosophical notes of experimental investigation on foolscap paper, paged in series, and partly bound in five volumes, a quarto MS. book of Philosophical Notes, a second larger quarto of similar notes . . ". Before his death Faraday had evidently had a sixth volume bound, for the minute refers to six folio volumes as well as to some unbound foolscap MS. The loose papers were subsequently bound up, by order of the Managers, making folio volumes 7 and 8.

These two quarto and eight folio volumes, containing upwards of four thousand closely written pages, constitute the Diary as it is to–day. In the commonly accepted sense it is not a diary at all, but a laboratory note book, a day to day record of experiments and observations of a scientific character. At first this record was kept in bound books, the two quartos; but afterwards sheets of plain foolscap paper were used, to be bound later into volumes. The pages are numbered and dated throughout, with the date as a rule methodically repeated at the top of each page. The separate paragraphs are also numbered, except in the early bound books, in which, instead of the numbering, marginal headings have generally been written in vertically at the sides of the pages, for ease of reference.

Overlapping and irregularities in the order of date will be found, particularly in the two quartos, in which it is clear that different parts of the books were in use at the same time, and Faraday turned back to complete entries or to describe further experiments after intervals of weeks or months or sometimes years.

The second quarto is only two–thirds filled, and it is evident that about 1831 he found the plan of keeping his notes on loose sheets much more to his liking. This method was adopted for the work recorded in the first folio volume. In this volume too the numbering of the paragraphs is started, and there are several short sequences up to June 1832. Then in Folio Volume II, on August 25th, 1832, a new series of numbers is begun, and runs in an unbroken sequence of over sixteen thousand paragraphs nearly to the end of the Diary. The dates and paragraph numbers of the original are as follows:

Quarto Vol. I.		Sept. 1820 to Dec. 17, 1823.			
Quarto Vol. II.		Dec. 10, 1823 to Oct. 1, 1833.			
Folio Vol.	I.	(i)	Feb. 1828.		
		(ii)	Feb. 2, 1831 to Mar. 14, 1831.	Pars.	1–72.
		(iii)	June 17, 1831 to July 18, 1831.	„	1–147.
		(iv)	1832 and 1833. Miscellaneous.		
		(v)	Aug. 29, 1831 to June 11, 1832.	„	1–441.
Folio Vol.	II.	Aug. 25, 1832 to Jan. 15, 1836.		Pars.	1–2817.
Folio Vol.	III.	Jan. 15, 1836 to Nov. 5, 1838.		„	2818–5060.
Folio Vol.	IV.	Nov. 5, 1838 to Jan. 10, 1845.		„	5061–7299.
Folio Vol.	V.	Jan. 10, 1845 to Jan. 21, 1850.		„	7300–10740.
Folio Vol.	VI.	Jan. 21, 1850 to Dec. 19, 1854.		„	10741–13591.
Folio Vol.	VII.	Mar. 3, 1855 to Mar. 6, 1860.		„	13592–16041.
Folio Vol.	VIII.	Nov. 25, 1861 to Mar. 12, 1862, and Miscellaneous.			

Many of the pages have a pencil line drawn vertically down the centre. It seems to have been Faraday's practice to cross through in this way passages made use of in the preparation of papers for publication.

The Diary is illustrated with freehand sketches, placed usually towards the right–hand margin of the page, and evidently drawn with the fine pen used for writing. Only very occasionally is a formal drawing produced with the help of ruler and compasses. So numerous are the illustrations in some parts, and so characteristic a feature of the manuscript are they, that the problem of their reproduction in the printed book has called for special consideration. It may be of interest to say that the method of taking photostat prints of all those pages of the Diary containing illustrations has been found the most convenient. The process is much simpler and quicker, and involves less risk of injury to the original, than the alternatives of tracing or redrawing, or of setting up the manuscript and photographing each illustration separately in the usual way. The blockmaker works with equal facility from the paper negative obtained by the photostat process, and the spirit of the original drawing is preserved in the resulting facsimile impressions.

The placing of the diagrams in the margin is no new device in book illustration. It was used in some of the early editions of Euclid, and a particularly fine example of the method, which was consulted at the British Museum, by the courtesy of the Keeper of the Printed Books, when the treatment of the drawings in the Diary was under discussion, is the "Cursus quattuor mathematicarum artium liberalium" of Petrus Ciruelus Darocensis, published in 1516. But if the marginal arrangement of illustrations is not new, it is sufficiently unusual to call for some explanation, particularly as it may be thought to have some relation to the disposition of the diagrams in the original manuscript. And so indeed it has.

In the written pages the diagrams are invariably adjacent to the matter they illustrate; and it was deemed important to preserve this contiguity in the printed text, for ease of reference, and because any alteration of the relative positions of diagrams to text would have led to the introduction of figure numbers which are not in the original, a step which would have altered to some extent the free character of the Diary. But with the reduced scale of the printed as compared with the written page, correct placing would often have been difficult, and on crowded pages impossible, if the general practice of insetting the diagrams in the type area had been followed. The difficulty will be apparent on reference to some of the heavily illustrated pages.

With the diagrams removed to the margin, each one is placed opposite the paragraph it illustrates; except when its shape or size, or the proximity of others, makes it necessary to place it at the foot of the page, in which case a reference mark and number in square brackets denote the paragraph to which it belongs. This arrangement of the illustrations has the added advantage that it gives an unbroken text for reading; while the facsimile sketches, with their freehand lines, gain by being less closely contrasted with surrounding type.

The majority of the illustrations in this volume have been reduced in size to three–quarters of their original linear dimensions. It has seldom been necessary to use a greater scale of reduction, for in the manuscript the diagrams are generally small. In some cases they have been reproduced full size.

The text has presented a number of those problems which must always attend the reproduction in type of a manuscript not designed by its author for publication. The handwriting in the original is generally clear and legible, and with experience in reading it acquired during the progress of the work, doubtful passages have usually been resolved on returning to them, but a few isolated words or short sentences have proved undecipherable. For these it has been thought better to print the word "illegible", enclosed in square brackets, rather than to guess at the meaning. Very few variations from the original text have been allowed. Here and there, however, a missing word or part of a word has been inserted, where it assists to make the meaning clear. All such additions are enclosed in square brackets. The pages of the printed book cannot, of course, correspond with those of the manuscript, so that some variation from what is actually written has been necessary in the matter of repetition of dates, and of marginal headings where these are used; but in this Faraday's own general practice has been followed, of repeating the last new date or marginal entry at the head of the next page.

It is hardly necessary to say that peculiarities of spelling have been everywhere retained; even when, as sometimes happens, a word is differently spelt at two places on the same page. Redundancies, as when a word has been inadvertently repeated, or where there is a clear intention to alter a sentence and a word or words have not been struck out, have generally been omitted. Blanks, that is, spaces in the text for numerical or other entries which have never been filled in, have been left as blanks.

Perhaps the most difficult problem has been that of punctuation. In the Diary, Faraday frequently disregarded grammatical construction, and he even more frequently wrote without, or almost without, punctuation. Occasionally the customary punctuation marks were used, with capitals following the full points. In other places the only sign is a short horizontal stroke or dash. Elsewhere long passages occur without break or indication of any kind, except such as can be got occasionally from the spacing of the words.

The dashes have been preserved in places where they occur; and punctuation has been inserted throughout the book, but sparingly, and with the objects of removing ambiguity and assisting the reader rather than with a pedantic insistence on the inclusion of every comma. Not infrequently, and particularly in ungrammatical sentences, the meaning is determined by the punctuation. If in such sentences the meaning has remained in doubt after full consideration, punctuation has not been added.

Another problem has been that of the abbreviations, which in some parts are much used. Faraday sometimes slurred the ends of his words in writing in such a way as to leave it in doubt if an abbreviation was intended or if the word was only hurriedly written. It has been difficult to establish a consistent practice in dealing with such cases, but as far as possible only words in which it has been judged that a definite shortening was intended have been printed as abbreviations. All such words are followed by a full point in the text. Of abbreviations regularly used, the majority are self–explanatory, but the short list given on page xxv may be of assistance.

A special case is that of the word “and”. Although Faraday sometimes wrote this in full, he more frequently used a looped sign for it, rather like the Greek *a*, which would have been very imperfectly represented by the printer’s ampersand. Moreover, the frequent occurrence of this sign in the text would have been typographically objectionable; and the word “and” has therefore been printed in full wherever it appears. A sign which is hardly distinguishable from that for “and” was used for “et cetera”. This has been printed in the usual abbreviated form of “etc.”

The conventional signs (* † ‡ § || ¶) have been used only for references to diagrams placed at the foot of the page. For references from one part of the manuscript to another Faraday sometimes used marks of his own devising, and to represent them certain special signs have been used which may readily be distinguished from those used for the diagrams. References to editorial footnotes are by numerals.

Lastly it may be said that the object in preparing this volume for the press has been to present in type as accurate a representation of the original as its form allows, and to preserve in the printed page at least some of the characteristics of the manuscript. Editorial comment has been indulged in as little as possible, and has been confined strictly to what is necessary for the elucidation of the text.

T. M.

ROYAL INSTITUTION
June 1932

Cross references to Faraday's published papers have been given in the table of Contents, so that readers may compare the entries in the Diary, made during the progress of the experimental work, with the accounts of the same work which were subsequently written and published. The papers originally appeared in the *Philosophical Transactions of the Royal Society,* the *Quarterly Journal of Science,* the *Philosophical Magazine* and other journals, but they were afterwards collected by Faraday and printed in book form, in the *Experimental Researches in Electricity,* Vol. I (R. and J.E. Taylor) 1839, Vol. II (R. and J.E. Taylor) 1844, Vol. III (Taylor and Francis) 1855; and the *Experimental Researches in Chemistry and Physics* (Taylor and Francis) 1859. They are most conveniently consulted in these volumes, to which the page references have therefore been given, instead of to the original papers.

The inclusion of these cross references affords an indication of the extent to which the Diary was drawn upon by its author in preparing his published works, and enables the Contents to be used as a guide to all those other experiments and miscellaneous observations which were recorded from day to day, but which led to no results considered worthy of publication at the time, and of which no account has previously been printed.

QUARTO VOLUME I OF MANUSCRIPT

QUARTO VOLUME II OF MANUSCRIPT

FOLIO VOLUME I OF MANUSCRIPT

PLATES

INDEX

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Some abbreviations used by Faraday

bar. or B., baryta or barytic; also barometer.
C.A., carbonic acid.
c:i:, cubic inches.
cor. sub., corrosive sublimate.
M.A., muriatic acid.
mur. or M., muriate or muriatic.
N.A., nitric acid.
N.M., nitro-muriatic.
nit. or N., nitrate or nitric.
oleft., olefiant.
S.A., sulphuric acid.
sul., sulphate or sulphuric.
sul. cy., sulphocyanate or sulphocyanic.
sul. hy., sulphuretted hydrogen.
suls., sulphurous.
sulet. or sulrt., sulphuret.

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QUARTO VOLUME I
OF MANUSCRIPT

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ARTIFICIAL CAMPHOR. Rather heavier than water when fused—requires a higher heat than 212° for its volatilisation—readily fuses and is not so volatile as the new substance—burns with bright flame. It will bear fusion with potash for some minutes without decomposition. Heated with oxide of Zinc it is immediately decomposed giving mur. zinc and oil turpentine. Heated with Zinc it act on it evolving much Mur. Acid gas, probably also hydrogen and forming Mur. Zinc and oil of turpentine which remain fluid.

ARTIFICIAL CAMPHOR.

Artificial camphor in chlorine—heated, partly sublimed and fused—much M.A. Gas formed, but bulk of gas not changed—hence chlorine absorbed, hydrogen evolved. Substance now seems purer than before, the first kind being probably a mixture of this with oil of turpentine. It was now not acid—volatile—crystallisable—not by any means so combustible as before, but when burning giving off very much smoke and M.A. Gas—soluble in Alcohol, etc. Acting violently on oxide of zinc making it of a very dark red or crimson colour and forming chloride of zinc—water—a little bituminous looking substance and a volatile combustible fluid matter exactly resembling in appearance and *smell* the volatile oil produced by distilling fixed oil 3 or 4 times.

Put a larger portion of the camphor into a retort and acted on it by chlorine, melting the substance in it. Nearly the whole of the gas became M.A. Gas—blew this out and put a second atmosphere in—whilst heating the substance in the retort it took fire within, the hydrogen burning and the charcoal depositing. It was only the vapour which burn[t], the portion below remaining unchanged except by a little dirt.

Put in fresh chlorine and it again burnt on heating—did so a third time—the fourth time it did not, the heat being very gently applied. The substance taken out—dissolved in alcohol—crystallised, etc. and examined proved still to be the same substance as before.

CHLORIDE OF CARBON.

Chlorine and olefiant oil exposed in a retort to sun light soon act; the vessel becomes misty, the colour of the chlorine disappears, a little heat is extricated and the bulk of the gas perhaps from that cause appears increased. The gas contains much M.A. and there is a smell as of Phosgene gas. (Query oxygen present?)

Dendritical crystals gradually form; these may be washed in water, dissolved in Alcohol and crystallised.

The Substance is transparent—colourless—volatile and crystallising in flat dendritical crystals sometimes half an inch long—it [illegible] to the light—smell strongly aromatic and very like artificial camphor—soluble in cold alcohol and more soluble in hot—crystallises from hot solution in dendritical crystals or in prism (quadrangular?)—soluble in ether—precipitated from the Alcohol by water and then its smell very strongly developed. It is volatile in the air—a small thin crystal took about 2 hours to evaporate—has a slightly aromatic taste.

It is much heavier than water—alcoholic solution precipitated by water and left to stand, the substance gradually settles in a fine white powder. May be washed from acid in this way. Crystals of it instantly sink in water.

Put into a tube with water and heated, the substance did not fuse under the water or dissolve in it but it softened and then became vapour long before the water boiled. In this way it may be sublimed in water and well washed—the water brought off acid which had adhered to it. It was then dissolved in alcohol, a saturated hot solution being made; as it cooled beautiful dendritical crystals formed in the solution resembling those formed by sublimation.

A little of it heated in a glass tube with oxide of Zinc was partly decomposed and in part escaped—that which volatilised burnt when sent into the midst of the flame of a spirit lamp with a yellowish flame and smoke but ceased to burn the moment it was removed out of the flame, and the substance appeared in white fumes, easily condensible—the vapour apparently heavy—the oxide of Zinc in the tube blackened and when washed out by water a solution of muriate of Zinc was obtained. Query permanent gas liberated?

CHLORIDE OF CARBON.

When Alcohol holding it in solution is burned acrid fumes are given out and M.A. Gas made evident by ammonia.

Query Its Spec. Gravity?

The quantity of it dissolved by hot Alcohol?

" " " " cold alcohol?

" " " " water?

The weight of its vapour?

The form of its crystals?

Under what pressure it fuses?

The Elasticity of its vapour?

1820. OCTR. 3RD.

Put Oleft. oil into a retort—exhausted—introduced chlorine—exposed to light—after the action introduced a little water—this absorbed the M.A. Gas and made a fresh vacuum—let in more chlorine and again exposed to light, shaking the retort—fresh action, then more chlorine—when the substance all changed, filled up the retort with water so as to wash out the acid well—repeated washing—dissolved the substance in alcohol and crystallised. This saves exhausting by the air pump which is very injurious to the instrument.

OCTR. 4TH.

Made a similar expt. to the above in the large head, first filling with chlorine then letting in Oleft. gas—forming the oil—afterwards giving more chlorine—exposing to light—putting in water, etc.—much of the substance formed.

Got a retort to exhaust, capacity 34·5 inches; put in 8·6 c:i: oleft. gas, then 27 chlorine—exposed to light—oil formed and afterward the excess of chlorine acted on it to form substance—when colour gone, contraction of volume, and 8 c:i: more of chlorine entered, making 35 on the whole—exposed to weak sunshine—colour nearly gone—no further condensation—a good deal of crystals formed but much oil remaining unchanged—not chlorine enough in this experiment.

Some of the gas forced out over Mercury by heat: 12·5 gave 9·5 M.A. Gas absorbed by water and 3 of an uninflammable gas; this was a mixture of Carbonic acid gas and nitrogen; 4·5 became

CHLORIDE OF CARBON.

4 by potash hence the 12·5 = 9·5 M.A.G., 2·667 Nitrogen and 333 carbonic acid.

Common air appears to have been with the chlorine and there was not enough of the latter. Try 5 vol. of chlorine to 1 of Oleft. gas.

Detonate vapour with oxygen.

„ „ „ hydrogen.

Send it over red hot oxide of copper or Zinc.

1820. OCTR. 5.

The crystals of the substance (from a strong alcoholic solution) were very brittle and crumbled into a white powder very easily. It is on this account difficult to preserve them. They were taken out of the Alcohol, dried by pressure between filtering paper, exposed to the air for half an hour and then put into a bottle. The substance was then a white dry powder.

The Alcoholic solution spontaneously evaporated; left crystals of the substance but they evaporated also in an hour or two afterwards.

The crystals by sublimation are much tougher than those formed from solutions.

The substance dissolves much more readily in Ether than in Alcohol. A hot solution of Ether deposits crystals as it cools. A glass rod dipped in it and exposed to the air is instantly covered with the substance in white powder from the evaporation of the ether.

Query acidity of solution? My ether was acid.

A drop of the etherial solution put on a glass plate instantly expands, evaporates and its surface becomes covered with square crystalline plates, the crystals being dendritic and their axes lying parallel to the diagonals of the square. In this way the substance may be got very dry.

Water dissolves but a very small portion of it when boiled with it.

The solution of it in Alcohol is not acid—and is not precipitated by Nitrate of silver.

Solution of potash does not dissolve it perceptibly by boiling—nor Ammonia (strong). Muriatic acid does act on it.

CHLORIDE OF CARBON.

Nitric acid (strong) boiled upon it dissolves a portion but does not decompose it: as it cools the substance deposits again unaltered. The concentrated acid diluted lets more of the substance fall; and then filtered and tested by N. of Silver gave no precipitate—hence no chlorine separated from the substance by it.

Put into strong Sul. Acid it very slowly sinks to the bottom, hence its S.G.; boiled with the acid the acid became brown, probably from some little pieces of dirt that were mixed with the substance. The substance sublimed from and through the acid unaltered and the acid tested contained no Mur. Acid or chlorine. It was not precipitated by water, hence no substance dissolved.

Clean dry retort: its capacity when tried by the air pump 34 c:i: but the air pump does not exhaust close. When measured with water its real capacity 35 c:i:.

This was first filled with nitrogen to exclude oxygen.

Then 3 inches of Olefiant gas let in and 31·5 inches of chlorine.

The chlorine examined and found to contain 1/40, of common air.

Exposed at 2 o'clk. to sun light for 40′; at first the fluid formed and afterwards crystals—the excess of chlorine remained. At the end of that time it seemed perfectly dry and all action had ceased. It was now cooled to the Laboratory temperature.

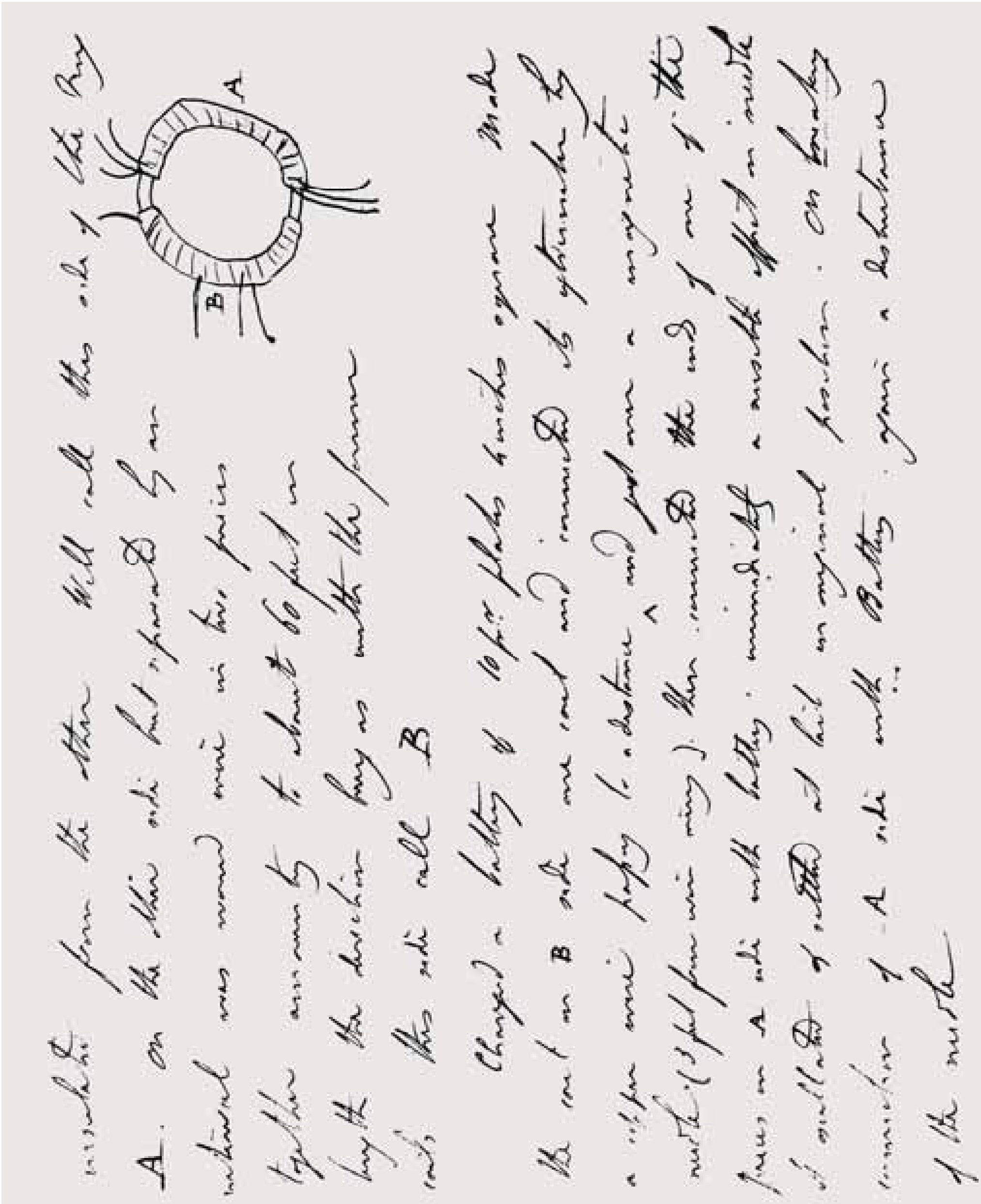
Put it on to a jar of chlorine and opened the stop cocks—there was contraction of volume and 5·5 c:i: of chlorine entered, making 37 of that gas altogether.

A strong solution of chlorine was now made and admitted into the retort to absorb the M.A.G. It rose to a mark in the bulb of the retort which being afterwards measured equalled 13 inches absorbed, and the unabsorbed part being also measured = 22 c:i: —the absorption would have gone on still farther but from its slowness was evidently owing to the chlorine being taken up.

The 22 c:i: shaken in water were all absorbed except 1·75 which were azote; and the 20·25 were therefore chlorine—a very slight portion of C.A. was there.

The gases therefore in the retort were

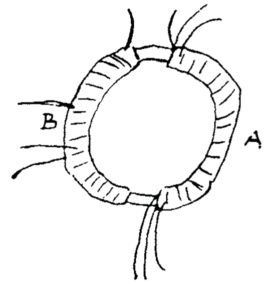
20·25 c:i:	chlorine
13	M.A. gas
1·75	Nitrogen



The entry recording the first successful experiment in electro-magnetic induction. August 29, 1831 (*slightly reduced*)

1. Expts. on the production of Electricity from Magnetism, etc. etc.

2. Have had an iron ring made (soft iron), iron round and 7/8 inches thick and ring 6 inches in external diameter. Wound many coils of copper wire round one half, the coils being separated by twine and calico—there were 3 lengths of wire each about 24 feet long and they could be connected as one length or used as separate lengths. By trial with a trough each was insulated from the other. Will call this side of the ring A. On the other side but separated by an interval was wound wire in two pieces together amounting to about 60 feet in length, the direction being as with the former coils; this side call B.

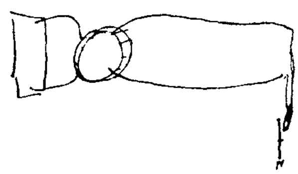


3. Charged a battery of 10 pr. plates 4 inches square. Made the coil on B side one coil and connected its extremities by a copper wire passing to a distance and just over a magnetic needle (3 feet from iron ring). Then connected the ends of one of the pieces on A side with battery; immediately a sensible effect on needle. It oscillated and settled at last in original position. On *breaking* connection of A side with Battery again a disturbance of the needle.

4. Made all the wires on A side one coil and sent current from battery through the whole. Effect on needle much stronger than before.

5. The effect on the needle then but a very small part of that which the wire communicating directly with the battery could produce.

6. Changed the simple wire from B side for one carrying a flat helix and put the helix in the plane of the Mag. Meridian to the west of the S pole of the needle, so as to shew best its influence when a current passed through it—the helix and needle were about 3 feet from the iron ring and the ring about a foot from the battery.



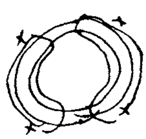
7. When all was ready, the moment the battery was communicated with both ends of wire at A side, the helix strongly *attracted* the needle; after a few vibrations it came to a state of rest in its original and natural position; and then on *breaking* the battery connection the needle was as strongly *repelled,* and after a few oscillations came to rest in the same place as before.

8. Hence effect evident but transient; but its recurrence on breaking the connection shews an equilibrium somewhere that must be capable of being rendered more distinct.

9. The direction of the pole towards the helix was, when the contact was first made, as if the helix round B was a part of that at A, i.e. the electric currents in both were in the same direction; but when the contact with the battery was broken the motion of the needle was as if a current in the opposite direction existed for a moment.

10. Had a short cylinder of irons 7/8 thick, 4 inches long, and coiled round with 4 pieces of wire each about 14 feet long: made these coils into one and substituted this in place of the flat helix. The needle was affected as before, but not at all as if the iron had helped to develope magnetic power—not more than helices round it would probably have done without the iron. It was the same transient and inverted states as before.

11. Removed the iron and helices and substituted two platina poles to ends of B coil; put these into solution of copper, lead, etc. etc., but could get no evidence of chemical action. Put solution of copper on to one pole and then touched the drop with the other; then connected the battery, then broke connection at drop, and then at battery, and so went on in succession so as to avoid the recurrence of the return or opposite current on the drop: but got no evidence of chemical action.



12. On making *all* the wire round the iron ring one helix and sending current from battery through it, and also hanging a magnetic needle over the ring, one pole being in the middle at the point of suspension, the needle darted about irregularly and shewed poles—two N and two S. On putting paper over the ring and sprinkling filings over it also see the 4 poles, but were irregularly placed. Iron probably not soft but evidently not a perfect conductor, for the parts between the ends of the two general helices of A and B were of very different magnetic power to the ends of the helices.

13. Put a helix (of brass brace spring) round a glass jar and brought a needle within it in various positions, but it behaved merely as a single ring of wire would have done.

14. Repeated (6): continued the contact of A side with battery but broke and closed alternately contact of B side with flat helix. No effect at such times on the needle—depends upon the change at battery side. Hence is no permanent or peculiar state of wire from B but effect due to a wave of electricity caused at moments of breaking and completing contacts at A side.

15. Tried to perceive a spark with charcoal at flat helix junction B side but could find none. Wave apparently very short and sudden. No use trying platina wire. Not sure large battery would not produce spark.

15*a*. Then disjoined the three portions of wire on A side—made two into one helix and sent battery current through that—and connected the third portion with the flat spiral and needle, etc. so as to represent B side. Effects on needle stronger than before but same in character, occurring inversely, etc. on breaking battery connection, etc. etc.

16. A large bar magnet brought in contact with the ring caused no change at the flat helix.

17. May not these transient effects be connected with causes of difference between power of metals in rest and in motion in Arago's expts.?

18. Took the iron cylinder (10) and connecting two of the wires into one Helix and the other two into another, connected one of these Helices with the flat spiral and needle and the other with the battery—immediately a sharp short pull upon the needle, the effects being exactly as before but not so strong. Hence a ring magnet is not wanted.

19. Brought the poles of strong magnets in contact with ends of the iron cylinder, but found no difference upon the needle at the flat spiral—all the effects seem due to the Electrical current only.

SEPT. 12, 1831.

20. Have prepared several coils, helices, etc. etc. etc. Coil A consists of various lengths (as under) of copper wire, string being interposed between the turns of each coil and calico or linen between the different coils.

Coil B was composed of alternate copper and iron (see lengths beneath), the iron either covered with cotton or else separated as before.

A	B
20—6	C—26—6
20—5	I—30—6
21—4	C—31—5
24—3	I—31—5
25—2	C—36—4
26—1	I—38—4
27—6	C—38—3
28—5	I—37—3
28—4	C—39—2
29—3	I—37—2
31—2	C—38—1
31—1	I—41—1
310	422
	C—208
	I—214

These lengths are the lengths of coil (not including the projecting ends), and the core in each is of wood.

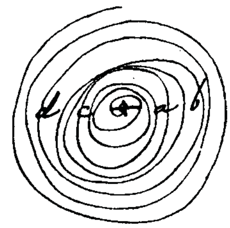
C a flat spiral of covered iron wire containing about 6 feet.
D a double flat do. do. 19 feet.
E a do. copper wire uncovered do. 14 feet.
F a cylindrical round solid helix of covered iron wire containing about 12 feet.
G do. of 31 feet.
H a double flat spiral of covered iron wire of 18 feet.
I a coil of covered iron wire 35 feet about and 2½ inches mean diameter, forming a thick ring; this then covered by a helix at right angles to it of two lengths of copper wire 40 feet each or 80 feet together, these being separated by string and calico from each other as in former cases.
K The iron ring covered of (2).
L The covered iron cylinder of (10).

SEPTR. 24, 1831.

21. A tried—each length independant and right—half the lengths (i.e. 1, 3, 5, 7, 9, 11) made into one helix, the other half (i.e. 2, 4, 6, 8, 10, 12) into another helix by connection, so as to form two helices closely interposed, having the same direction, not touching anywhere and each 150 feet long. One helix connected as in (6) with flat helix and the other with battery of 10 pr. plates 4 inches square. Not the slightest effect on needle at flat helix either at time of contact or disunion or any other way. No induction sensible—contacts all perfect.

22. Then B, the final copper helix containing 208 feet wire, the final Iron helix 214 feet, but no effect on needle, though sometimes the battery current was sent through the iron, sometimes the copper; the iron seemed to do no more than the copper.

23. Used H. When brought towards pole of needle concentric with it, the pole seemed to be repelled towards edge in any direction going from +. In fact the pole of needle in inducing magnetism on the wire could do it better at *a* than at + , and better at *b* than at *a,* opposite poles no doubt being formed at *c* or *d*. Would be it so also with a continuous plate as well as a flat helix. Evident therefore that magnetic action tends to arrange particles longitudinally in the direction of its own axis and is itself powerfully arranged by previous arrangement of iron particles—important influence thus exerted.



24. When H connected with the battery then the centers were the poles, and there was nothing particular in the magnetic action evolved. The moment the connection was broken the magnetism was lost.

25. The double flat iron helix H connected with wires leading away, as in (6), to flat copper helix and needle, and then pole of a powerful bar magnet brought flat against iron helix and suddenly removed to distance—but no effect produced at test helix beyond that of motion of magnet itself.

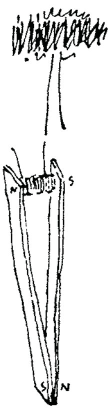
26. The flat helix H put between N and S poles of the two bar magnets, the lower ends of bars touching, and then the upper poles put close to or kept at distance from H—but still no effect at indicating helix at distance.

27. A flat copper helix E substituted for H and experimented with but still no effect.

28. Two double iron spirals such as H but side by side—one connected with indicating spiral at distance and a current from 10 pr. of plates and trough sent through the other. No sensible inductive effect.

29. I. Tried in various ways by currents through iron or copper but no effect at indicating coil. Magnetic poles brought quickly to it and then removed, still no effect.

30. When copper of I carrying current no particular magnetism in the iron that was sensible to needle; those parts of iron wire ends nearest to rings were powerful poles for wire but not powerful for helix used and for battery.



31. Covered Iron wire bent to and fro like a cracker; when current through it no particular action.

32. The iron ring K and cylinder L acts as before and very well.

33. The iron cylinder and helix L. All the wires made into one helix and these connected with the indicating helix at distance by copper wire: then the iron placed between the poles of bar magnets as in former expt. and in fig. Every time the magnetic contact at N or S was made or broken there was magnetic motion at the indicating helix, the effect being as in former cases not permanent, but a mere momentary push or pull. But if the electric communication (i.e. by the copper wire) was broken then these disjunctions and contacts produced no effect whatever. Hence here distinct conversion of Magnetism into Electricity.

34. Perhaps might heat a wire red hot here—try with Marshes magnet.

SEPT. 29TH.

35. M. Put two coils of copper wire round block of wood, string intervening and the coils alternating. Each coil had 34 turns of 73 inches each—each was therefore 2432 [*sic*] inches or 202·8 feet in length. Each had one joint but bright and well twisted.

OCTR. 1, 1831.

36. A battery of 10 troughs each of 10 pr. of plates 4 inches square charged with good mixture of sulphuric and nitric acid, and the following experiments made with it in the following order. The discharge of the battery between charcoal points was very powerful at the first and very good at the conclusion.

37. One of the coils of M connected with the flat helix as in expt. (6), and the other with the poles of the battery (it having been found that there was no metallic contact between the two). The mag. needle at the indicating flat helix was affected but so little as to be hardly sensible.

38. In place of the indicating helix our Galvanometer was used and then a sudden jerk was perceived when the battery communication was *made* or *broken,* but it was so slight as to be scarcely visible—it was one way when made, the other when broken, and the needle took up its natural position at intermediate times.

39. Hence there is an inducing effect without the presence of iron, but it is either very weak or else so sudden as not to have time to move the needle. I rather suspect it is the latter. Use a hollow helix in place of indicating galvanometer and put needle in to magnetise. Compare with effect with Iron present also.

40. The Galvanometer tells better than the flat helix.

41. Endeavoured to obtain evidence of Chemical action as in (11), but could not. Probably the interference of fluid conductors is enough to stop the wave.

42. Could get no heating effect with Platina wire or spark with charcoal with this arrangement at induced side.

43. When a small battery was introduced on induction side so as to send a continual current through that helix and constantly deflect the galvanometer needle, the making and breaking of the contact on the other side was not more sensible if so sensible as with the helix alone. And as contact was made in opposite directions it would appear that when currents in both wires there was little effect or none beyond that with no first current.

44. Now used the Ring K instead of these large coils—all other things for induction, etc. remaining the same as in the former expt., and in (6). With index helix there was powerful effect, and with indicating galvanometer very powerful, pulling the needle quite round, but still it was only momentary. The needle settled as at first though contact continued, and when contact was broken the needle was pulled for the moment in the opposite direction with equal force.

45. Decomposition as at (11)—could not perceive the least trace.

46. Got a spark with charcoal at the end of the inducing wires, very distinct though small—only at the moment of contact or disjunction. Tried to heat a platina wire but probably failed because of too great thickness of wire.

47. Deflected the galvanometer needle by use of little battery (   ) and then passed battery charge through the other helix—but obtained no extra action.

48. When the Ring K was held near to the cap of an excited electrometer, the contact of one of the battery wires with the helix caused a little increased divergence, but on completing the contact it fell to its first position. The effect seems due to the whole mass becoming feebly electrical with the pole which first touches it, and acting by induction on the electrometer. It is nothing important.

49. Two double flat iron helices each containing 19 feet of wire D were placed side by side, one connected with the galvanometer and the other with the battery, but no effect on galvanometer occurred.

50. See old expts. on magnets in helices.

51. The two helices (iron) D when hung about 2/3 of inch apart powerfully attracted each other at moment of communication with battery, the direction being the same in both, but in a moment or two continuing the contact the attraction ceased—apparently entirely. Upon breaking the contact for a minute or two and then renewing it the attraction was reproduced, but when contact was broken for a shorter period the attraction was not so strong, and when broken for an instant only it scarcely appeared at all. When iron coils first receive magnetic state they attract strongly; when contact broken they lose that peculiar state gradually, and if before it is much diminished it be renewed attraction is only small. Attraction proportionate to approximation to natural state and to intensity of peculiar state to which it is rising. Query would this happen with copper helices.

52. Cylinder L arranged with indicating galvanometer etc. produced its effect but nothing to compare with ring K.

53. Attraction of filings with Cylinder L very good—a very fine bunch, inch or more long, fell off moment contact broken—good class expt.

54. Arranged Sturgeon's plate for Vibration-20 vibrations were necessary to diminish it from one arc to another—then hung two iron helices D over the edge of the plate so as to act as the poles of a magnet on the sides of the plate, but there was no sensible difference in the number of vibrations. Then used two poles of two bar magnets instead of the helices, and then 10 vibrations made the same diminution in amplitude of vibration as 20 before or thereabouts. But the poles of the magnets were far stronger on a needle than the helices were. Still doubt that pure Electro-magnets will produce Arago's effect.

OCTR. 17, 1831.

55. Have prepared two pieces of apparatus—N a piece of musket barrels 7/8 of inch in diameter, variable in thickness but about 1/16 of inch. This was covered by one piece of copper wire 61 feet 4 inches long, in a helix passing from end to end and back again (in the same direction), so as to surround the barrel four times.

56. O a cylinder, hollow, of paper, covered with 8 helices of copper wire going in the same direction and containing the following quantities

		f.	in.
1 or outermost		—32	10
2	—	—31	6
3	—	—30	
4	—	—28	
5	—	—27	
6	—	—25	6
7	—	—23	6
8 or innermost		—22	
		220	

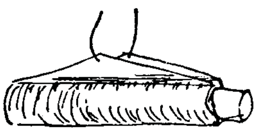
feet exclusive of projecting ends all separated by twine and calico. The internal diameter of paper cylinder was 13/16 of inch in diameter, the external diameter of whole 1½ inches and the length of copper helices (as a cylinder) 6½ inches.

57. Expts. with O. The 8 ends of the helices at one end of the cylinder were cleaned and fastened together as a bundle. So were the 8 other ends. These compound ends were then connected with the Galvanometer by long copper wires then a cylindrical bar magnet 3/4 inch in diameter and 8½ inches in length had one end just inserted into the end of the helix cylinder—then it was quickly thrust in the whole length and *the galvanometer* needle moved—then pulled out and again the *needle moved but* in the opposite direction. This effect was repeated every time the magnet was put in or out and therefore a wave of Electricity was so produced from *mere approximation of a magnet* and not from its formation in *situ*.

58. The needle did not remain deflected but *returned to its place* each time. The order of motions were inverse as in former expts. —the motions were in the direction consistent with former expts., i.e. the indicating needle tended to become parallel with the exciting magnet, being on the same side of the wire and poles of the same name in the same direction.

59. When the 8 helices were made one long helix the effect was not so strong on the galvanometer as before, probably not half so strong. So that it is *best* in pieces and combined at the end.

60. When only one of the 8 helices was used it was least powerfull —hardly sensible.



61. Made a sort of jacket of tin foil round a paper cylinder so that, being separated at the edges by paper, the galvanometer wires could be attached to. Then pushed magnet in and out but could perceive nothing at galvanometer. Could hardly indeed expect it, because as magnet introduced there was the part in advance ready to carry the current back. Now in coil, the part in advance could not do so. But jacket may be effectual with iron in its place made a magnet at once, either by contact of bars or by helix round it.

OCTR. 18, 1831.

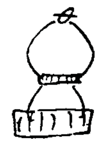
62. Again charged battery of 12 troughs, 10 pr. each 4 inches square.

63. Re-experimented with block and coils M (   ) connected as before with the Galvanometer. When battery was connected with one wire the other very feeble affected galvanometer. When contact was broken the galvanometer was affected the other way—the effect was very small, but it did not depend upon electricity of tension diffused from battery, as was evident from the direction of the disturbance.

64. Then concluding that there might be a powerful wave though too sudden to move Galvanometer needle and more like a common electric shock—use a hollow helix instead of the galvanometer, and put two or three unmagnetised needles into it; found they were made magnets instantly by contact of the other wires and magnets of the contrary kind when contacts broken. Hence mere wires do induce upon each other, but action very sudden and quick.

65. Interposed brine and two copper plates, of large surfaces, in the Galvanometer circuit to see if by retardation might render effect on needle more visible—was just as before, not more—hence *can pass* fluids though it does not decompose them.

66. Sent the current of the battery through a rod of copper, the rod being at the same time connected by two other wires with the galvanometer—to see if on the current ceasing there was any return in copper rod; but could find no effects. Perhaps ought to use helix and needle because of very quick action.



67. Same expt. with iron rod, obtained no action.

68. Now used the arrangement O, connected spirals 1, 3, 5 and 7 end to end to form one long helix for battery circuit, connected 2, 4, 6 and 8 with similar ends together (   ) for galvanometer connection. When all was arranged and the middle of O was empty or had wood only, then there was very slight momentary effect on galvanometer. But on using helix in place of galvanometer then needles were rendered magnets very sensibly and reversed at pleasure.

69. On repeating expt. with copper bolt in middle of O still effects as before.

70. On repeating expt. with iron bolt in middle of O then Galvanometer needle powerfully pulled aside even 70° or 80°, coming to rest if contact continued, and occurring in opposite direction when contact broken. Hence effect of iron very evident. On using helix and needles in place of galvanometer the magnets formed were stronger than if iron not used, but the difference was not so great as with galvanometer.

71. Arranged O so as to be able to make Sturgeon's vibrating experiment with it, either with or without iron axis; the plate being allowed to vibrate from one extent or arc of vibration until it had diminished to another, these being marked. Now when the helices O had no current through them the average vibrations of the pendulum and plate were about 19. When the current was passed through it was nearly the same. When the iron bar was in the middle, but no current, still nearly the same. When iron bar and current also the number was only about 15. It is true, however, that end of iron bar was nearer a little than end of helix, but still helix with current and no iron seems to be doubtful in effect.

72. The current was sent through helices 1, 3, 5 and 7 as one long helix. Must suspend plate by glass horizontally so as to be free from retardation at point of support.

73. Expt. with N. On sending Electric current—powerful magnetism, more so on needle than if iron not there—far more; but pole of iron and pole of helix same kind, say S. But there was no tendency of N pole down middle of iron cylinder; it always flew to the edges, hence quite unlike helix alone, as if therefore currents inside the cylinder were reverse direction to those outside.

74. On putting needles into the iron cylinder whilst an electro-magnet, if in contact with iron then ends assumed state of opposite poles, i.e. S gave N. If iron had been away that end of needle would have been south. On holding needle in axis of cylinder so as not to touch and then making contact there was no charge given to needle. Hence evident that Magnetic power of helix cannot penetrate this iron cylinder, but is absorbed as it were by it. Extraordinary that as there is this direct action there should not be more reaction of magnet on wires.

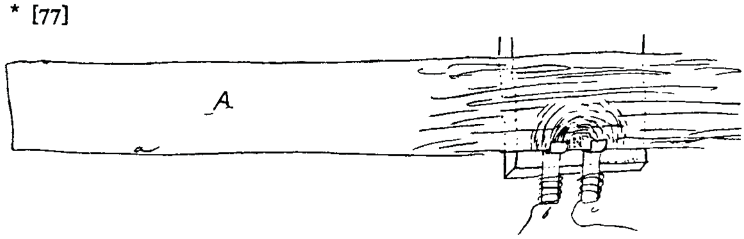
75. Expt. with O : arranged all the helices ends together as one (i.e. like ends together) and connected with a little hollow helix, then put a needle in latter and thrust cylinder magnet into former; withdrew latter and then former; introduced latter and then former, and so on, but could not so get evidence of any magnetism communicated to the needle.

OCT. 24, 1831.

76. Used cylinder O : all similar ends bound together and connected with Galvanometer—an iron cylinder of 12 inches long in O and contacts of magnets as in (33) made and broken—affected the Galvanometer very well.

77*. A large magnet was laid down on a table, the north pole as indicated by a needle being at N—it was covered with paper—a

* [77]



long slip of copper, perhaps 1/16 of inch thick, was laid across it, with the edge *a* amalgamated: little bitts of copper plate were bent as at *b, c* so as to form a sort of groove to receive the edge *a,* and these were amalgamated to make contact, then being placed as in the figure with mercury between the conductors and edge. The slip A was drawn quickly to and then fro, but no motion was observed at the galvanometer. The hopes were that the pole being at N a semi-vortex would be formed and electricity gathered up by the conductors at the section of the vortex occasioned at the edge of the plate. Still it may do with more delicate apparatus and more powerful magnets.

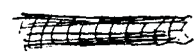
OCTR. 27TH, 1831.

78. Tried arrangement above with better adjusted conductors and with much more delicate small galvanometer—got no effects.

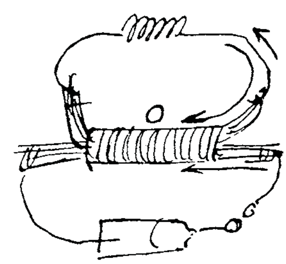
79. Used rotating wheel—conductors good thick copper both well amalgamated—put bar magnets, one pole over the opposite pole of other bar under, and connected other ends of bars. Hence bars were horizontal and along the plate but think it ought to do; yet got no effects.

80. Used a basin of mercury and touching or collecting conductors covered with paper except at edge as it were of mercury disc. Put one magnet above the other below, but got no effects when mercury made to revolve.

81. Thought there might be a kind of statical effect or pressure in the electric fluid, so that a narrow wire would only convey that produced from an area equal to its own, and therefore used thicker wire, but still obtained no effect.



82. Tried if common electric discharge could produce this kind induction. Used O. Coils 1, 3, 5 and 8 [?7] were connected at end as bundles and connected with outside and inside of Leyden jar. Coils 2, 4, 6 and 8 were similarly connected and attached to a hollow helix in which a needle was placed. A needle in the trial helix became magnetic, North pole being to the left hand. A needle in helix O also became magnetic, but with less force and more labour, but N pole also to left hand. Hence it was proved that the magnetization in trial helix was not effect of induction, for induction would have caused a current in the direction of the

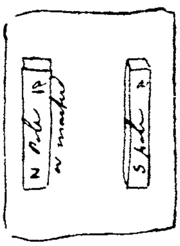


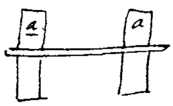
inner arrow, whereas the effect was that of a current in direction of outer arrow. In fact a little of the positive electricity had passed through the further as well as near helix and produced similar effects in both.

83. Tried sparks, but had the same effect—no sensible induction.

84. Prepared to go to Mr Christie's to-morrow.

OCT. 28, 1831.





85. At Mr Christie's to-day making many expts. with the great magnet of the Royal Society. <u>*a*</u>, *a* are the ends of the projecting poles of the magnets: each is 12? inches long and 3? inches wide and they are about 10 inches apart. A soft iron cylinder 3/4 inch in diameter and 13 inches long was put through O—all the ends of O helices made into 2 bundles and these connected by long copper wires, bell wire, with the last galvanometer that I made in a jar. This Galvanometer was about 10 feet from the magnet and in about the position marked in the sketch*. In altitude it was rather above the middle of the poles.

86. By connecting the two poles (magnetic) by the soft iron cylinder, when connection between the galvanometer and wires was not made, the galvanometer was very slightly affected, so little as to be barely sensible. But when wires *were* connected, then on making or breaking the magnetic contact with the iron cylinder—a powerful pull *whirling the Galvanometer needle* round many times was given.

87. As the helix or cylinder were moved to or from the magnet, *not touching*, corresponding effects were exhibited by the galvanometer.

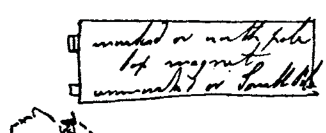
88. When the contact was continued—no permanent effect on the needle was produced. Even with this powerful magnet the effects were only for a moment as in former cases at home.

89. As the evidence of a strong current for the moment was visible we tried other actions. Easily made magnetic needles when a helix (hollow) was substituted for the galvanometer.

90. On bringing ends of short wires from the ends of the helices O to the tongue and gums could get no taste or flash.

91. Could get no spark by charcoal and wire—could not heat platina wire of the fineness I possessed, but I had none of Wollaston's Silver platina.

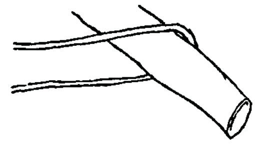
* [85]  



92. Could get no chemical action on Sul. copper though took care to repeat contact at proper times, etc. etc.

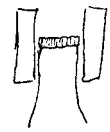
93. Re-arranged Galvanometer—tried a jacket like that of (61) but of thick copper plate—put with paper on the soft iron cylinder. Obtained a good effect at the Galvanometer. Put on three jackets and connected at projecting parts—somewhat more of effect but contact not good with wires—too hasty.

94. Put paper on iron cylinder and took 6 turns of copper wire on it. Very powerful effect at Magnetic contacts and disjunction.



95. Took only a half round of wire on iron cylinder—still excellent effect on needle.

96. Brought helix O up suddenly between the large poles of the magnet; it having no iron bar in its axis. The needle was strongly affected; and also upon its removal as in former cases. This of course a mere effect of approximation and that not very near—not subject to any objection founded on notion of the iron exerting a momentary peculiar action at time of becoming a magnet—is directly connected with Arago's expt.

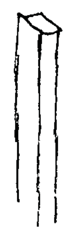


97. A copper bolt 3/4 inch in diameter put through helix O: not more action than without.

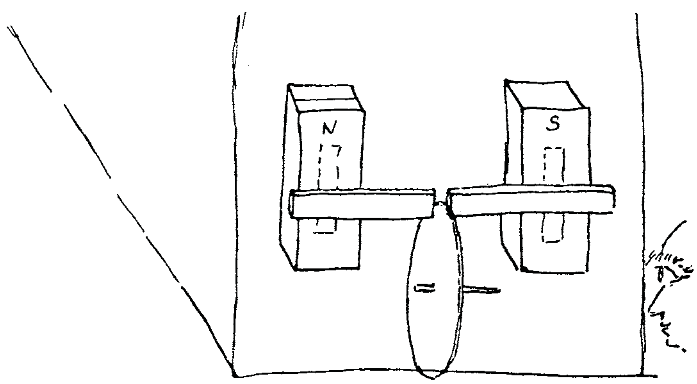
98. A thick iron wire put through helped to increase the action.

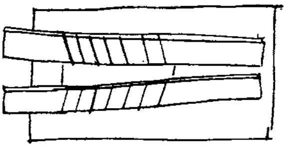
99*. Made many expts. with a copper revolving plate, about 12 inches in diameter and about 1/5 of inch thick, mounted on a brass axle. To concentrate the polar action two small magnets 6 or 7 inches long, about 1 inch wide and half an inch thick were put against the front of the large poles, transverse to them and with their flat sides against them, and; he ends pushed forward until sufficiently near; the bars were prevented from slipping down by jars and shakes by means of string tied round them.

100. The edge of the plate was inserted more or less between the two concentrated poles thus formed. It was also well amalgamated, and then contact was made with this edge in different places by conductors formed from equally thick copper plate and with the extreme end edges grooved and amalgamated so as to fit on to and have contact with the edges of the plate. Two of these were attached to a piece of card board by thread at such

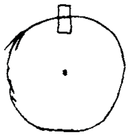


* [99]





distances that they might come in contact with two near parts of the edge of the plate at once; these conductors being connected by wires with the Galvanometer.



101. The circular plate was in all the expts. nearly in the plane of the magnetic meridian, the Galvanometer in the same place as before. In the following notes and diagrams the plate will be represented by a circle looked at from the west or from the eye () in the above sketch; the direction of its motion will be represented by arrow heads. The place of the poles (magnetic) by a red area thus [1], and the place of the conductors by black sketches thus .

102*. The Galvanometer had two needles put north and south, one between the helix the other above, but the upper one gave by its stronger power direction to the whole, and the observations were always made upon its south or unmarked pole (that pointing south).

103†. When conductors were on edge of plate equidistant from place of poles, and plate raised up till edge level with middle of each pole and then turned as in the figure (from right to left above), then galvanometer gave indications and the S pole of the upper needle passed towards the *East*. But when plate turned the other way, all other things being the same, the S pole went *West*.

104. The direction of the Galvanometer wires from the conductor is evident above[2]. The effects were very distinct and constant.

105‡. Next held the conductors against the edge as if they were fixed to it, and moved them with the circle by the Magnetic poles. When the motion was in the same direction as in the last expt., the S pole went west as before.

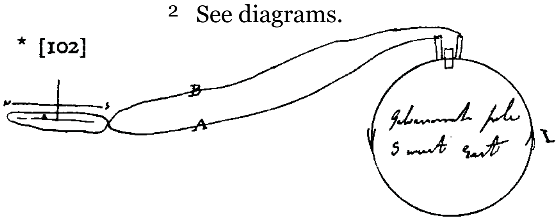
106§. But when the motion was reversed and therefore in the same direction as in the first expt. the S pole went east, i.e. the contrary of the last and the same as the first.

107. All these effects were constant in direction but difficult to obtain regularly, because of the difficulty of holding both con-

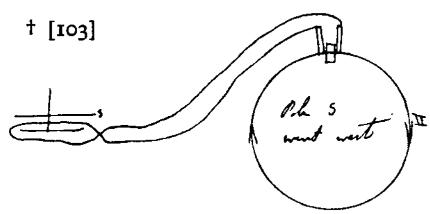
[1] The small rectangles here referred to, which were drawn in red ink in the manuscript, will be seen in diagrams to pars. 101 to 167 inclusive.

[2] See diagrams.

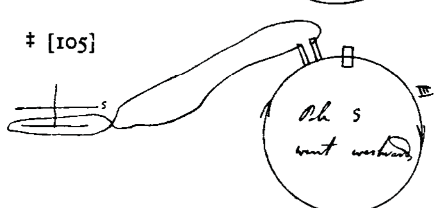
* [102]



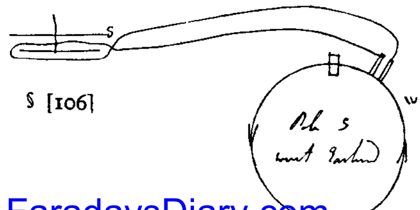
† [103]



‡ [105]



§ [106]



ductors in contact at once—both for that the edge of the plate was not perfectly regular and also that the stand was not steady.

108*. Then suspecting terrestrial action, especially as plate was revolving nearly in Magnetic meridian, the latter was raised until the magnetic poles were in the plane of the magnetic equator of the revolving plate, but the effect was the same, i.e. the south pole went west when the plate moved from left to right above. By depressing the place of magnetic poles still lower still the same deflection was produced when the plate revolved the same way.

109. Hence the earth's effect either null or too small to confuse the experiment.

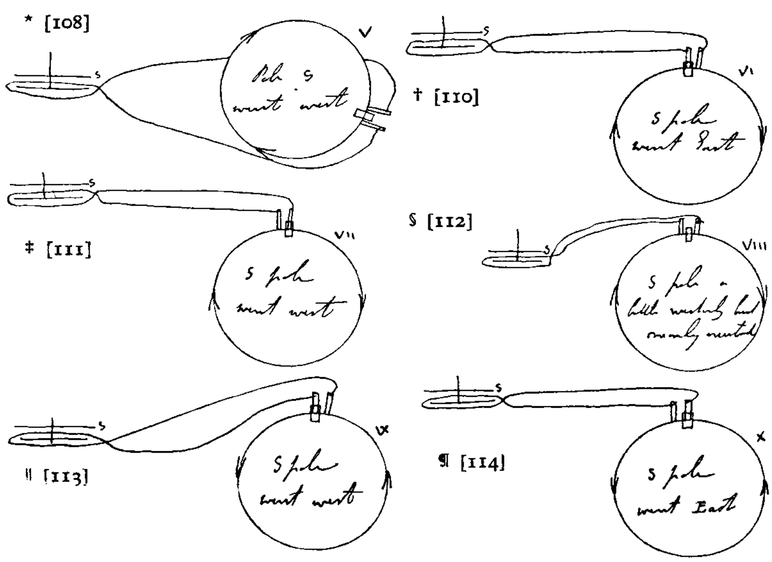
110†. Then endeavoured to examine effect more minutely. Put conductors as in the figure, i.e. the left hand one between the poles and the other at one side on the right; then revolving the plate as the arrows point out the S pole went *east.*

111‡. But on making the motion and every thing else the same, except that the right hand conductor was between the pole and the left a little to the left of it (which did not require more than an inch of displacement), now the S pole went *west,* i.e. the reverse of the last expt.

112§. Hence it was evident there ought to be a nearly neutral position, and on examining the position of equidistance more carefully it was found that the power though westerly was very weak and depended more upon irregularity of contact than constant action. This had every appearance of a neutral position.

113‖. Now the experiment was repeated except that the motion of the plate was reversed. The conductors being placed as in vi, south pole went west instead of east.

114¶. On placing conductors as in vii the south pole went east instead of west.



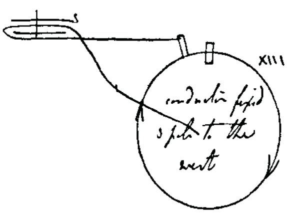
115. Hence changing the direction of the motion of the plate changes the current, and also changing the conductors to the right or left reverses the current.

116. Suspected from all this that a single conductor would do more than two, and that the condensed imaginary bisected vortex was not so definite as supposed, if existing at all. The wire coming from the upper coils of the Galvanometer was therefore fastened round the brass axis of the plate as the most neutral part of it, and the conductor from the other wire end applied to the edge of the wheel.

117*. When the wheel was revolved from left to right above and the conductor placed between the magnets the S pole of galvanometer went powerfully *west,* and when the conductor was placed either right or left of that still the S pole went west, but with less force. Even when a good way removed the S pole went westward.

118†. On the contrary, when the wheel revolved in the opposite direction the south pole went *east* most strongly when the conductor was directly at the magnets—less strongly when removed right or left as in the faint figures.

119. Hence it is easy to see how two conductors acted. They only shewed the difference of intensity of the two currents setting into them, and consequently by changing the conductors with relation to the strongest part of the plate the direction of the current in the wires and galvanometer was changed and therefore the direction of the needle. The neutral position was that in which each conductor tended to receive a current of equal intensity and in the same direction, consequently there was no circulation.

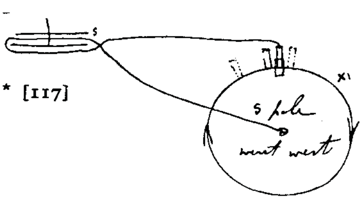


120. When the conductor was fixed on the plate, and with the plate itself moved forward from left to right above, the S pole went west first as if the plate had moved without the conductor.

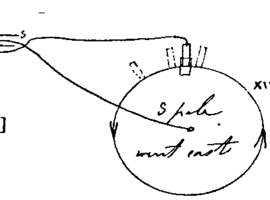
121‡. When the plate and conductor moved together by the Magnetic poles in the opposite direction the S pole went east—again as if plate had moved without the conductor.

122§. When the two conductors were placed as in the neutral position viii, but connected as one as in figure, then revolving

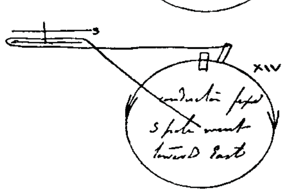
* [117]



† [118]



‡ [121]



§ [122]

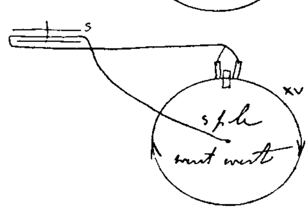


plate made S pole of needle go *powerfully* west. On reversing motion of the plate it went as powerfully east.

123*. Now raised the plate so that the projection of the Magnetic pole should be quite within the edge of the former.

124†. But although supposed vortex thus nearly included, effects were as before, i.e. a current of the same kind set off from all the parts of the edge of the plate near the pole whilst the rotation one way. But on reversing the rotation the deflection of the needle at the Galvanometer was reversed.

125. As now the edge could be seen and the contact better preserved, it was found that whilst the plate continued to revolve the Galvanometer needle was permanently deflected.

126‡. On raising the plate so that the magnetic poles were much nearer the center still effect was the same and the deflection was very sensible. It was reversed with the reversal of the rotation of the plate. Hence if any vortex it must be very diffuse.

127. To ascertain effect of vicinity to one pole or the other, one pole was quite taken away from plate and only the other left. The effects were exactly of the same kind, though not so strong as the former.

128. None of all these effects took place when the plate was made to revolve away from the magnet. They occurred very feeble when the plate was placed between the large magnetic poles, each being then several inches from it.

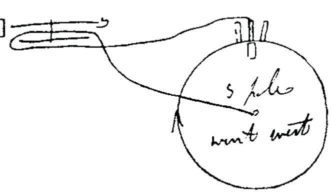
129. No electric effect from friction, mercury, etc. etc. could have therefore led to mistakes.

NOVR. 4TH, 1831.

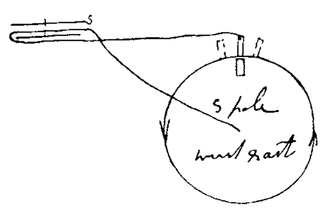
130. Again at Mr Christie's—making expts. He has had two iron bent bars made, so as to apply to sides or ends of large poles and thus concentrate them into two smaller poles, one over the other (the axis of these poles being perpendicular), and so that either north or south pole could be made uppermost. These bars were about 3/4 of inch square and consequently the face of each pole presented that area.

131. He had also fitted up the large copper plate on a table with

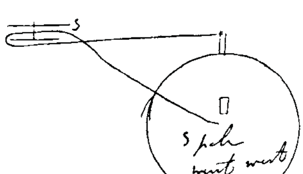
* [123]

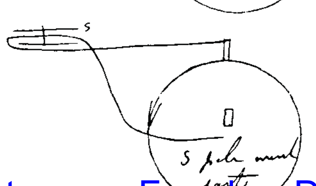


† [124]

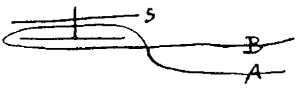


‡ [126]





a horizontal motion, so that it could be brought up between the poles and revolved; there.



132. The character and position of the poles and wires at the last day were first verified. The large magnet stands nearly N and S, its poles towards the north and the marked or north pole to the East, the other to the west. The Galvanometer was to the North of the magnet and its wires as before.

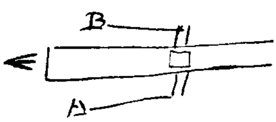
133*. The plate was now made to rotate horizontally, moving as the arrow heads point. The galvanometer wire A went to the conductor at the edge by the poles and the wire B to the center of rotation. The S end of needle went W.

134. On reversing the motion of the plate the S end went E.

135. On putting wire B on plate a little way from the axis effect produced agreeing with the motion.

136. A place could be found between the edge of the plate and the centre in which the electricity was not evolved, i.e. if both being placed there, one was then moved towards the rim it would receive one electricity, if towards the axis another. This ought to be in fact the quality of the line passing under the center of the magnet, and therefore extend right and left from the magnet on the plate and parallel to the circumference.

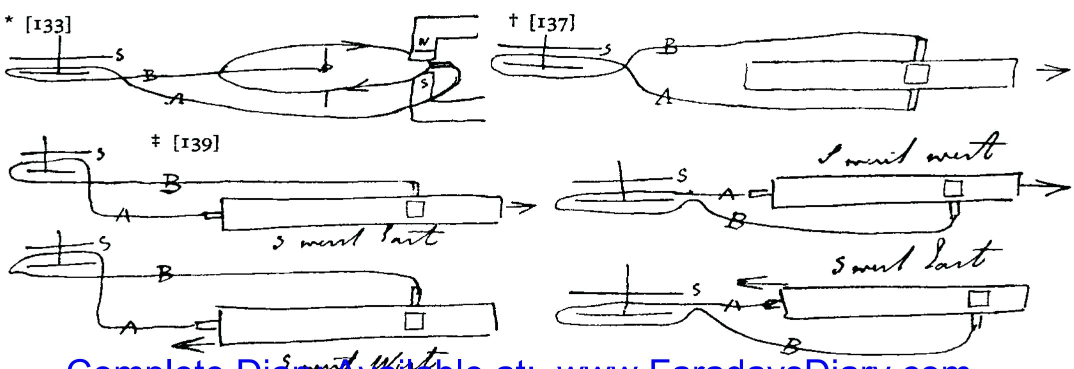
137†. Then experimented with plates, as being simpler; in the figures it is supposed that you are looking down upon the plate, the north or marked pole above, the S pole below. A plate 1½ inch wide, 12 inches long and 1/5 thick, of copper, was passed between the poles in the direction of the arrow; the conductors of lead being at the place of the poles on each side as figured. The S pole went East.



138. On reversing the motion the S pole went west. These effects very distinct and good.

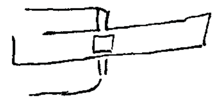
139‡. Then to shew opposite character of Electricity on the two sides, the wire A was made fast to the end of the plate and the wire B touched conductor on one or other side of the plate at place of pole. The following were the results[1], North or marked pole of Magnet being above.

[1] The diagrams are referred to here.



140*. Now put conductors at end of the same bar and carried it through between the poles transverse to their axis; and at about the middle the motion of the needle was as in the figures.

141. The effect was as strong as if the conductors had been held against the sides of the bar when passed through, thus



142†. When the bar with the conductors at the ends was passed lengthways between the mag. poles there was no effect either way. No effect on Galvanometer.

143. When narrower plates of copper were used the effects were produced as before, but there was no great or even sensible difference in the intensity of effect.

144. When a mere copper wire, amalgamated, was drawn through between the conductors and poles the effect was produced, but not so strong as with the plates. Perhaps because it was thinner, perhaps because amalgamating mercury might wrap over it and conduct and perhaps also because of its being narrower. I think wider plates are better.

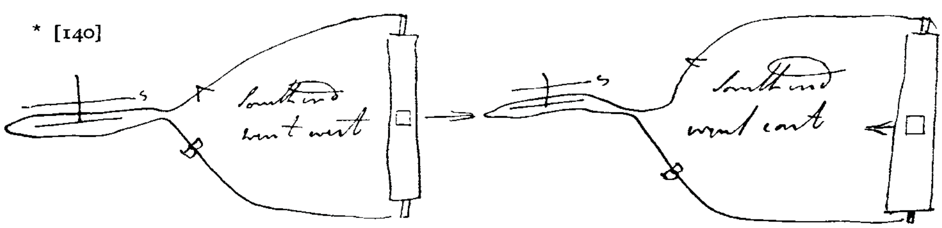
145. When an *iron plate* of 12 inches by 1½ inches and 1/5 was used, it being turned at the edges and amalgamated, it gave effects the same in direction and kind as copper but stronger, but then the Magnetic poles were attracted to it and were nearer, for there was only a slip of card board between the soft iron bar and the poles. On the whole do not think it was stronger than copper at the same distance.

146‡. When copper plate was quite out of polar axis of magnets and moved as in figure, still effects, which though not so strong as when plate just between poles were of the same character, for S pole went East.

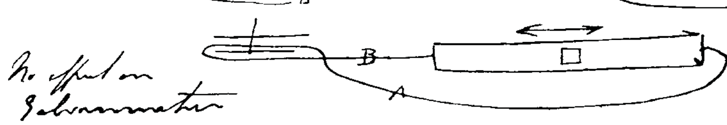
147. In this experiment, as in those of the last two pages[1], the North or marked pole of large magnet was above, i.e. between the sketches and the eye of the observer.

[1] Pars. 137 to 147.

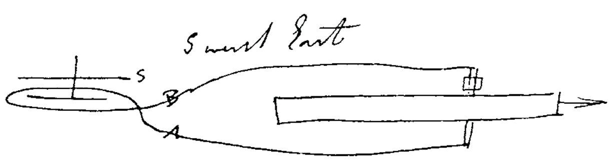
* [140]



† [142]



‡ [146]

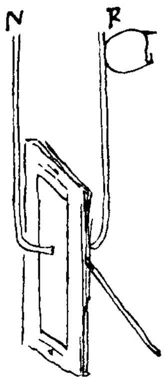


404. Harris' expts. on interposition of copper, silver, etc. between moving magnet and iron plate prove absorption of power of magnet by the interposed metals and consequently a *reaction*.

405. Will these metals *obstruct* equally the force of a magnet at *rest?*

406. Will flame intervening obstruct power of moving magnets but not of magnets at rest?

407. Will white hot iron allow magnetic forces freely to pervade it, i.e. when not magnetic is it no obstacle?



APRIL 6TH, 1832.

408. Had two brass plates, each 3 inches by 6 and flat, each with a wire P and N fastened to it. Obtained two glass plates 6 inches by 9 inches, thin and flat; held all together as in the figure, when they formed a Leyden arrangement of some force. Opened the edges of. the glass plates a little, still the Leyden arrangement was strong.

409. Now immersed the whole in Oil (lamp), connected P with the prime conductor of machine and N with the ground. Took a small needle free from magnetism and by a whalebone slip drew it up from *a* to *b*, then discharged the arrangement, depressed the needle, recharged the arrangement and *raised* the needle. Repeated this again and again many times, endeavouring to cut the electric lines of tendency or curves by the needle and make the latter a magnet; but could get no traces of such an effect.

410. The Leyden power of the arrangement *in the oil* was much weaker than in the air. Cleaned every thing therefore and repeated the experiment in the air, but could get no magnetic effects.

APRIL 7TH.

411. Repeated the experiments in air but could get no magnetic effects.

APRIL 17TH.

412. Have experimented upon screening power of copper when at rest or in motion. In latter case ought to screen. Used the two bar magnets, put similar poles together—they were arranged horizontally and in magnetic meridian, the marked poles southwards. Used a double needle (belonging to the galvanometer) vibrating slowly, i.e. to and fro in about 20″. It was put magnetically south of the bar magnets, at the same elevation and about 3 feet off, and was deflected about 70° from its natural position, the stronger marked pole which gave direction to the whole going westward.

413. Now arranged the copper round plate on whirling table in a vertical plane perpendicular to Mag. meridian and close to the marked magnetic poles of bars, so as to form a screen between them and the distant needle.

414. When the plate was interposed and then removed isochronously with the time which would be required for the vibration of the needle, still not the slightest effect was observed; whether there or not the magnetic influence between the bars and the needle was equally strong.

415. But when the plate instead of being removed was made to revolve then the magnetic forces *were affected.* Upon revolving the plate 10 seconds and then leaving it still 10 seconds and so on several times, the vibrations of the needle were varied to several degrees.

416. When the plate revolved the needle tended to return to the position it would take if no bar magnets had been used, i.e. the power of the latter over it was diminished.

417. Whichever way the plate revolved the effect was the same, i.e. as if bars had been removed a little further off.

418. Hence moving metals screen magnetic power off when still metals do not, i.e. the currents of electricity generated by the vicinity of the mag. pole react upon it and neutralize part of its magnetism. As it ought to do and as was expected.

419. Hence Harris' expts. in screening influence involve a fallacy, for his magnet was moving and currents were formed.

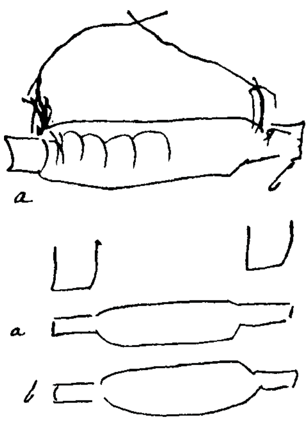
420*. At first the apparatus was arranged with the parts at different altitudes, but then the needle was affected by the *current* produced, and went this way or that as the motion was one way or the other. In the arrangement already described the magnet was so place[d] that the principal current was horizontal and therefore tended to raise or depress the needle but not to make it approach to or receed from the plate; and then it was found that revolution *either* way always made the needle approach the magnets and *rest* made it receed.

* [420] 

421. The bar magnets and needle being as first arranged (412, etc.), a bat's wing gas burner was so arranged as the flame should occupy the place of the copper plate, acting as a screen and cutting the magnetic curves proceeding to the needle. It was then turned on and off alternately at intervals of 10 seconds so as to indicate any effect it might have over the curves. It being supposed possible that the heat might do something, but no results were obtained, i.e. the magnetic forces did not appear to be at all diminished when intersected by the flame.

422. It is very necessary in these expts. to take care and avoid the motion of portions of iron about the burners or vessels during the expts. Effects were at first obtained which after a while were found referable to these causes.

1832. FEB. 8TH.



423. This Evening at Woolwich experimented with Magnet and for first time got the Mag. spark myself. Connected ends of a helix into two general ends and then crossed the wires in such a way that a blow at *a b* would open them a little. Then bringing *a b* against the poles of the magnet the ends were disjoined and bright sparks resulted.

424. They were produced as the lifter went up and also as it was pulled away.

425. In pulling away, 3 or 4 sparks in succession could be observed, i.e. as lifter went from poles to *a* a spark, and also as it went from *a* to *b* a spark, i.e. without even touching the magnet.

Proposed using hydrogen but had not any at hand to inflame.

FEB. 9TH.

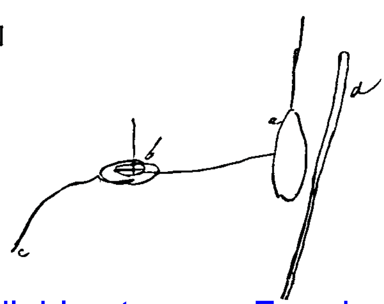
426. At home succeeded beautifully with Mr Daniell's Magnet. Amalgamation of wires very needful.

427. This is a natural loadstone and perhaps the first used for the spark.

FEB. 16TH.

428*. *a* a brass plate hung by silk, connected by wire with galvanometer *b* and that with wire *c* passing off to the earth. An excited Glass rod *d* brought towards and from *a* alternately—there

* [428]

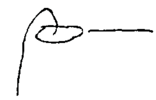


should be current through wire of *b* but could not get any effect. Try however expt. in better and larger way.

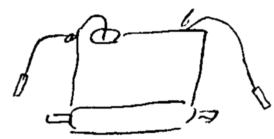
1832. JUNE 11TH

429. Have been experimenting on chemical power of magneto-electric current, etc. etc.

430. On tinning the surface of the copper plate and the end of the wire resting on it, obtained (after amalgamation) a much better spark than before. Very good, but amalgam of tin may perhaps wear off.



431. On making two copper wires attached to platina spatulas fast at *a* and *b* so that the tongue could be put between the spatulas and yet the contact of the end on the plate allowed to be broken as in former cases, a very powerful effect on the tongue was felt each time the contact of the lifter with Daniell's magnet was made.



432. On putting one of the spatulas up the gums the flash was very strong.

433. Then mixed a little solution of starch with solution of hydriodate of potassa—dipped bibulous paper in it and put it between the two spatulas so that they did not touch, but the current passed the solution. As the lifter with its helix was raised from the Mag. poles, allowed the plate and point to open, that the current might then be diverted and pass through the solution. As the lifter was put down on the poles, kept the plate and point close so that no current should pass solution. All that did pass wire therefore one way. Repeated the action many times so as to accumulate chemical effect. The spark was evidently smaller at the plate and point shewing that electricity went through the solution, but not the slightest appearance of decomposition occurred—no blueing of the starch.

434. Now put up the revolving plate between the poles of Daniell's magnet and could obtain with it a permanent deflection of the magnetic needle of 45° or 50° or more.

435. Then attached to the ends of the wires used a piece of platina and a piece of zinc wire; put the end of the platina wire into a glass of very dilute acid and then immersed the zinc wire so far as to give about 40° of deflection to the magnetic needle; for this about 1½ inches of the zinc wire were required to be immersed. This therefore formed a voltaic comparative arrangement.

436. Interposed elsewhere in the circuit the two platina spatulas separated by paper moistened with the starchy hydriodate of potassa. Using the standard voltaic arrangement just mentioned the needle was now very feebly deflected, instead of 40° or more now hardly 10°; but after a little continuance, on examining the moistened paper decomposition had proceeded and the starch had been rendered blue. It required more than a momentary duration for this purpose. After a few seconds no change was visible. In half a minute it was sensible, in a minute or two very strong.

437. Hence the original current, though much impeded, could pass the interposed solution, and if time were allowed sensibly decompose it.

438. Now using the current from the wheel and magnet, when this was interrupted by the platina spatulas and interposed moistened paper, etc., *No* deflection of the needle could be observed, nor after several minutes turning could any signs of decomposition be discovered. This current therefore, which when all in metal could deflect the needle more than the other, had no *sensible* power under these circumstances of passing fluid conductors. No doubt it would pass if exalted enough and then most probably decompose. Hence the hydro current more intense.

439. The little voltaic arrangement (435) did not affect the tongue so strongly as the first arrangement with the magnet (431); but then it was constant.

440. So that the effect of the Magnetic helix was strong enough but not long enough, and the effect of the wheel was long enough but not strong enough.

441. Then by means of little voltaic arrangement (435) tried the delicacy of different tests of voltaic action. Solutions of Acetate of copper, Mur. Gold, Acetate of Lead and sub acetate of lead, put in drops on one platina spatula and touched by the other so that the current of the Voltaic arrangement passed through them and feebly affected the needle, still did not give sensible signs of decomposition in a few minutes. But the mixed solution of starch and hydriodate of potassa did give very abundant proofs in same time. Hence the latter a very delicate test. The best of these.

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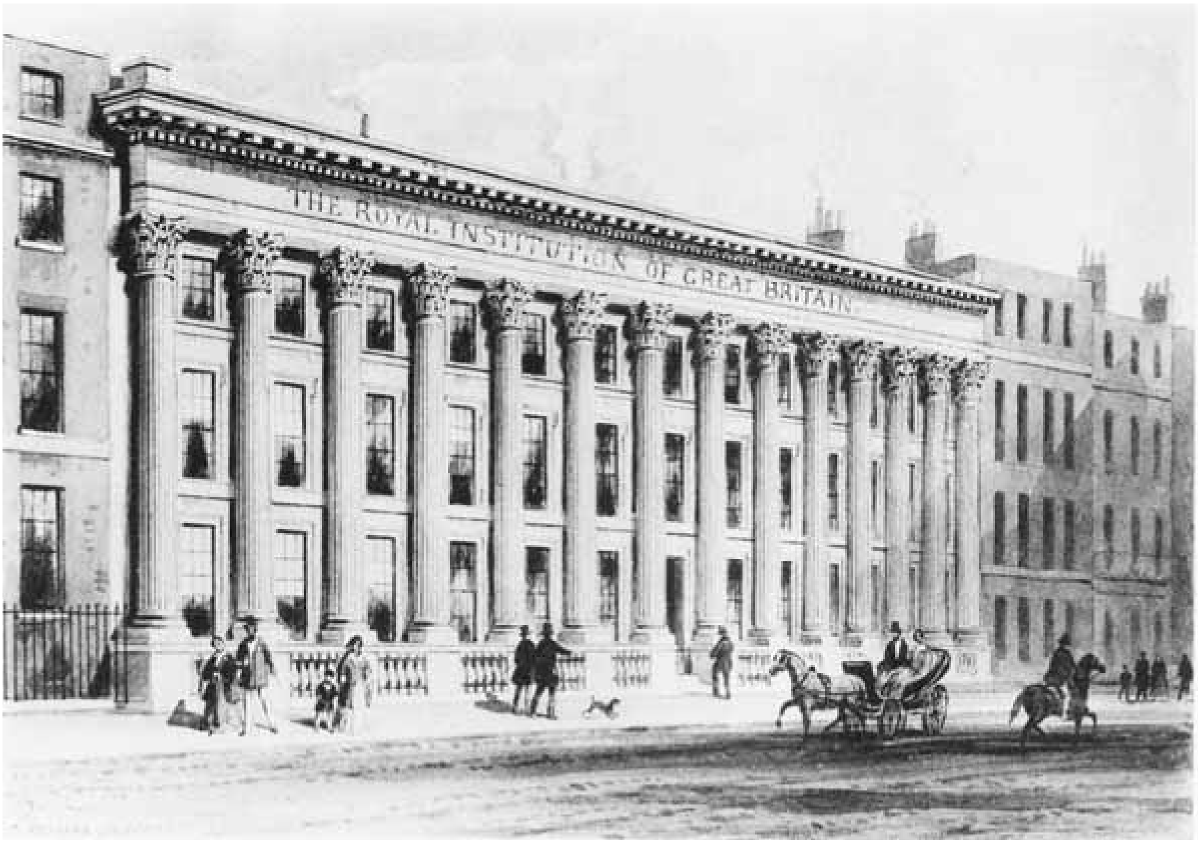
FARADAY'S DIARY

VOL. II



THE ROYAL INSTITUTION OF GREAT BRITAIN

From a water colour by T. Hosmer Shephard, painted about 1840

FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

made by

MICHAEL FARADAY
D.C.L., F.R.S.

during the years 1820 – 1862
and bequeathed by him to the
ROYAL INSTITUTION OF GREAT BRITAIN
Now, by order of the Managers,
printed and published for the first time,
under the editorial supervision of
THOMAS MARTIN, M.Sc.

with a Foreword by
SIR WILLIAM H. BRAGG, O.M., K.B.E., F.R.S.
Director of the Laboratory of the
Royal Institution

VOL. II

SECOND EDITION

AUG. 25, 1832 – FEB. 29, 1836

HR DIRECT

2008

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PREFACE TO VOLUME II

THE part of the Diary printed in this volume covers the period August 25, 1832, to February 29, 1836, and is contained in Folio Vol. II and part of Folio Vol. III of the manuscript. There is no break at the end of Folio Vol. II, which finishes, indeed, in the middle of a sentence, during the course of a description of electrostatic experiments on January 15, 1836. The entry for the day is concluded in the first few pages of Folio Vol. III.

The text has presented no new problems. Only a few isolated words have been found illegible, and punctuation, abbreviations and other peculiarities have been treated on the same general lines as in the first volume. Most of the illustrations have been reduced in size, generally to three-quarters of their original dimensions, and placed in the margin as far as possible, against the paragraphs to which they relate. Those which it has been necessary to place at the foot of the page are referred to by signs at the beginnings of the paragraphs, except in a few cases, where the context requires the reference sign at a particular point in the text.

An abbreviation which makes its appearance for the first time in this volume is "sulc." for sulphuric. In the manuscript there is a tendency, about half-way through, to drop the colons from "c:i:", for cubic inches, and to use full points instead, but in print the colons have been retained throughout. "Anion" and "cation" are sometimes spelt with the "i" modified, e.g. "anïon" and "catïon" at par. 2765.

T. M.

ROYAL INSTITUTION
September 1932

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CONTENTS

In this volume the unbroken series of paragraph numbers begins which runs nearly to the end of the Diary. In the Contents the paragraph numbers (in black type) are given as well as the dates.

Cross references to published papers are included as before. The part of the manuscript here printed contains the notes and records of experiments drawn upon by Faraday in writing Series III to X and parts of Series XI and XII of the electrical researches, as well as certain other shorter papers. All of these will be found in Vols. I and II of the collected *Experimental Researches in Electricity,* to which the page references have been given.

In some cases it has been difficult to decide in which of the published papers a particular entry in the Diary has been used, or if it has been used at all. No cross references have been given in cases where the connection is indefinite or doubtful. As in Vol. I, there is a considerable number of entries of which no published result can be traced.

No exact correspondence between the passages in the Diary and in the printed works can be expected. The cross references should be used as a general guide to the papers in which may be found, more fully developed, ideas of which the Diary contains the first suggestions and the day-to-day record of the relevant experiments.

In the first reference to each of the Series Faraday's own sub-headings have been given in full, since it may be of assistance to the reader to have included in the Contents these descriptions, in his own phraseology, of the succeeding aspects of his work.

FOLIO VOLUME II OF MANUSCRIPT

FOLIO VOLUME III OF MANUSCRIPT

PLATES

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FOLIO VOLUME II
OF MANUSCRIPT

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1. Experimented with large E. Machine for deflection of galvanometer by its current. Plate of Machine ____ feet in diameter.

2. Have arranged a good discharging communication with pipes, earth, etc. etc., so that when it is connected with delicate Electrometer and the latter with the machine, the most powerful working of the latter will not affect the Electrometer (Singer's Gold leaf).

3. Connected one end of Galvanometer wire with this communication and the other end with a wire and fine point. Charged large battery of 15 Jars and brought point suddenly and slowly up to it so as to cause continuous discharge—but could not affect the Galvanometer.

4. Occasionally needle moved a little but found that motions were always due to electricity excited upon the glass of the Electrometer jar. When machine is worked, a point in the room at considerable distance will cause motion of needle by rendering the glass electric. Or if wire of galvanometer is not perfectly connected with discharging medium, same effect. I think also that silk about it may occasionally become electric.

5. It is necessary to avoid passing a shock through galvanometer for otherwise the magnetism of the needles is disturbed or altered and the instrument deranged.

6. Colladon on deviation of Mag. needle by common Electricity of Machine or storms. Ann. de Chimie, 1826, xxxiii, p. 62.

AUG. 27TH.

7. Have coated galvanometer jar with tin foil inside and outside to a certain height, connected these with the discharging train (2) and then hung a strip of metal leaf within the jar by a wire through a cork, the wire being also connected with the discharging train. Now connected a point (needle) with the large machine by a wire —use an insulating handle—and brought the point towards the jar, the machine being at the same time at work. So long as the point was about the coated part of the jar there was no attraction or motion of the metal leaf within, except when very low, and that was by a part of the wind issuing from point being introduced beneath the edge and acting as wind only. But when point near upper uncoated part of jar and the leaf raised, then strong attraction of it from electricity communicated to glass.

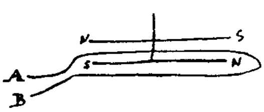
8. Made a frame work of wire and soldered needles on to it; this rested on uncoated part, the points projecting outwards. When machine point brought to within a foot of these, there was *no effect* on the leaf within; if brought quite within and between points of frame, then the glass could be charged a little and indicating leaf within moved. But so strong a case could never happen in expts.

9. Hence have now a galvanometer which will not be affected by common electrical attraction and repulsion.

10. When galvanometer coil is connected with the discharging system, know that it will not shew any common attraction and repulsion by electricity of machine (2).

11. Now exptd. by passing electricity of battery through water. A thick thread about four feet long was thoroughly wetted and then one end attached to the A end of the galvanometer, containing 36 feet of wire, and the other end to a discharger. The end B of galvanometer wire was connected with the discharging system (2). The battery of 15 Jars was then connected, its outside with the discharging train, which is the same thing as being connected with the end B of the galvanometer, and its inside with the Electrical machine. A Henly's Electrometer was put upon the general conductor and the machine worked and the battery charged until the electrometer stood at about 40° or 35° of inclination with the stem. Then the discharger was brought in contact with the conductor and so the discharge made through water in the thread and through the galvanometer in the direction from A to B.

12. On these occasions the needle was deflected, and by continuing the machine at work and discharging the charge in the battery each time the needle in swinging returned in the direction of the first impulse, the deflection was soon raised to 40° or more.



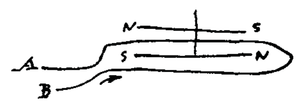
13. This effect was repeated again and again successfully and the deflection of the needle was always in the same direction. The upper needle of the galvanometer was the strongest and the arrangement as in the figure, N and S meaning north and South end when in natural position. On making contact, i.e. sending current from A to B, the upper S end went towards the east. This is precisely the kind of deflection that a voltaic current would cause.

14. Then the battery was separated from the machine and the effect of the machine alone on the needle was observed, i.e. the machine was worked and the discharger held against it whilst the needle swung in one direction; then the discharger was removed as the needle swung back and the machine meantime connected with the discharging train and also stopped, and then as needle swung in first direction the machine was reworked and the electricity from it sent through the wet thread, galvanometer coil, etc. as before. This was repeated several times and the deflection occurred and was increased each time until it was 30° or 40° or more.

15. The current on these occasions merely passed through the water and galvanometer coil away to the earth and not back to machine.

16. The direction of the deflection was the same as before and as every thing had remained in the same position gave consistent result. Hence mere machine current can thus deflect the needle.

17. Then changed the wires from the machine and the discharging train the one for the other at the galvanometer, so that the current now went from B to A.



18. On using the machine current only as it passed from B to A, it deflected the needle, causing the upper S end to pass now to the west instead of the east, as ought to be the case if true electro-magnetic effect were the cause.

19. Retaining the communications as last described, the battery was now put in use as before, so that its discharge also went from B to A, and the deflection occurred powerfully but contrary to what happened at first with the battery, but consistent with the changed state of the communications. Upper S end of needle went westward.

20. Now used a thick wet string instead of the thread and about the same length; effect was equally good and about the same.

21. Shortened the string, but with equal charge of the battery; the deflection still about the same in extent.

22. Used four short thicknesses of string instead of one long one; effect not apparently different.

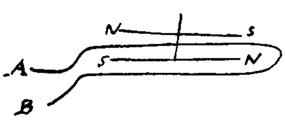
23. When a thread is used the battery charge sinks gradually, requiring perhaps 2 or 3 seconds to go down. When a short thick string is used it passed at once as a spark. But the effect on the needle is apparently the same. When it passes thus as a spark, wet string being in the circuit, the sound is very little and the spark had more of the appearance of flame than the ordinary spark, as if it lasted longer and was redder. It reminded me of the appearance of the discharge sometimes of a voltaic trough from points of charcoal.

24. On using an ordinary sized wire as connector between the galvanometer and the conductor of the machine and (throwing the battery out of gear) sending the current from the machine through it as had been done before through the water, I could [detect] no distinct effects upon the needle, but I must repeat the effect.

25. I rather doubt Colladon's actions of points, for I do not see why, if the galvanometer is sensible enough to shew effects with water, it should not shew effect if any of points, and yet I could not get such effects; but I must repeat the experiments.

26. I tried to make a discharger of fine needle points that could discharge the battery without a spark and yet suddenly, but I could not. A single point, unless approached very slowly and carefully, could not without a sharp spark. Nor could four points attached to wires about 3 inches apart but connected together at ends do it. A point requires much time to discharge from large surface and easily takes a spark from low intensity of battery charge.

AUG. 30TH.



27. Repeated the water expt. (18) with machine current only (i.e. not using the battery) and sending the current through the galvanometer from A to B. Effects were as before, and as current passed, the upper S end went eastward as on former occasions.

28. Now removed the string and used wire throughout, i.e. from discharger through the galvanometer away to the discharging train. Worked the machine and made and broke contact with the prime conductor alternately every 18 watch beats (that being the time for each swing of the galvanometer needle to the right or to the left). In this way accumulated effects and soon obtained deflection of needles. Hence water is not necessary for wire alone does it. As current passed from A to B, the upper S end of needle went eastward.

29. Then repeated the water experiment, reversing the direction of the current, and now the upper S end went westward instead of eastward.

30. Then repeated Battery experiment (11, 19, etc.) and obtained an excellent swing of the needle with one discharge. The direction was the same as before, i.e. current from A to B and S end of upper needle towards the east.

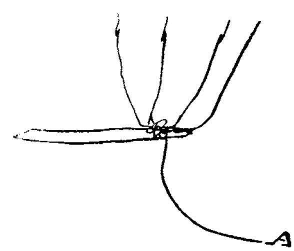
31. The appearance of the spark between the prime conductor and metal discharger very remarkable when the charge is thus passed through water. The spark yellowish, slow, flamy, about 3/4 of inch long, with little or no noise and in appearance, etc. very like a voltaic spark.

32. Then put a piece of box wood charcoal on to the prime conductor, and another piece on to the discharger, so as to pass the discharge between them and yet also through the water of the wet string included in the circuit. When thus retarded, the discharge of the battery gave a quiet spark at the charcoal, but it was very luminous and bright upon both surfaces of the charcoal, much resembling there the brightness of the voltaic discharge upon similar surfaces. In the rest of the course of the spark, i.e. the intervening distance, it was not so bright.

33. When the discharge not retarded by water was taken upon the charcoal surfaces, the spark was of the ordinary kind but very bright upon both surfaces of charcoal (resembling in that respect the voltaic effect), and the noise was loud and sharp and ringing.

34. When water formed part of the circuit (8 inches of wet string) then the shock of the battery charged by eight turns of the machine was such as I could easily support, caring indeed nothing about it. But when no water was there, I could not well bear four or five turns, the sensation then being general through the arms and chest and very much indeed greater than the former.

35. Now tried if currents drawn by points would affect the galvanometer. The end B was therefore connected with the discharging system and the end A by a long wire with a kind of fork consisting of four prongs of copper wire each terminated by a



1488. Oppose equal decomposing plates to equal generating plates.

1489. Alter size of the Poles.

1490. „ strength of the solution.

1491. „ nature of the decomposing body.

1492. Compare intensity of one pair or of two to machine electricity intensity.

1493. *All* decompositions require probably the same intensity if relieved from secondary action. No.

1494. Arrange two simultaneous decompositions of different kinds.

1495. By use of pure or amalgamated zinc, may have a battery not active when not connected—active when connected—here all the Electricity used and accounted for that is produced by chemical action. The reduplication being to produce intensity.

1496. This definite production of electricity a proof that the electricity is *due* to chemical action in voltaic pile, for 34 grs. of zinc and 9 of water, etc. produce a definite quantity, etc. etc. etc., as the argument runs.

1497. This may cause the Voltaic trough to be an economical mode of preparing some things, as Barium, etc. etc., or even Pm., for get it by the equivalent of Zinc used.

1498. But will shew that plan to decompose proposed in some other cases bad, because only the equivalent could be obtained, however many series of plates were used.

1499. In De la Rive's expts. with plates, etc., besides the constant effect of each additional diaphragm, there was also a counter effect of the elements arranged at the plates by the current, and this cause another ratio of the sum of effects.

1500. That this effect of arrangement is great is proved by the improvement in a trough produced by stirring the acid between the plates; the increase of power is great.

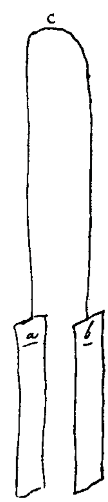
1501. When the charge in a battery is an acid able to act on the metals though no current occur, as N.A. for instance, the quantity or intensity (consider the two here) above the unit of effect is discharged laterally. Refer to tuning fork vibrations, etc. etc. for a figure.

1502. The power of decomposing water a good *unit* of intensity in voltaic apparatus. Query its relations to common intensity. Make this out by reference to diverging power of a battery with a certain charge—a *water* or *acid battery*.

1503. Daniell called on me to-day to ask me about my views of the elementary expts. of single pr. metals and the relation to pile, etc. etc., and if it had not occurred to me whether he might work it. I told him my views, and wished him to work cotemporaneously with me. He behaved very generously, leaving it open to me alone. But if another catches my idea, and works it out before I can write my paper, I shall always regret that Daniell has given way to me, and that another should come before him. Must leave fluorine and hasten this matter of the VOLTAIC PILE. I shewed Daniell my preparatory notes for the paper.

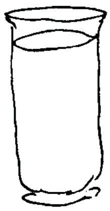
12 FEBY. 1834.

1504. Expts. on the generating plates and the intensity of the current they produce. The existence of a current was ascertained both by the chemical action at the Platina plates and also by a galvanometer having two needles. The position of the latter and of its wire was such that, when influenced by the earth only, the upper needle stood as in the position *a*; but that when the ends were zinc Z and Platina P, and dipped in dilute Sul. acid, the deflection was more or less in the direction *b* indicated by the dotted line. In the expts. to be described Z was always made zinc, or at least the more oxidizable metal, and P always Platina.

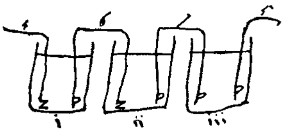


1505. Numerous arrangements of plates were now made which will be understood from the following description. *a* and *b* are plates of platina about 2 inches and 1/2 long and 1/2 or 2/3 of an inch wide. These are soldered on by gold to a Platina wire *c* about 8 inches long, so that *c* can be bent and the plates *a*, *b* inserted, either into the same glass or into contiguous glasses containing acid or other solutions, so as to build up a form like the *couronne des tasses*.

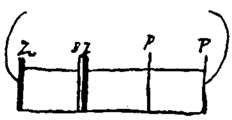
1506. Other pairs of plates had one platina, another zinc—or were platina and amalgamated zinc—or platina and copper, the metals



associated being different but the form the same. The cups were of the form in the margin and of glass, that the insulation might be perfect. I intend representing the different arrangements thus[1]; for instance, where i, ii and iii are three cups containing acid or other liquid to be described, 4 and 5 the zinc and platina plates just referred to as connected with the galvanometer, and 6 and 7 pairs of plates like those just described, 6 being platina at one end, zinc at the other, and 7 platina at both ends. The terminal plates, as 4 and 5 here, shall always be in the same relative position and order to the galvanometer as that described in the first expt. of this day.



1507. It will easily be seen that the arrangement of cups above is in effect exactly the same with the following, where a glass trough consists of three cells containing acid, at Z a zinc plate terminates the trough, at P one of platina., at PZ a compd. plat. and Zinc plate divides the acid, at P a platina plate only; and so on for all other arrangements of cups and plates.



1508. Some Sulphuric acid was diluted when cold; it had a Sp. Gr. of _____ and acted powerfully on ordinary zinc, evolving much hydrogen, but not on amalgamated Zinc, except very slowly. This was put into glasses and arranged with a pair of platina plates and the final plates, thus*. Glass rods were put into the cups between the plates to keep them from touching each other.

1509. In this case there was no sensible current. The middle plate of Platina had prevented all transfer or current of Electricity, even to the galvanometer.

1510. Removing A, I used in its place a mere platina wire, but the effect was exactly the same. Hence making the interposed plate touch by small surface, whilst evolving plates are large, causes no difference: no current.

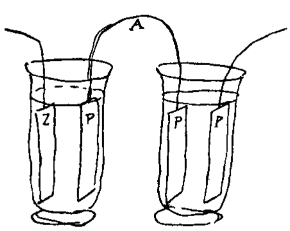
1511. I then used very large plates at ends of A and mere wires of zinc and platina at the extremities, but neither did this alter the effect.

1512. Variations of this sort therefore, as to mere quantity of evolving surface or mere increase of facility at transmitting surfaces of intervening plate, are of no consequence.

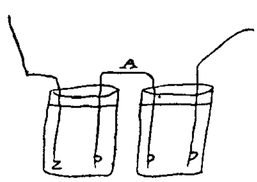
1513. It is needful that in these and similar experiments care.

[1] Refers to diagram in margin.

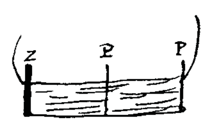
* [1508]



or thus



or thus



should be taken that all the Platina plates are equally clean and pure, or else they by their differences cause currents of electricity which can pass.

1514. Now used two electro-motive pairs of plates instead of one and one interposed decomposing platina pair, thus*.

There was at first a little deflection, but it soon gave way and the needle stood uninfluenced by any current; nor was there any gas or other signs of current at platina plates. So that it would seem that one decomposing plate of Platina having *no affinity* for the bodies evolved was able to stop the current which two decomposing pairs of zinc and platina plates tended to produce.

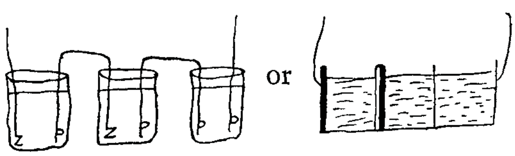
1515. When a third pair of electro motive plates were used, thus†, then a permanent current appeared both by the galvanometer and by the bubbles on the platina plates. Its direction being as if all the plates except the extreme ones were removed.

1516. Three series of decomposing plates therefore are able to produce a current having intensity enough to overcome the affinities of oxygen and hydrogen in cell 1. Two have not, but seem almost equal, if not quite; perhaps just balanced.

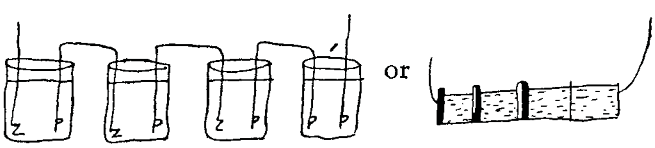
1517. Then three pr. of electro motive plates were put against two decomposing plates of platina, thus‡; but there was now no deflection—no decomposition—no current. The two mere decomposition required in cells 1 and 2 were more than could be effected by the current having an intensity due only to three zinc decomposition in 3, 4 and 5.

1518. Then *four* pair of Electromotive plates were opposed to two decomposing platina plates, thus §. Here there was no current —no deflection—no decomposition.

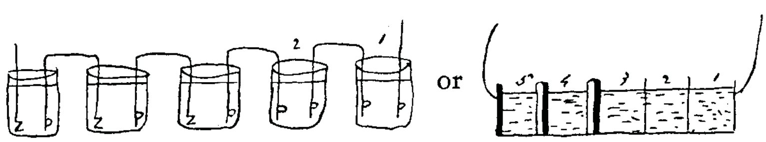
* [1514]



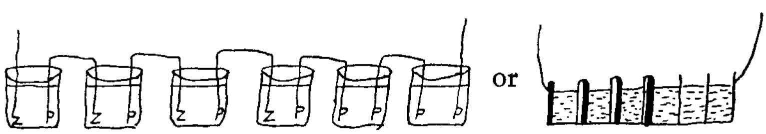
† [1515]



‡ [1517]



§ [1518]



1519. Then tried *five* pair of Electromotive plates against two decomposing plates of platina, as thus*. This gave a feeble current; there was both deflection and decomposition. The current was however much less than if all the intermediate plates were taken away, for on putting then into acid, 6 inches apart, very strong resulted and good decomposition as shewn by the gas on the platina.

1520. Hence five pairs contrasted with two decomposing pairs do not give a power equal to one unobstructed pair.

1521. In all these experiments the Zinc plates were amalgamated so as not to act except when the current was passing. But must try whether the ratio of effect the same with unamalgamated plates.

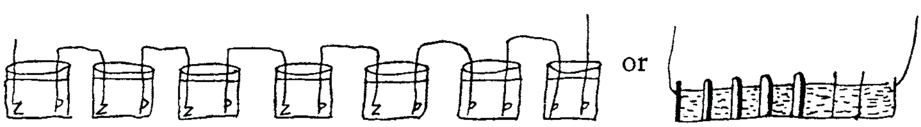
1522. I now changed the nature of the intermediate decomposing plate, using an amalgamated zinc instead of a platina plate, thus†.

1523. There was a strong current just as if the middle zinc plate were away, for it held the needles at right angles to the coil and decomposition was plentiful on the platina plate. Hydrogen was evolved at 1 and also at 2, but not at 3 or 4, the oxygen being rendered there and uniting to the zinc. There were a few bubbles on 3, but these were due to the direct action of the zinc on the acid, as I found by a piece left in the acid, and yet the circuit broken.

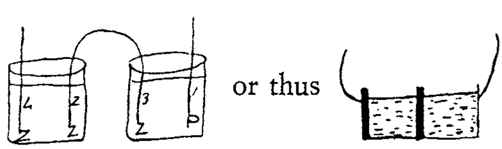
1524. Now two prs. of zinc intermediate plates were used in place of one, thus‡. This gave a strong current having the same direction as in former cases. It still was much weaker than when only one intermediate.

1525§. Now used three intermediate zinc plates to single voltaic pair. Still a current and pretty strong, but no[t] so strong as if from P to Z1 had been merely the dilute Sul. Acid uninterrupted.

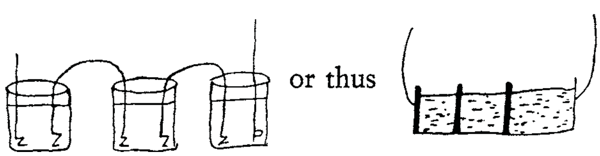
* [1519]



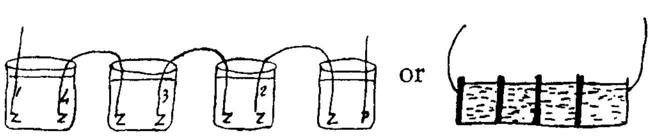
† [1522]



‡ [1524]



§ [1525]



The intermediate series therefore do retard each the current and its effect. There was evolution of hydrogen on the P and also on the zinc ends 2, 3 and 4, in apparently equal proportions.

1526. These zincs were all amalgamated.

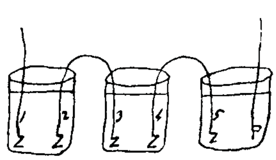
1527. That each zinc interposed should retard the current seems to be because each zinc wanted a little more power under the circumstances to enable it to decompose Water, and this power the current gave it, loosing of its own force. But still the point recurs; how is it that the zinc *in contact* with the platina has such power as to decompose water and half succeed in decomposing water at a second surface?

1528. How very needful the current is to the decomposition in a single pair of plates. How very needful for the existence of the current *is decomposition* in the cases where the intervening platinas are used. But as they cannot be cause and effect to each other, what is the common origin or cause of both? Must make this out. It is of no use continuing to suppose one as producing the other in either order.

1529. Now repeated the last expt. but one, i.e. the one with two interposed pair of zinc plates, but used them *un-amalgamated*, thus*. The end 1 was amalgamated, but 2, 3, 4 and 5 were not, or in the second figure *a* was amalgamated but *b* and c not. In this case the current was far more intense than when the intermediate zinc plates were amalgamated, and was as intense, I think, as if they had been away. Perhaps even a little more so. The fact shews that the nature of the interposed metal very important. For Platina interfere very much with the current. Amalgamated Zinc much less and Clean Zinc still less. This is consistent with the relations of the three to oxygen; and with the platina it is necessary to expell it per force; with the amalgamated zinc the affinity is not quite enough but wants help from the current; but with the clean zinc it can at once decompose the water; requires no help from the current. There is therefore no reaction on it; no retardation, but rather an increase of power, because zinc 5 unamalgamated is in more powerful electro motive relation to the platina than the amalgamated zinc 1.

1530. As the unamalgamated zinc acts by itself, decomposing water, it is probable that its interposed plates produce no retarding

* [1529]



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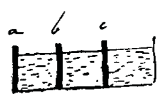


effects, except perhaps as to length of humid conductor involved; for the decomposition is continued on from one to the other without the aid of the voltaic current, receiving only direction of the elements from it.

1531. This unamalgamated zinc was acted on very strongly by the acid, evolving much hydrogen in a short time.

1532. These cases of retardation seem to indicate that the retarding effect is exactly equal to the affinities to be overcome, and that where the current is deficient in power it may be helped by calling in chemical aid; the former being replaced or made up by an equivalent of the latter. Does not this shew a remarkable *identity?* Further, the original current is in its power in proportion to the chemical affinities producing it, again shewing identity.

1533. The differences between the different interposed plates seem also to refer the electricity decidedly to chemical action.

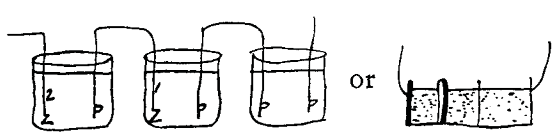
1534. The whole arrangement seems beautifully to shew the antagonism of the *chemical powers* at the Electromotive parts with the *chemical powers* at the interposed parts. The first are producing electric effects; the second opposing electric effects, and the two seem equipoised, as in a balance, and in both cause and effect appear to be identical with each other. Hence chemical action merely electrical action and Electric action merely chemical.

1535. That the chemical and Electrical action are convertible shewn by the condition of action and non-action at the interposed plates, according to their nature and number. It seems that to balance the power of a battery current, all that is required is to interpose plates until the sum of the chemical action against them is equal to the sum of those in the battery. If, as with platina, all the power must come from the current, then few will do. If, as with amalgamated zinc, but little is required from the current by each, then many are required, and so on.

1536. I am continually wanting a clear definite view of the actions in a single voltaic circuit.

1537*. To ascertain the superior electro motive power of un-amalgamated over amalgamated zinc, now experimented to see if two pair of it, and Platina, were also neutralized by one interposed platina plate.

1538. There was a very trifling current at first, but that soon

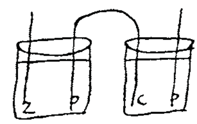
* [1537] 

ceased and the tendency seemed balanced. Perhaps there was no reason to expect that the abstraction by sufficient affinity of oxygen in two generating cells was more than sufficient to counterbalance the two separations of oxygen and hydrogen in one cell; since the attraction of oxygen for hydrogen must be the same as of hydrogen for oxygen.

1539. There was much gas evolved at 1 and 2 zincs, but not at any of the platinas. There was no current.

1540*. Then tried the electro motive power of pairs of Platina and copper in this pure strong Sul. Acid 1336. But there was no sensible action either at Galvanometer or by appearance of bubbles on the platinas. Probably a trifling action, but nothing of importance to me; neither was there chemical action.

1541. Concluded a pair of platina and copper plates would therefore neutralize the current of a pair of platina and zinc plates, thus; and found this was the case, notwithstanding that the Plat. and copper were in the order to form a current like that of the platina and Zinc. This may also well illustrate the way in which bad plates, or a weakly charged or exhausted trough, or one with copper on the zinc, interferes and neutralizes the action of the other good.



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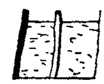
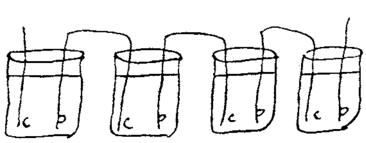


1542. Never put weak and strong troughs in succession again. Better to leave out the weak. Have more power.

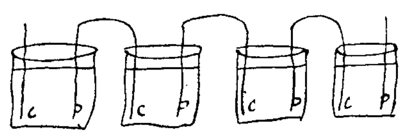
1543†. The pile was now reconstructed with the four pair of copper and Platina plates, but a little nitric acid had been added to the Sulphuric acid with which the glasses were charged. The arrangement was now very active and gave a powerful current to the galvanometer; one pair being as powerful in that respect as the four, according to the well known law. But the important point is to observe that, the moment the chemical action upon the metal could go on, that moment a current began. The oxidation of the metal is not now from the water but from the nitric acid.

1544. Is it not possible that the electricity is evolved between *the metal* and what it combines *with,* i.e. in this case the oxygen; and that the previous condition of that oxygen is only of consequence as offering a certain quantity of chemical effect to be overcome, and a relation to other particles which can favour or allow of

* [1540]

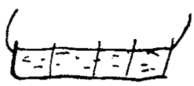


† [1543]



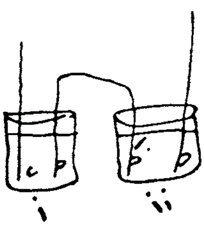
transmission by Electrolytic action across the humid conductor? The combination of the acid with the oxide produced seems to have no *direct* influence in producing electricity, for the Electricity produced is *equivalent to the* OXYGEN combined with the metal.

1545. In that case, look to the particles of the metals for the Electricity evolved, and in that view remember their conducting yet indecomposable state, which no other set of bodies have, i.e. no other set than those which, like the metals, are needful in the voltaic pile.



1546. Took out the three intermediate pairs of copper and Platina, and using the same Sul. Nitric acid, interposed a single pair of platina plates between the extreme copper and platina plates, thus*; and found that a strong current passed. Then used two interposed platinas, thus†. Still a current passed. And even when three interposed platinas were used, still a current passed, though weaker much than in the former cases.

1547. Must remember that Nitric acid present in all the glasses, that therefore the copper has not to decompose water, but finds oxygen ready to combine freely with it; its powerful affinity is not therefore opposed by another powerful affinity and the electricity evolved is of proportionate intensity. On the other hands, the interposed plates afford *less* obstruction than if in Pure S.A., for there is either no need to decompose water there, or if there is, the hydrogen has an affinity presented to it at the N. Electrode. Probably get some means here of deciding whether N.A. is decomposed primary or secondarily, and if the latter, how it favours.

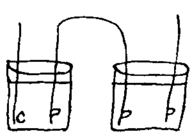


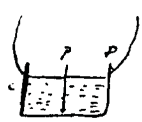
1548. Then again in this arrangement. When the acid in both i and ii was Sulphuric with Nitric acid, the single pr. of copper and platina was able to send a current through the whole. But when the acid in ii was the dilute Sul. acid without nitric acid, so that *water* had to be decomposed there to supply the oxygen at the Platina 1, then *no current passed.*

1549. Hence the chemical relation at the *incidental* or decomposing plates important as to the final decomposition and transfer.

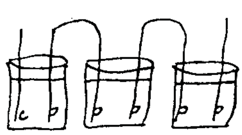
1550. It is curious to observe how the possibility of decomposition against the platina plates in ii *here governs* the evolution of Electricity. I see no positive proof as yet that contact is necessary for

* [1546]





† [1546]



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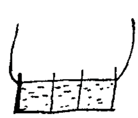
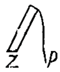
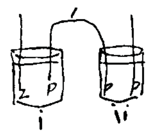


generation, only for the conduction and restoration of state. But the current seems necessary for decomposition.

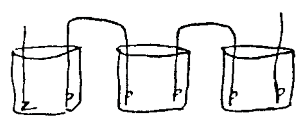
1551. Must consider the case of single decomposition very well and closely,  for that includes the whole. Why is it necessary there should be a discharge of electricity before action can go on? Why not zinc alone decompose, and how is it that in existing circumstances the platina helps?

1552. Made an expt. of transmission through many intervening plates by only one pair of Zinc and platina plates, thus*. But all the vessels contained sulphuric with Nitric acid diluted as before. The current passed pretty strongly through one plate—it passed also through two, not so strongly—and it even passed through three intervening platina plates, but less strongly. Hence to obtain an intense current, a charge with N.A. probably good; but effect here on intermediate was also on dilute N.A.



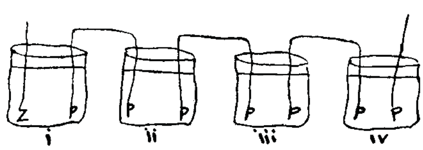
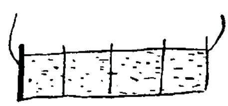
1553. When i contained the S.N. Acid with zinc, the current evolved would pass through the interposed Platina plate 1, though the acid in ii was pure sul: acid diluted, but by no means equal to what would have passed had ii contained S.N. Acid.

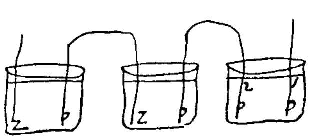
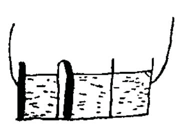
1554. The current could then even pass through two intervening plates, though ii and iii contained sulphuric acid without any nitric acid; but the force was very small compared to that of the current passing when ii and iii contained Sul. N. Acid.



1555. Now used Muriatic acid so strong as to act well and quickly on unamalgamated Zinc, and evolve much hydrogen. Only one generating pair of zinc and platina plates were used. The current could pass feebly by one interposed platina, but scarcely sensibly with two. Then arranged two pair of zincs and platina with only one interposed plate, thus†. There was a feeble current on the galvanometer, but so feeble as not to give any quick signs of bubbles on the Neg. Plat. wires. When P1 made to touch P2 so as virtually to remove the latter, then a powerful current passed and much hydrogen gas was evolved at all the Plat. surface.

1556. Notwithstanding these and all other variations in the intensity of the power, the law of definite action of quantity appears to hold constantly true.

* [1552]  or 

† [1555]  or 

1556½. Mur. acid can give no chlorine to zinc without evolving Hydrogen somewhere. In this respect it is like water and unlike Nitric acid. Hence more like water in its *actions* also and regulations.

1557. The Muriatic acid did not make a much better voltaic battery with Copper and platina than the Sul. acid did.

1558. With respect to the interposed plates, it is easy to pass from them to voltaic pairs of plates, considering first platina plates with sul. Acid and then N.S. Acid, then zinc plates with same variation and then conjoint plates acting of course as battery plates.

1559. Compare intensity of one or of two pairs of plates to machine intensity.

1560. Decompositions cannot all require same intensity of current. Evidently differ in the attraction of the particles united, and also in the attractions of the plates against which their elements are to be rendered.

1561. Try a decomposition of Nitre or fuzed chloride of silver by zingle pairs of plates.

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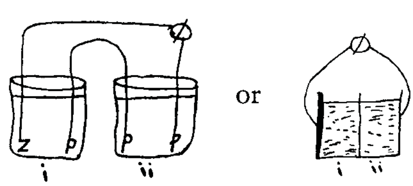
1562. Examined the action of pure zinc on the S.A. diluted; it acted and hydrogen was evolved, but it acted very differently indeed to a similar piece of ordinary zinc; yet on putting platina wire in contact with it, the wire gave off abundance of gas, more than if in contact with the ordinary zinc. Left the acid with both pieces of zinc in. I have no doubt the small piece will be gone in an hour or less. Next morning examined the glass. Of the ordinary zinc only a little flocculi, black (lead, copper, etc.) was left; of the pure zinc above one half was left. That was still evolving gas, and being touched by the platina wire, evolved on it abundance of gas. This was not quite pure, for there hung about it metallic flocculi which proved to be lead. Perhaps, however, they had been dissolved from the other piece and reprecipitated on this.

1563. See De la Rive on pure zinc.

1564*. Now experimented on certain decomposition effected by a single pr. of plates to ascertain whether the intensity produced by one constant pair was enough for *some decompositions and* NOT *enough for others.*

* [1564]

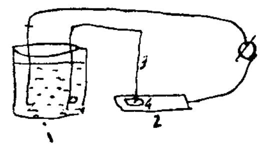


1565. When the same dilute Sul. acid was in both i and ii then no permanent current passed; a little effect when the plates were put in, but no other. But when a little N.A. was put into i, then a permanent current passed, but not very strong. Hence increasing the action in i by N.A. (query whether more s.a. would do), made the current far more intense.

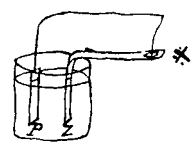
1566. When N.A. was added to ii also, facilitating the decomposition, etc. required to favour current there, then a strong electric current passed. So here a PROOF that a single pair of plates can *decompose* well in a second vessel.

1567. When N.A. was in the S.A. of ii whilst only S.A. diluted in i, then the current.

1568. Now experimented on solutions of hydriodate of potassa, sulphate of soda, etc. etc. etc., thus. i contained dilute S.A. (no N.A.). The zinc plate Z was connected with the wire of the galvanometer ∅ and that with a platina plate 2, on which rested a piece of paper of two thicknesses moistened in the solution to be tried; and upon this the end of P rested, the whole of that, both plate and wire 3, being platina. When the paper at 4 was moistened in solution of hydriodate of potassa, plenty of iodine instantly appeared (as it ought to do) at the end of the wire 3; and when the paper was turmeric paper, corresponding dots marked by alkali were found on the opposite side against the platina plate 2. Hence a single circle will decompose hydriodate of potassa freely, though only S.A. be in i. The Galvanometer swung but very little, the point of contact being very small for the discharge; but the iodide a better test than the galvanometer.



1569. Now arranged so as not to have a single pair in metallic contact. In fact no metallic contact, thus. P was a platina plate made fast to a platina wire which was bent so as to come round and rest on the piece of moistened paper at *. Z was a zinc plate amalgamated and bent round so as to sustain the piece of paper at *. The vessel contained dilute Sulphuric acid without any N.A. The observation was to be made at * and there was no metallic contact here except perhaps the difference of amalgamation at the two ends of the zinc plate.



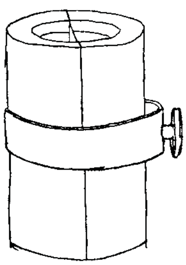
1570. Neutral hydriodate of potassa in solution both on white

1742. Thus it is proved that the ignition of a platina wire has no relation to its length but only to its diameter. For the same quantity of electricity that will heat one inch of wire red hot will heat a hundred inches if it pass through. The only effect of a long wire is to retard the current and so diminish the quantity which passes, and hence the lower heat in the usual form of making the experiment.

1743. The first effect of a battery is not an effect of accumulation, but only a result of the favourable state of the fluid at the plates. It is both: Oct. 1834.

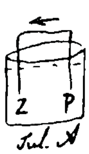
13 MAY 1834.

1744. Have begun experiments with single circles to determine the current induced by the use of different metals and especially different fluids, for I do not believe that metals are rendered, as the phrase goes, positive by acids and negative by alkalies.

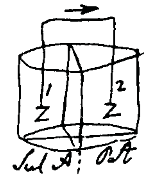


1745. Have had a wooden cup made, cylindrical in form and open only at one end. This had been cut down the middle into two halves, which could be held together so as to make one vessel by a ring going round it which could be tightened by a screw. If before putting the two halves together a piece of filtering paper were interposed, and then the screw tightened, the paper divided the cup into two cells or portions, into one of which acid and into the other solution of alkali could be put; and these were then in electrolytic communication and could be used to form the fluid communication between two pieces of metal, either of the same or different metals, and which might either communicate together directly or through the wire of a galvanometer and yet form only a single voltaic circle.

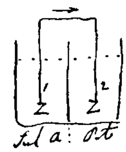
1746. This cup is an excellent general instrument of investigation. Can try all sorts of combinations by it; it becomes a universal chemelectric test. It will supply the arrangement of *two metals and one fluid*; or of *two fluids and one metal*; or of *two metals and two fluids*; or of *any combination* of these in the simplest manner.



1747. I shall represent the results of experiments diagrammatically thus: the first figure shews that when zinc and platina are in the same portion of dilute Sulphuric acid, the electric current by the Galvanometer is from the P to the Z through the metal and from the Z to the P through the liquid. Fig. 2 shews that when two pieces of Zinc are put into dilute S.A. and sol. of potassa in electrolytic communication, the current is from Z^1 to Z^2 through the metal and from Z^2 to the Alkali, then to the acid, and then to Z^1 below. That in fact that Zinc is rendered more positive, as the phrase is, by solution of potash than by dilute Sulc. acid.



1748. *Zinc in dilute S.A. and in Potassa*. The current was always as in the figure, so that the Zinc in alkali was positive to the Zinc in acid, and strongly so at first—so as to swing the galvanometer needle quite round. The effect was strongest at first. As effervescence at Z^1 from direct action of the acid came on, the deflection diminished, but was renewed by taking out Z^1 and after a moment returning it, touching the surface of the acid with it. In this way the effect was very strong.



1749. By keeping Z^2 in the Alkali and making Z^1 touch the surface of the acid at intervals of about 1/2 or 1/3 of a second, a far greater deflection of the needle could be preserved than if the pieces of metal were retained constantly in the solutions.

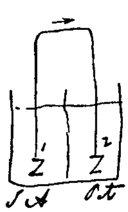
1750. The pieces of zinc and of other metals used in these experiments to touch the acid and alkali were merely wires about the 1/15 or 1/20 of an inch in diameter and made perfectly clean.

1751. A little piece of platina wire was twisted round the end of the wire Z^2, or that in the alkali, to diminish its force a little or render it less positive towards Z^1, but still, though this platina was with Z^2 immersed in the alkali, it made no difference in the direction of the current nor apparently in its force.

1752*. When the two Zincs were turned, i.e. taken out of the respective liquids and immediately immersed in the other liquids, the first effect at the Galvanometer was a current in the same direction as before, i.e. from Z^1 through the Galvanometer to Z^2 (which implies a current from Z^2 to the acid, then to the Alkali and then to Z^1); but in a moment this is reversed and a strong current sets from Z^2 through the Galvanometer to Z^1, then to the Alkali, next to the acid and again to Z^2.

1753. The final reversion of the current is consistent with the first observation that the Z in Alkali is more positive than that in acid. The momentary current produced directly after the change of the metals is due principally, I think, to the acid and alkali

* [1752]



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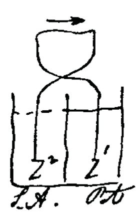
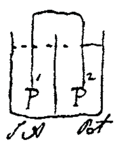


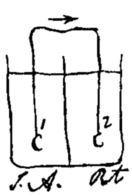
carried by the metals, and being that which, until it is displaced, acts upon them; and the Electrolytic conductor, instead of being S.A. against Z^2 and Potash against Z^1, is Potash against Z^2, then S.A. next, again Potash and finally S.A. against Z^1. So soon as these terminal but small portions of S.A. and Potash are neutralized by the surrounding Alkali and acid, then the current takes the direction consistent with the first result. The whole is therefore consistent.

1754. In order to free these and the other experiments of a similar kind from the supposed influence of the action going on between the acid and alkali at the paper diaphragm, Platina was next used, these pieces being not merely wires but large plates. The effect on the Galvanometer was so little as to be doubtful altogether. If existing, it was from the Alkali to the acid and then back from P^1 by the galvanometer to P^2. On washing and changing the platina plates the result was the same. On using fresh S.A. and solution of potassa still it was the same. Hence there is no sensible current of Electricity produced by the action of the acid and alkali on each other at the diaphragm. I have before given reasons why there could not be, for there are not Electrolytes to conduct on the power; i.e. the S.A. and Alkali are *ions* in the separate state and not combined, therefore no *electrolytic conduction* can be effected by them.



1755. In the former and future experiments therefore the current is the result of the action of the acid and alkali on the *immersed metals*.

1756. *Copper in S.A. and Potassa*. On using two pieces of copper wire cut from the same piece, the results were exactly as with the Zinc. That is, copper in Potash is more positive than copper in dilute S. Acid. The momentary effect on reversing the places of the copper was not so strong as with the zinc, but was very distinct.



1757. The relation of the copper to the acid and the alkali was much exalted by previous immersion of them in acid and alkali, i.e. copper from acid put into the alkali and copper from alkali put into the acid formed a stronger combination than if merely cleaned copper had been used.

1758. This effect may have relation to Marianini's effect, i.e. be due to the momentary current which has been formed in the

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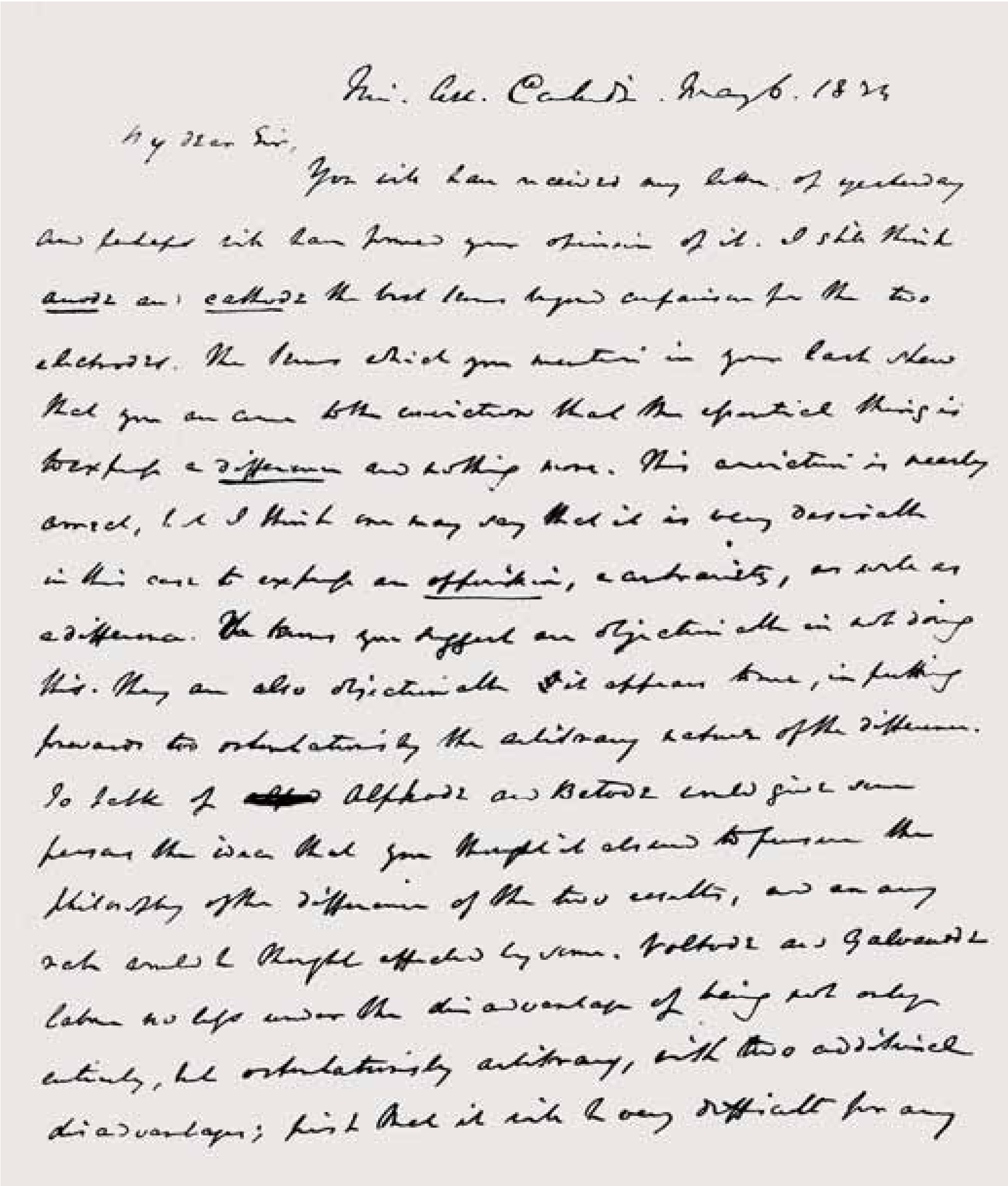

Trin. Coll. Camb. May 6. 1834

My dear Sir,

You will have received my letter of yesterday and perhaps will have formed your opinion of it. I still think anode and cathode the best terms beyond comparison for the two electrodes. The terms which you mention in your last show that you are aware of the conviction that the essential thing is to express a difference and nothing more. This conviction is nearly correct, but I think one may say that it is very desirable in this case to express an opposition, a contrariety, as well as a difference. The terms you suggest are objectionable in not doing this. They are also objectionable, it appears to me, in putting forwards too ostentatiously the arbitrary nature of the difference. To talk of ~~alpha~~ Alphode and Betode would give some persons the idea that you thought it absurd to pursue the philosophy of the difference of the two results, and as any such would be thought affected by some. Voltode and Galvanode labour no less under the disadvantage of being not only entirely, but ostentatiously arbitrary, with two additional disadvantages; first that it will be very difficult for any

First page of the letter written to Faraday on May 6, 1834, by W. Whewell, afterwards Master of Trinity College, Cambridge, advising the use of the terms "anode" and "cathode". The first occurrence of the words in the Diary is on May 13, 1834 (§1758). [*Reduced from* 8 *in. x 10 in.*]

Full text of the letter from Whewell to Faraday.

Trin. Coll. Cambridge. May 6, 1834.

My dear Sir,

You will have received my letter of yesterday and perhaps will have formed your opinion of it. I still think *anode* and *cathode* the best terms beyond comparison for the two electrodes. The terms which you mention in your last shew that you are come to the conviction that the essential thing is to express a *difference* and nothing more. This conviction is nearly correct, but I think one may say that it is very desirable in this case to express an *opposition,* a contrariety, as well as a difference. The terms you suggest are objectionable in not doing this. They are also objectionable it appears to me, in putting forward too ostentatiously the arbitrary nature of the difference. To talk of Alphode and Betode would give some persons the idea that you thought it absurd to pursue the philosophy of the difference of the two results, and at any rate would be thought affected by some. Voltode and Galvanode labour no less under the disadvantage of being not only entirely, but ostentatiously arbitrary, with two additional disadvantages; first that it will be very difficult for anybody to recollect which is which; and next that I think you are not quite secure that further investigations may not point out some historical incongruity in this reference to Volta and Galvani. I am more and more convinced that *anode* and *cathode* are the right words; and not least, from finding that both you and Dr. Nichols are ready to take any arbitrary opposition or difference. *Ana* and *Kata* which are *prepositions* of the most *familiar* use in composition, which indicate *opposite* relations in *space,* and which yet *cannot* be interpreted as involving a theory appear to me to unite all desirable properties.

I am afraid of urging the claims of *anion* and *cation* though I should certainly take them if it were my business–that which *goes to* the *anode* and that which goes to the cathode appearing to me to be exactly what you want to say. To talk of the two as *ions* would sound a little harsh at first: it would soon be got over. But if you are afraid of this I think that *stechion,* as the accepted Greek name for element, is a very good word to adopt, and then, *anastechion* and *catastechion* are the two contrary elements, which I am sure are much better words that you can get at by using dexio and scaio or any other terms not prepositions.

I expect to be in London Friday and Saturday, and if I am shall try to see you on one of those days and to learn what you finally select. Believe me

Yours most truly

W. WHEWELL.

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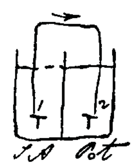
reverse direction. Or it may be due to the clean and chemical condition of the metal wires. On looking at the two wires, the one from acid looked palest and most metallic; the one from alkali looked a little redder, as if a film of oxide were upon it. The difference was small, but still it might be enough to account for the effect, for the Alkali would have a perfectly clean surface to oxidize, and the coat of oxide in the acid might help to take the hydrogen from the water in the acid; for it is certain that, when the currents are formed, water is electrolyzed; and that oxygen is disposed of at the *anode* in the alkali and hydrogen at the *cathode* in the acid. This will be shewn well in the *Tin* experiment, and I have no doubt that Tin in solution of potassa could cause hydrogen to be evolved against Copper in dilute S.A.

Tin in S.A. and Potash.

1759. This metal acted as Zinc and Copper, but more interestingly. First, the Tin in Potash was always strongly Positive to the T. in Sul. acid. In the next place the tin required to be clean or the current would not be produced, and sometimes when it was apparently quite clean, still no current was occasioned. Then on a sudden the current would start into existence, and was always powerful and from Z^2 through the alkali and acid to Z^1.[1] When the current existed, a regular decomposition of the water occurred and hydrogen gas was evolved at T^1. This was not due to the action of the acid on T^1, but solely to the current; for when T^2 was taken out of the potash the evolution of hydrogen at T^1 ceased, and was again renewed by the re-immersion of T^2. Hence both chemical and galvanometrical proof of production and *direction* of the current and proof that the Tin in the Alkali determines it and is the Positive metal.



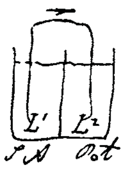
1760. On changing T^1 and T^2 for each other, the current through the cup was reversed for a moment, but quickly resumed its first direction as before. So that all right in that respect, and either piece of Tin could be made Positive to the other by immersion in the alkali.

1761. These experiments with Tin excellent. They are good proofs that metallic contact is not required. They afford also an excellent and wonderful case of the transference of the chemical powers,

[1] ? T^2 and T^1.

for in fact a piece of tin of any length can have its power of determining the combination of oxygen at one end made influential in determining hydrogen to the other end.

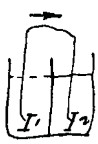
Lead in S.A. and Potash.



1762. Lead acted as the other metals, being positive in solution of potassa to other Lead in dilute S.A. The current was very fair at first, and hydrogen was evolved at L^1 in the acid, as in the case of tin; but not so abundantly. If the contact of L^1 with the S.A. be intermitting, a good deflection can be continued.

1763. On changing L^1 and L^2 in the divisions, then a little and only a little inversion of the current in the cup, but there was much strengthening of the principal current from the Alkali to the acid.

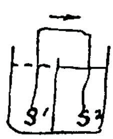
Iron in S.A. and Potash.



1764. Iron gave a current in the same direction with the other metals, but it was not strong. There was plenty of Hydrogen from the I^1 in the acid, but it was all chemical. It illustrates that the intensity of action there is nothing to the production of an electric current unless the power is electrolytically transferred onwards.

1765. It will be important however to consider in this case and in the case of Zinc, how it is that an affinity which can cause water to be decomposed that the metal may be oxidized, does not determine a current through the liquid toward the I^2. There is some important point hanging by this. It may however depend upon the iron presenting an infinity of little voltaic circles because of its heterogeneous nature. This by the bye is probably the true cause of the effect.

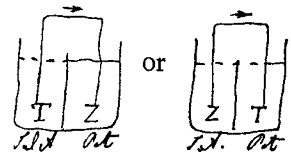
Silver in S.A. and Potash.



1766. There was scarcely a sensible action, but on very close examination the little current observed was, as in former cases, from the alkali to the acid in the vessel. When tried in fresh acid and alkali still the same.

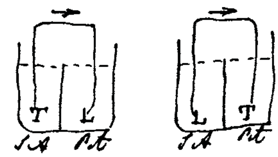
1767. Now tried a few cases of two metals and two fluids. Remember that in such cases no cause of action or *current* exists at the contact of the two metals or of the two fluids, but only between the metals and fluids.

1768. *Tin and Zinc compared both ways in S.A. and Potassa.*



Whichever of the metals is in the Solution of Potassa is Positive to the other and determines a current from it through the liquids to the other. By former expts. (1697), if both Z. and Tin be in the same liquid, whether that be sol. of Potash or Amm. or dilute S.A., the Zinc is positive to the Tin. Hence power of alkali very evident.

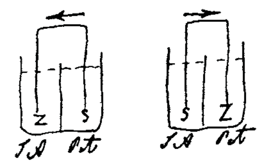
1769. *Tin and lead.*



In both cases the metal in the alkali was positive to that in the acid, but the current was weak when the lead was in the alkali, and stronger far when the Tin was there. The lead in the Sul. Acid then actually evolved hydrogen whilst the tin touched it and was also in the alkali.

1770. When both the Tin and lead were in the Alkali, the Tin was much the most positive. When both were in the Sul. acid, there was very little difference. This agrees with former expt. (1703).

1771. *Zinc and Silver.*



Zinc was always positive to silver. But it was more positive when in the alkali than when in the acid. The current is always from the Zinc through the acid or alkali to the Silver when both metals are put into the same solution (1701).

1772. In these experiments the Sul. Acid and the potash gradually combined, forming a thick layer of crystals of Sulphate of potassa on that side of the paper only which was in contact with the solution of alkali. This might be a consequence of the acid being stronger than equivalent to the potash in an equal bulk of solution, but it is worth noticing as having always happened on this day. Note next time the relative value of equal volumes of the acid and alkaline solution, etc.

1773. The formation of such quantities of Sulphate of potassa without an enormous current of electricity, or even any current, shews that acids and alkalis in combining thus from the free state do not produce current.

1775[1]**.** May get the order of intensity of current formed by different solutions by means of this cup, etc.

[1] Par. 1774 is omitted in the MS.

1775½. Must make out how or why bodies in solution which, like Sul. Acid or Veg. acids or alkalies, give nothing to the oxidizing metal, still have different exciting powers.

1776. There may be a difference in the intensity due to the exciting cause and yet the quantity of electricity excited or transmitted be the same. Thus with Solution of Potassa and Sul. Acid, the first may excite electricity of a greater intensity than the acid.

1777. Has not the useful effect of the nascent state something in it analogous to the condition of a single circle?

1778. I sometimes think it may be possible to have a current of Electricity without a circuit, that is, to have an absorptive effect at each end of a series of apparatus. It would be a current between two vessels not forming a circle. Try Amalgam of Zinc and S.A. in one vessel and Copper in Sulphuret of potassa or in solution of sulphur in the other.

1779. Or the nitrate or chloride of Silver arrangement which makes the reverse current. Or silver in Hydrosulrt. ammonia.

1780. Considerations as to the intensities of the different forces which matter is governed by.

1781. The force of attraction of aggregation is as nothing compared with the force of attraction of chemical affinity. Hence chemical attraction so easily overcomes cohesion, and dilute sulphuric acid or even water, where the oxygen is already partly counteracted, will disintegrate and dissolve Iron.

1782. With reference to Mag. attraction, can a Magnet take iron or nickel out of a very fluid amalgam?

1783. Gravity is still far weaker. Must consider the relation of these three forces: Chem. attraction, Cohesion and Gravity.

Herschell's motions of Mercury between the Electrodes or Poles.

Combustion of diamond in oxygen a case.

1784. There is no case of the weakest chemical affinity being overcome by the strongest exertion of attraction of aggregation or even by Gravity. As to Hatchet's [? Hatchett's] cases of alloys, it depends upon other principles.

1785. Hence there is a most intense power active in all the cases of chemical attraction. Even in those of vegetation and animalization.

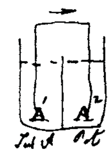
1786. Never *overcome* chemical by mechanical force, although we often *determine* its action. It is then probably incidentally.

1787. Chem. aff. as much surpasses aggregation as the latter does gravitation. Even Solution, which is so weak, can overcome aggregation.

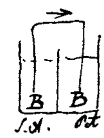
19TH MAY 1834.

1788. Continued the experiments with one metal associated with dilute Sulphuric acid and solution of caustic potassa.

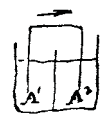
1789. *Antimony in Sul. Acid and Potash.* The effects were just as in former cases, the current from the Potash to the acid in the cell. On reversing the pieces, first the momentary current and then the permanent one, just as before.



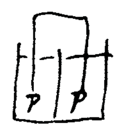
1790. *Bismuth in Sul. Acid and Potassa.* Just as with antimony and the other metals. Current strong at first.



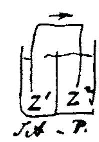
1791. *Arsenic in Sul. Acid and Potassa.* The two pieces of Arsenic used were first heated in tubes, to drive off all the white arsenic and oxide and bring them to a perfectly metallic state, and then allowed to cool in the tubes. The effects were just as in the former cases. On changing A^1 and A^2 for each other, the first current lasted longer than in former cases and was stronger, but as the pieces of Arsenic were crystallized and shaken and as therefore more Sul. Acid and Potassa would by capillary attraction be held against and in them, that may be the cause.



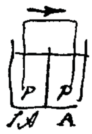
1792. *Palladium in Sul. Acid and Potash.* No effect. It gave no distinct current either way. There were some agitations of the needle, but these I refer to contact of the fingers, for which see notes at the end of this day's experiments.



1793. *Amalgamated Zinc in dil. S.A. and Potassa.* Strong current from the Alkali to the acid through the cup. The zinc in the acid did not evolve gas by itself, but the moment it was made to complete the circuit by contact with Z^2, then it evolved gas. Hence the chemical proofs of the current and its direction also present.

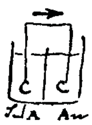


1794. Now repeated many of these experiments, using *Ammonia* in place of *Potassa* as the alkali, but retaining the same dilute Sulphuric acid. As solution of Ammonia is a very bad conductor, sulphate of ammonia was added to it. The ammonia solution was lowered about one half by water and the state of the liquids in the two divisions of the cell continually watched to see that there was always plenty of free acid and alkali there.



1795. *Platina in Sul. Acid and Ammonia.* Large plates of platina were used. The current was distinctly from the Amm. to the S.A. in the cup, but there could not be any chemical action on the metal here.

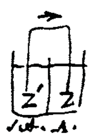
1796. *Silver in Sul. Acid and Amm.* Much as platina. Current feeble but distinct, direction the same.



1797. *Palladium in Sul. Acid and Ammonia.* No action or current.

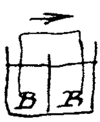
1798. *Copper in Sul. Acid and Ammonia.* Gave a strong current, the direction from the Ammonia to the acid in the cell, and therefore as with Potash.

1799*. *Lead in Sul. Acid and Ammonia.* Current in the same direction but not so strong as when potash used instead of the Ammonia.



1800. *Zinc in S.A. and Ammonia.* The current from the Ammonia to the S.A. in the cup and very strong. It was able to modify the power of Z^1 in the acid of decomposing water, for when contact was broken Z^1 evolved very little gas, but when the contact was made it evolved much. Hence chemical proof of the current as before.

1801. *Amalgamated Zinc in S.A. and Amm.* Just the same as in the last experiment, and as to the chemical proofs even more distinct, becauze Z^1 evolved no gas except when in contact with Z^2. Here therefore Potash and Ammonia the same, although perhaps the former the strongest.



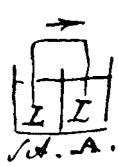
1802. *Bismuth in S.A. and Amm.* There was a current in the same direction as with Zinc, etc., but it was a weak current.

1803. *Antimony in S.A. and Ammonia.* As Bismuth: current weak but in the same direction.

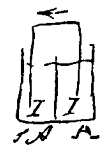
1804. *Arsenic in S.A. and Ammonia.* As Bismuth: current weak but in the same direction, i.e. from alkali to acid in the cup.

1805. *Tin in S.A. and Ammonia.* Tin was remarkable for its action and in some respects much unlike that in S.A. and Potash. Yet a similarity exists. There was sometimes the appearance of a current, but it was uncertain; and on the whole the action was very weak and doubtful and the direction of the current as much one way as the other.

* [1799]



1806. *Iron in S.A. and Amm.* Again, Iron had remarkable effects. Sometimes the current was strongly one way, then strongly the other; but the general result was from the acid to the ammonia in the cup and the reverse therefore with that when S.A. and Potassa were used. These effects could not depend upon differences in quality, for both pieces used were the halves of one piece. They are probably connected with the superficial oxidation of the metal at the moment of contact, etc. etc., and seem to shew both that the forces are nearly ballanced, and also that there is a kind of inertia in the current when once produced.



1807. In all the experiments where the currents are feeble, it is needful to be cautious as to the source of the currents affecting the needle. The ends of the galvanometer wires were at first of copper, and when these were clean, touching them by the fingers of each hand deflected the needle. Even when the hands were washed still I could affect the instrument by holding one end and grasping the other at intervals only. Besides this, a motion of the wire across the magnetic curves of the earth can produce currents, etc. etc. I am inclined to think that some of the current with Platina, etc. etc. might be thus due to extra action.

1808. Must test the current from the Alkali to the acid, when Platina, Palladium, Gold and Silver are used, very accurately and carefully, to be sure that it really exists or not.

1809. The vibrations above by touching occurred even when the galvanometer terminations were of platina wire.

1810. Now made a few comparisons between *potash* and *ammonia*.

1811. *Zinc in Potash and Ammonia*, the latter containing Sul. Ammonia. The current was from the potash to the Ammonia through the cell and of moderate strength.

1812. *Lead in Potash and Ammonia.* Strong current, direction as with Zinc.

1813. *Tin in Potash and Ammonia.* Current weak, direction as with Zinc.

1814. *Copper in Pot. and Amm.* Current weak, direction as with Zinc.

1815. *Iron in Pot. and Amm.* Current feeble, direction as with Zinc.

2982. With respect to the main point of whether the intensity is the same for the same small discharging surface whether it be P. or N., it appears *that it is*, or that the irregularities are due to some other cause.

2983. Tried to obtain a glow upon a large surface rendered Positive by a Neg. point very near to it, but obtained nothing of the kind. In fact it could not be, for a Neg. point will take down glow and cause its disappearance on the end i just experimented with.

2984. Made the end i positive and then by working the machine well and approaching the hand obtained the glow or the brush on it at pleasure. I found the glow gave a powerful direct wind, but that the brush was much less windy. There was a very strong current of air set off by the glowing wire end. Hence not likely that a ball would glow, or at least only on one side.

22 FEBY. 1836.

2985. Made similar experiments to those just described using the large ball (7¼ inches in diameter) and the brass point iv. First of all the ball was primarily electrified P. and N. and the point electrified N. or P. by induction. The connection of the ball was every second observation changed from N. to Pos. or P. to N., so as to allow the effect to be observed both ways at the same distance.

	Ball P. Electr.		Ball N.
In air	... 84°	Star visible on point in dark.	in air ... 83° star
13	... 80°	Star visible on point.	13 ... 79°
11	... 77°		11 ... 70°
9	... 75°		9 ... 67°
7	... 67°	Electr. very unsteady.	7 ... 60°
5	... 50°	Electr. continued unsteady.	5 ... 50°
4	... 35°		4 ... 35°
3	... 20°		3 ... 25°
2	... 12°		2 ... 15°
1	... 5°		1 ... 2°

2986. Great irregularities exist here—there were also great unsteadinesses of the Electrometer with same distance and state. Working machine slow or quick would make 20° difference. Working it quickly caused great vibration because electrical cylinder not true on its axis. Again the P. and N. prime conductor not equally well insulated and charged. But the general result is, I think, that whether the point is P. or N. by induction, the power of discharge through air is the same and the required intensity therefore the same.

2987. Now made the Point primarily P. and N.

Point P.			Point N.		
			in air	... 60°	
in air	... 75°	sank to 70°	in air	... 65°	sank to 60°
16	... 65°		16	... 55°	
12	... 50° ?		12	... 50°	
8	... 45°		8	... 30°	
6	... 30°		6	... 25°	
4	... 15°		4	... 10°	
2	... 7°		2	... 5°	

2988. Think that much of the difference between the two series generally depends as before upon difference of the excited condition of the P. and N. prime conductors. Any effect of that kind comes serious in here where there is a point always throwing off into the air and keeping the electricity down. As the working the machine more or less quickly makes a serious difference in the intensity on the conductor, it with many other facts shews that there is a resistance (considerable) to the discharge, but it shews also that time is an element, for it can compensate for that resistance and suffice for the discharge, even at much *lower intensities*.

2989. When the point electrified in the air, there is this peculiar and striking effect. Suppose it *positive*. The intensity will rise to a certain height, as 75° but continuing to work, it will fall to 70° or 65°, as if point discharged more freely. Then make the point *Neg.*: the intensity will rise to 65°, but continuing to work will fall to 60° or 55°. Again make it Pos: it will rise to 75° but gradually fall again and so on continually—as if, by continuing the discharge from a point, it became more able to effect that discharge—acquired an aptness for the purpose, but on the contrary became more inapt for the discharge of the opposite electricity.

2990. I thought I saw something of this kind also when using the rounded ends of the wires (2972).

29TH FEBY. 1836.

2991. Examined the light of Positive and Negative terminations by Wheatstone's revolving mirror.

2992. Ends i, ii and iii (   ) rendered Positive. Whilst they gave the brush, attended by sound, the light was resoluble by the revolving mirror into distinct discharges; but not more readily, I think, than when the eye alone was rapidly moved across them.

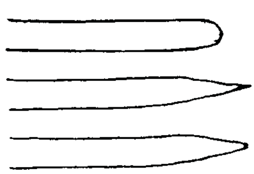
2993. When the glow was on these points, I could not resolve the light either by the eye or the mirror. In the mirror the circle of light formed was exceedingly faint.

2994. Point or end iv Pos. gave its small star of light always and this I could not resolve.

2995. I do not think that under the different forms of stars and brushes and glows from these different ends, the light is the same for the same discharged quantity of electricity. The point gives decidedly less than the other ends, and I think the glow less than the brushes.

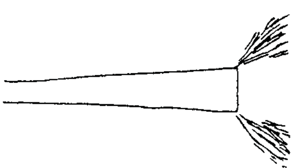
2996. When the ends i, ii and iii (   ) were rendered Negative, the brushes produced, though short, were always sonorous and resoluble; the glow could not be obtained.

2997. When end iv or the point was negative, I could not separate the star into distinct discharges. The light was so little that it was not visible in the revolving mirror—too much diluted to be sensible.

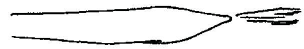


2998. *Wood.* Ends of wood were used. When these were positive, could obtain either brush or glow. The brush was resoluble, the glow not.

2999. When the wood ends were negative, the discharge was always as a brush, shortest and highest in sound with the finest end. The mirror resolved them much about as the moving eye did.



3000. *Paper.* Ends of card projecting beyond the brass conductor about two inches. The angles of the square end and the end of the pointed piece gave beautiful brushes when the charge was positive, especially if a large inducing surface opposite.



3001. When the paper or card was Negative, it gave a luminous edge extending a little way from the points or angles—it was rather difficult to obtain the negative brush; it was rather the elongated star, i.e. star extending along the edge of the card. Yet small brush could be obtained by management. If the end of the card were touched with the finger, then strong brushes covered the card, extending from finger to conductor; but these brushes originated rather at the finger rendered pos. by induction, the paper being then nearly an insulator.

3002. *Charcoal point* obtuse. Positive, a brush as usual at pos., sonorous—resoluble into successive discharges by the mirror and the eye, and I could not break it into the glow by this machine.

3003. Negative. A small singing brush as usual at Neg. surface, resoluble by the eye and mirror.

3004*. *Nitre—a* crystal fixed by tin foil on to the end of a long wire —about half an inch of crystal projecting rendered *Positive*; it conducts so badly that though there was a little brush from the end, the principal brushes were from edge of foil against the crystal.

3005. When the Nitre was rendered Negative, there was a small star at its extremity, but plenty of starry points of light round the edge of the foil against the nitre. The P. and N. differences of light just as in former cases.

3006. *Citric acid* arranged in the same way—conducts better than nitre. When *Positive*, brush—when *Neg.*, small brush or star.

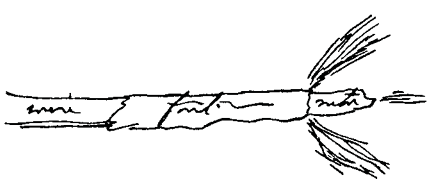
3007. *Oxalic acid* crystals in same way—conducts badly, Pos. brush from foil round it—Neg. star on foil round it.

3008. *Oxide of Lead—a* piece of fuzed prot oxide in same manner. Not conduct to produce light. Brush and star at edge of foil as before.

3009. *Chloride of lead*. Conducts fairly—all the brush or star at the end of it. When *Pos.*, short brush, sonorous and resoluble. When *Neg.*, star of light scarcely resoluble, but feebly sonorous.

3010. *Carb. Potassa—fuzed piece—Positive*, a long linear brush, singing sound, but hardly resoluble to eye or this mirror. When *Neg.*, star of light not resoluble, no audible sound.

* [3004]



3011. *Potassa fusa*—good conduction—Positive, a good brush. Neg., a small brush as with metal—both sonorous and both resoluble.

3012. *Strong solution of Potassa*, i.e. made end of Potassa fusa damp—results as before.

3013. *Strong Oil Vitriol* on end of wood. When Positive, obtain good brushes, but soon passes into glow. When Neg., small brush, star.

3014. *Sulphur*. Not conduct—the edge of the foil discharges.

3015. *Sulphuret of Antimony*—conducts well. *Positive*, it gives good brushes. Neg., small brush or star as with metal, etc.

3016. *Hæmatite—fibre* of—just as metal when rendered Pos. and Negative.

3017. It seems as if variation of the chemical nature of the Electrode made no difference as to the character of the P. and N. discharge into *Air*.

3018. Now experimented with a few edges to observe where the discharge occurs.

3019. *Knife edge*, a case knife, *Positive*. The brushes into the air were partial, i.e. existed here and there on the edge and not generally. They could be determined, in this or that place on the edge, by approaching the finger or other conductor. But it is clear that when one point of an edge is discharging the neighbouring parts are not discharging; that point overcomes the vicinity and determines something like an opposed state there, probably by the condition into which it brings the air by its discharge.

3020. Could easily obtain the glow on the extreme end of the knife and this glow extended a little way round the end, and was *not at a point*.

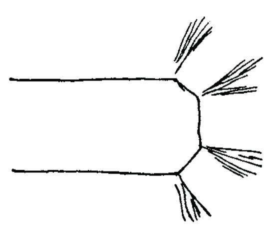
3021. When the knife *Neg.*, then starry points, but only here and there—no brushes.

3022*. *Tin foil* edge—Positive. Brushes from corner angles. But if finger brought opposite one, the others disappear more or less.

3023. Rendered *Neg.*—stars at angles. If finger opposite one the others weakened and even reduced to nothing.

3024. *Gold leaf*—became very tense and stretched when electrified

* [3022]



but still the star or glow very limited and only in few places. There only a discharge.

3025. But brushes obtained if a spark determined elsewhere in the course of the electricity—or probably if a piece of badly conducting matter intervene. *Good*.

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FARADAY'S DIARY

VOL. III

MICHAEL FARADAY

From the portrait by H.W. Pickersgill, R.A.,
presented to the Royal Institution by the artist, 1831

FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

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MICHAEL FARADAY
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and bequeathed by him to the
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Director of the Laboratory of the
Royal Institution

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MAY 26, 1836–NOV. 9, 1839

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FOLIO VOLUME III
OF MANUSCRIPT

(CONTINUED)

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3026. Have been making a few experiments this morning in relation to Professor Schcenhein's (of Basle) letter to me on the relation of Iron to Nitric acid and other metals, etc. The following is a summary of part of the facts.

3027. Using N.A. of S.G. 1·3 or 1·35, if a piece of clean iron wire is put in, there will probably be strong action, but on touching the iron with a piece of platina also dipping in the acid, the action quickly stops and the iron remains bright and unacted upon. The preserving power of platina is very distinct.

3028. Still, iron wire put into such N.A., though acted on at first, will often cease to act and then become bright and remains unchanged as if touched by platina. This should not be done in a shallow vessel as a dish, but a deep narrow one as a tube.

3029. Another piece of iron wire immersed alone will generally be acted upon, but if connected first with platina, or with iron wire quiescent in the N.A., and then introduced, it will not be acted on. The second piece brought into this state may be used to protect a third and that a fourth, etc.

3030. If a piece of common iron wire dipped into the acid and acting, *then* be put in contact with the quiescent Iron wire, the latter does not protect the former, but on the contrary, loses its state and becomes common acting iron.

3031. Wished to know what currents occurred at such times and what relation such iron had to other metals: also what other metals would have the same effect as platina on it.

3032. First took a Galvanometer and then, with a piece of Zinc and a piece of platina, noted the position of the needle, which of course is noting the direction of the current. In the rest of these notes I will compare the action of the iron to either that of the Zinc or the platina—these being standards of direction and power.

3033. Common iron connected by Galvanometer wire with Platina and then both immersed in the N.A. is at the first moments as *zinc* to the platina.

3034. If iron action goes on in the acid, the metal continues as *zinc* to the platina and the current is very strong.

3035. When the platina has stopped the action on the iron, then the current is also stopped or rather ceases and the iron is to the platina what *platina* would be to platina.

3036. If iron taken out of acid and held in the air until action on it of the adhering acid commences, then on its reimmersion in the N.A. it acts as zinc, but the moment the action on it stops the electric current stops also.

3037. When an inactive iron in the N.A. is touched in the acid by another fresh iron which acts, the latter sets the first in action also. But at the moment of contact (through the Galvanometer or otherwise) the first is as *platina* (strong) to the second. When both are acting then there is no current.

3038. If whilst the two last irons connected by the Galvanometer are active in the acid, either the one or the other is touched directly by a piece of platina in the N.A., then action of both is stopped, but still there is no current. The stopping or protecting action of platina is very striking.

3039. If iron be brought into a quiescent state by N.A. in a tube *without* the use of platina, it has the same relations as if platina were used. If in with fresh iron and there is action on latter at first moment, then there is a current and the prepared iron is as *platina* for the instant, but action immediately supervenes on it. If in with fresh Iron and there be no action on the latter, then there is no current. Iron rendered quiescent by itself in N.A. is then as platina to platina and there is no current.

3040. Gold has the same power over iron as platina has and the same habitudes and relations.

If the iron and the plat. or gold both be put into the Acid first and *then* connected, there is very powerful current at the first moment.

3041. Even silver will do the same thing with Iron as Gold and platina, but then a changing doubtful action comes on; for at some moments silver is as platina to the Iron and at others the iron is as platina to the silver and the currents are often rapidly reversed.

3042. When Iron is rendered inactive in N.A. and is then connected with Silver in N.A.—it acts as platina to the silver, and the silver tarnishes, and the current continues.

3043. When Silver stops Iron action in N.A., the current continues in the new order, i.e. the iron continues to act as platina.

3044. Copper acts in the same way, for it affects the action of iron and the final current when steadied is as if the Iron were platina.

3045. Very curious shoots of action came over the iron sometimes, especially when it was in association with silver, etc.

27 MAY 1836.

3046. Have re-examined the relation of copper to iron. It does not seem to affect its state and action. The constant current shews that the copper acts as platina—the iron as zinc—must be some mistake above.

3047. Iron and silver. When each are employed in the ordinary state, there is immediate action and a current, the silver being as platina—then the current suddenly changes and the iron acts as platina—then another change and silver is as platina—immediately after, the iron is again as platina and this will go on changing more or less frequently, until at last there is a constant current, the *iron* being as platina. This very curious and shews first silver action in stopping iron force, then reversion, as silver attracts oxygen and sends hydrogen to the iron, then renewal of the action of the iron and its assumption of the peculiar state, and so on.

3048. Made a mixture of equal parts sol. nit. copper and nitric acid. If ordinary iron put in, action and precipitation of copper. If inactive iron put in, a precipitation of the copper takes place and action generally goes on on the iron. If inactive iron in N.A. be first put into contact with platina, and then both transferred at once to the acid nit. of copper from the strong N.A. and kept so a while, the iron can be so treated as not to precipitate the copper or any action on the acid, and by care even the platina may be removed, leaving the iron alone and unable to act on the acid or precipitate the copper solution. Generally however, after a short time it starts into action, begins to precipitate copper and is corroded rapidly.

3049. A few more expts. on the Iron action.

3050. Supposing that the platina might take up more or less of that peculiar state which it assumes in association with zinc in dilute S.A., I tried platina which had stopped Iron and ordinary platina in the N.A. S.G. 1·35, but they were alike to each other.

3051. Iron rendered passive by platina was equally indifferent to that platina or to an ordinary piece.

3052. Iron oxidized in a flame, the same as platina to either of the former platina—no current.

3053. Iron heated so far from the end that though blue and yellow where heated, was not sensibly changed at the end, was still as platina to the platina. So that very thin film of oxide will do.

3054. If after iron is oxidized in flame a part of the surface be rubbed with sand paper—then on immersion in N.A. it is at the first moment as zinc to the platina.

3055. If at the place so oxidized a little be rubbed off and then the wire plunged into the N.A., the oxidized part protects the unoxidized part, as might be expected.

3056. Zinc is not protected by Platina in this N.A., but its action rather exalted.

3057. Iron with platina and also with silver in fuzed Nitrate of silver. There was no current or if sometimes a little, it was more often as if the Iron were platina to the silver as zinc.

3058. When the iron is thus introduced into fuzed Nitrate of silver, it oxidizes and even blues, as if it had been heated in the air, *exactly*. This a link in the chain of facts relating to the oxidizement.

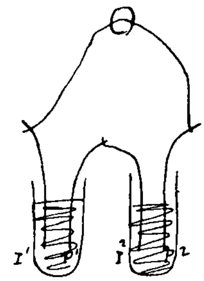
3059. The effect appears to be due to a superficial oxidizement or an association of oxygen and metal at the surface and is connected with the strength of the acid, etc. Oxidizement by heat must be a uniform and perfect superficial action and this looks very like the same.

3060. Consider it as due to a relation or affection of the surface just like the cases of affected platina. Ritter's explanation, Marianini, Daniell's, etc.

3061. Is related to my anomalous currents with Iron and the chloride and nitrate of silver.

3062. Is there any relation between it and the common mode of rusting? Preservation of Gun barrels when a continuous coat of oxide, by browning or bluing is put upon them.

3063. The action on the iron does not stop for want of conduction but want of chemical action, for on arranging two glasses with N.A. 1·35 thus, I^2 iron heated in flame and therefore oxidized, I^1 ordinary iron, P^1 and P^2 being ordinary platina—I^1 of course acted and on completing the circuit, there was a current with deflection of the needle, but gradually the current diminished and the action on I^1 also, and at last it stopped, I^1 having acquired the peculiar state.



3064. Then removed the oxidized Iron and made this prepared piece I^2, using again an ordinary piece at I^1; there was again action there and a current, but there was *no action* or solution of iron at I^2. It therefore conducted—it was not acted upon and the cessation of action when it and platina were alone in relation was due to the want of chemical action and not to the want of contact (metallic) or of conducting power.

3065. A series of three or four irons and platinas were put into action at once, all ordinary irons. There was a strong action and current; gradually the action ceased and the current fell off: some of the irons ceased to act, becoming bright in the acid; this again diminished the current and at last all the irons stopped. This was to be expected. Such a battery could not act.

3066. It is well that the electrolytic intensity of strong N.A. is so high that there is conducting power enough remaining, when there is no decomposition in one glass, to shew that going on in another.

3067. Heated a piece of iron wire so as to blue it all over and then put it into N.A. 1·35, so as to be altogether immersed. There was no iron in the acid dissolved. Bound another piece of iron wire in contact with a piece of platina wire, brought it into the peculiar state, and then put it into fresh pure N.A., leaving the platina in association with it. These two were left at about 1 o'clk.

3597. Experimented on inductive power of glass. Made a Singer's electrometer electric—the top terminating with a broad plate—then brought another plate near the upper—sometimes air intervening and sometimes also a plate of crown glass. When the glass was there, the electrometer collapsed more than when away, as if induction *more* facil thrgh. glass than air. But this was found to be due to the conducting power of the glass (though it was warm). For in the first place, when the glass was there, then the approach of the inducing plate made no difference, so that induction was evidently not through the glass—and on the other hand, when the plate of metal was taken away, the mere approach and recession of the glass itself made a difference in the divergence of the leaves, approach diminishing the divergence and recession increasing it. In fact, through the conducting power of the glass, the part opposite the upper plate of the electrometer had assumed the opposite state, and hence acted by induction as a conductor itself would do. I must cover the plate with a coat of shell lac.

3RD DECR. 1836.

3598. Have lately made a Coulomb's Electrometer (torsion) from his own directions, and like the instrument very much. The insulation is very good and with repulsions equivalent to forces expressed by the angles of 30°, 40°, 50° or more will hold the charge for 20′ with little sensible diminution. Even when the repulsions indicate 600° or 700°, the loss of forces is very slow.

3599. I have used for the torsion a fine glass thread, according to Ritchie, and find it excellent. With a torsion of 1000° or more, it returns perfectly; there is of course no set.

3600. The first glass thread I used was not fine enough and the lever and pith ball [vibrated] once in 2½ beats of my watch, I then used a silk worm thread, but that was too delicate and appeared to take a set—for instance, when I carried the Index of torsion round Six times, the lever suspended to the thread went round only twice. Sometimes for a single revolution of the Index the pith ball would not move at all. This was probably due to a very feeble electric state in the ball and the want of perfect regularity in the roundness of the glass jar, so that in one position the pith ball was nearer the side than in another, and then would be attracted. The suspension thread was about 2 feet long.

3601. I then used a second thread of glass, very fine, and it now remains attached as the thread for service. It is so fine that the time of vibration of the suspended needle is 13 beats of my watch (140 beats = one minute).

20 DECR. 1836.

3602. Have for some time past been engaged in preparing plates of shell lac, etc. for experiments on induction. Easily cast square and round plates, and then soldered in little loops of white silk into them at various places at the edges, so as to allow of future suspension by long threads of white silk.

3603. I found it exceedingly difficult to free these plates from all electricity, so that they might be used as interposed media in cases of induction without exerting any influence by their own electric state. Handling made them electric—wiping with a dry cloth made them electric—with metal, etc. the same. Wrapping them up in tin foil or paper for days or weeks did not remove this electricity, so perfectly does this body insulate or non-conduct.

3604. The only process I found to succeed was to wash two cloths well in distilled water, wring them as dry as I could, and then, whilst holding the shell lac in one of the wet cloths, wipe it carefully over in every part with the other, and at last deliver the plate into the air, without touching it with any thing else. It is of course dry. immediately. It is then free from electricity, and to prove that no film of conducting or interfering matter has been left by the cloth, a mere touch with the finger in any part will render it electric, and that electric state it will retain for hours together undiminished.

3605. All these conditions of the plate, etc. are exceedingly well shewn and examined by means of the Coulomb's electrometer.

3606*. *a*, a plate of brass 4½ inches in diameter, insulated on stem of shell lac and connected by a wire 5 inches long with the brass ball *c* half an inch in diameter—*b,* a similar plate of copper opposite *a*, distant 1¼ inches from it, and connected with the ground—therefore uninsulated—d, a square plate of tin foil 6½ inches in the side, suspended by white silk so that it could be brought between *a* and *b* or removed at pleasure.

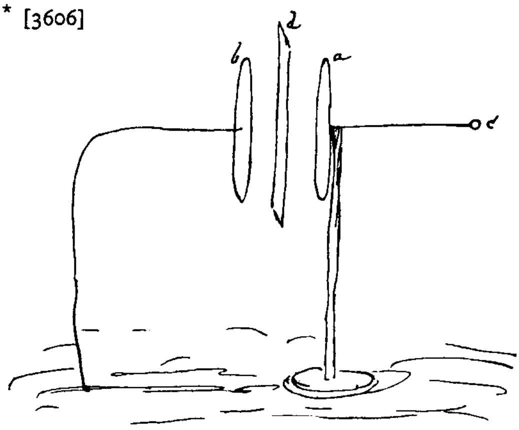
3607. *a, c* was then electrified negatively and the ball in Coulomb's balance was also electrified negatively. Then a pith ball at the end of a stem of shell lac was brought to *c*, made to touch it, introduced into the balance and the repulsion observed. Now whether *d* was between *a* and *b* or not, the intensity at *c* was not much affected by it, though *d* was attracted and moved, but not allowed to touch *a*. The effects were less than I expected, but were of no consequence to the general question of specific induction.

21 DECR. 1836.

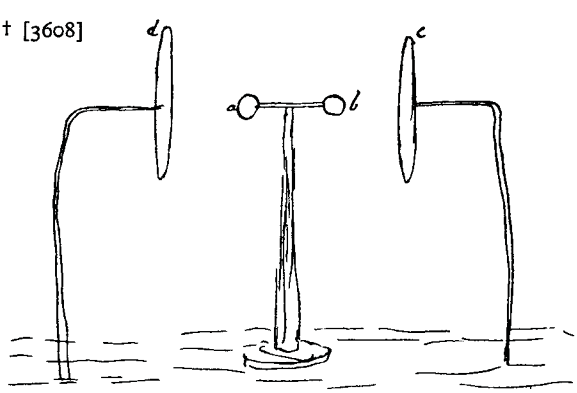
3608†. Experimented carefully in a case of comparitive induction between Air and shell lac. *ab* a small conductor about 4 inches long, consisting of a thick short wire terminated by two similar brass balls 9/10 of an inch in diameter each. The conductor was insulated on a pillar of solid shell lac—c and *d* two metal plates each 4½, inches in diameter, placed in similar positions opposite the two balls *a* and *b,* and connected with the ground; the distance between the balls and the plates was nearly 2 inches. A square plate of shell lac 7/10 of an inch thick and 5 inches square was then suspended by long white silk threads so that it could hang between *a* and *d* without touching either, or be drawn away to a distance without altering the relative distance of the metallic parts.

3609. Then *a b* was electrified Negatively and of course acted by induction through the air towards *c* and *d*, and in an exactly similar manner. The carrier pith ball of the Coulomb was two or three times brought in contact with *a* and *b,* and with the

* [3606]



† [3608]



repelled or measuring ball in the Balance of torsion, so that the latter was fairly and similarly electrified. After which, observations to be recorded were made thus. The Carrier ball was brought to the extreme end of the conductor at a or *b,* then introduced into the Coulomb's balance and the divergence or repulsion of the two balls measured; and this was done successively with the ends *a* and *b,* so as to yield a correct average, sometimes with the plate of shell lac in between *a* and *c* and sometimes away.

3610*. Without shell lac and with air at each end of *a* and *b,* the repulsion was nearly 36° at either extremity.

i **3611.** Shell lac introduced between *a* and *d.*

		b . .	35°	
	37°·5 . .	*a*		
		b . .	33°·7	Average of *b*, 34°·3
Average of *a*, 36·97	37·0 . .	*a*		
		b . .	34°·2	
	36°·4 . .	*a*		

ii **3612.** Now the shell lac plate removed

		b . .	34·2	
	34·2 . .	*a*		
		b . .	33·8	Average of *b*, 34°
Average of *a*, 34°·06	34 . . .	*a*		
		b . .	34°	
	34 . . .	*a*		

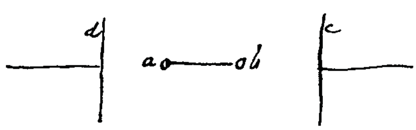
iii **3613.** Shell lac between *a* and *d* again

		b . .	33	
	34·7 . .	*a*		
		b . .	32·5	Average of *b*, 32·66
Average of a, 34·7	34·7 . .	*a*		
		b . .	32·5	
	34·7 . .	*a*		

iv **3614.** I now advanced the metal plate *d* nearer to *a*, indeed close up to the plate of shell lac, which was still in place.

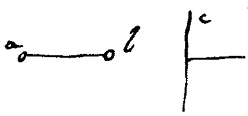
	35·3 . .	*a*		
		b . .	32	
Average of *a*, 35·33	35·5 . .	*a*		
		b . .	31	Average of *b*, 31·5
	35 . . .	*a*		
		b . .	31·5	

* [3610]



v **3615.** Now took away the metal plate *d* altogether, leaving the shell lac plate opposite *a* as it was.

Average of *a*, 32°	33 . . *a*			
		b . . 31·5		Average of *b*, 31·5
	32 . . *a*			
		b . . 31·5		
	32 . . *a*			
		b . . 31·5		



vi **3616.** Now took away the shell lac plate also.

Average of *a*, 30·33	31 . . *a*		
		b . . 31	Average of *b*, 30·86
	30 . . *a*		
		b . . 30·8	
	30 . . *a*		
		b . . 30·8	

vii **3617.** Brought the plate *c* to within 1/2 an inch of ball *b*.

Average of *a*, 28	28 . . *a*		
		b . . 33·5	Average of *b*, 33·5
	28 . . *a*		
		b . . 33·5	
	28 . . *a*		
		b . . 33·5	

viii **3618.** Put the plate of shell lac close up to *a*.

b . . 32.	Average of *b*, 32°.
b . . 32	
b . . 32	

ix **3619.** Took the shell lac away from *a*.

b . . 32·5	Average of *b*, 32·83
b . . 33	
b . . 33	

3620. It is evident from vi that the plates *c, d* had not done much by their presence in modifying the general induction at *a* and *b,* as they differ so little in their intensity when one is away. At vii, however, the inductive effect of the plate is evident, and in that state the approach of the shell lac is a little to increase the intensity at the air inductive surface at *b,* as if induction were somewhat more difficult from *a* through shell lac than through air. But this is the reverse of the action in i and iii, which seems to indicate

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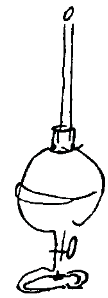
THE SPECIFIC INDUCTIVE CAPACITY APPARATUS
The apparatus of metallic spheres used by Faraday (see par. 3622). The photograph shows the outer sphere in two parts, with examples of the dielectric fillings and the boxwood mould used for preparing them.

that induction is more facil through *lac* than *air*. The effects however were very small in the difference and probably accidental, and indeed v and vi would seem to shew this, for they indicate no difference whether shell lac or air was next the electrified conducter.

3621. These results seem to shew that induction is alike forcible in both.

23RD DECR. 1836.

3622. Have had two apparatus made for Induction through air and liquids, in which an inner and an outer ball are kept at fixed distances and serve as coatings of Leyden Phials, air being between. They are numbered i and ii and are in all general respects alike. The insulation of the Inner ball is by a metallic wire enclosed in a glass tube, the latter being covered well over with lacquer.



3623. Tried these by charging them alike with Neg. electricity and charging also the indicating ball of Coulomb's electrometer.

	No. i		No. ii	
At 9^h. 30′. .	39°	divergence	39°	divergence.
9 46 . .	25	„	33	„
10 10 . .	18°	„	29	„

In these experiments the repelling ball was brought against the upper ball of the two pieces of apparatus and then its repulsion on the indicating ball observed. As the indicating ball would have stood naturally at Zero, the 39° or other quantity not only shews the degrees of torsion overcome but also the *distance* at which that force was overcome.

3624. It is evident from the above experiment, that both i and ii lost electricity rather fast, and No. i most rapidly.

3625. Now rewarmed and revarnished several times over the part of the glass within the great ball, and kept the whole hot for the day to dry the varnish or lacquer *well*. (Pure lacquer was used.) By the close of the day, the glass was well covered with a hard firm coat of shell lac.

3626. Renewed the trial of Yesterday, without previously warming or drying the apparatus.

	Time	i	ii
	10h. 18′	47°	47°
In 0h. 23′ or at	10h. 41′	43°	44°
1h. 5′ „	11h. 23′	39°	41°
1h. 42′ „	12h. 0′	37°	38°
2h. 32′ „	12h. 50′	34°	35°
3h. 40′ „	1h. 58′	30°	31°
4h. 10′ „	2h. 28′	28°·5	29°
4h. 57′ „	3h. 15′	25°	27°
6h. 30′ „	4h. 48′	22°	22°·5
7h. 22′ „	5h. 40′	20°	21°

3627. Here the loss is progressive, and about the same in both, and is also about the same with that of the loss from the balls in the Electrometer. The apparatus will now do.

3628. The Coulomb cannot be trusted except when the balls are nearly equally electrified, or at least not when they are very different. Thus the two balls were both neg. but one weaker than the other: the repulsion was 17°·5; on making them touch so as to equalize their charge, the repulsion was 21°, *with the same electricity*.

29TH DECR. 1836.

3629. Have readjusted the balls and marks of the Coulomb's balance so as to bring all into their places accurately and mark off divergences accurately.

3630. Have also been experimenting with the two ball induction apparatus, but resolved to measure off all the torsion force by the circle above. Therefore adjusted the *repelled* ball so as to stand at 20° in the Electrometer, when the Index above at Zero; then always made the distance from the carrying or *repelling* ball 20° and measured off the force of torsion above.

3631. Charged App. i Negatively; it gave in succession

580
540
488—hence looses rather fast with these high charges. Now divided the charge with App. ii and tried each in succession.

i		ii
255	. . .	233
247	. . .	230
249	. . .	221

These are near enough to shew that the charge is divided and that the sum of the two forces is equivalent to the original force. But seems to indicate, either that ii loses faster than i, or that some cause exists in perhaps both, which lets down the charge for the first moment or two after it is given rather faster than by mere conduction.

3632. Then discharged i entirely, tried ii and found it on two trials

ii
192°
195°

Divided the charge with i and found each as below:

i		ii
106	. . .	106
104	. . .	105

So that now both nearly equal, but the whole amount of action rather too much, as if a kind of hidden charge had been preserved by App. i. Perhaps it is the taking up of this condition of the electrics, etc. which made the loss before and now makes the advantage. But the general results, that the two apparatus nearly divide the power and give good account of it afterwards is very distinct.

3633. Again,

App. i charged, not so highly:

i

180°

167°

170°—then the charged divided with ii; the indications then were

i		ii
84	. . .	78
88	. . .	73

To remove the effect of the higher charge of the repelled ball than that of the repelling, brought both into contact; then

i		ii
69°	. . .	58°
65°	. . .	57°

3634. So evidently the two not fairly charged from each other, for having been in contact outside, they must of necessity present equal influence in the manner they are tried; although it was possible that specific electric induction might have made the mean less than half the original quantity.

3635. Attached App. i to the Air pump. Then electrified ii Neg., tested it and found its force = 170°; connected with i, the air within the latter being rarefied so as to sustain about 12 inches of mercury (Bar. 30·2), then:

i		ii		
rarefied		as usual		
119	. . .	118	sum	237
114	. . .	115	„	229

This would seem as if rare air had less capacity for Electric induction than dense, but suspect some mistake about first estimate.

3636. Made this expt.

i with common air, charged to	. . .	118° of force
rarefied till pressure within 3·2 in	. .	104 „ „
Air let in again, was	. . . .	104 „ „
Rarefied again to 3·5	. . . .	104 „ „

3637. Here the first rarefaction had caused discharge, but after that was attained, dense or rare was the same, and this a good form of experiment.

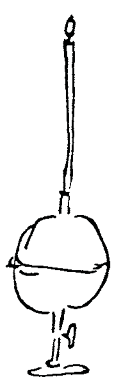
3638. Charged No. i on the Air pump—exhausted it until pressure within was 1 inch mercury—discharge and the force fell to 22°, at distance (always) of 20°. Restored the full pressure of the atmosphere within and the force was still 22°, the same as before.

3639. Hence dense or rare air causes no variation of the inductive force or capability.

3640. Repeated the last experiment. At pressure within app. ii of 1·2 inches mercury, the charge was = to 45° of force of torsion. When Air let in to pressure of 30·2, the force was 43°. This, allowing for discharge by stem, may be considered as identical.

3641. Again no change for change in density of the Air.

3 JANY. 1837.



3642. Experimented with Ox. and Hy. in the two apparatus marked i and ii. Have adjusted so that when Index of torsion was at Zero, the repelled ball stood at 20° of graduation on glass. Then always brought the distance to 20°, and so measured off the whole force of torsion above.

3643. Filled i, *Oxygen*—ii *with Hydrogen.*

Charged i Neg. It then gave, in the torsion electrometer

240	losing fast, apparently by conduction.
214	
203	
190	

Touched ii with it, so as to divide the Electric charge; then

i	ii	
i = 101° . .	= ii	
	. . 88°	only. Made contact again, then
	. . 88	
91° . .		
	. . 84	
88 . .		
	. . 74	as ii, so inferior; again made contact
	. . 73	
84 . .		again contact
	. . 81	
78 . .		

It is evident that at the first moment after touching,
3644. i and ii ought to give the same result as to the specimen taken from their outer balls, and the great difference would seem to imply that a part within (perhaps the inside of the tube carrying the wire) takes up the charge slowly, and again gives it slowly as the charge is removed. This would account for the difference between the two after contact provided, as was the case, a little while intervened after contact before the trials were made. For supposing them, at the first moment after contact, to be equal externally to the air round the upper ball: i would have its charge raised a little by the return of the electricity spoken of, and ii would have its charge reduced below the mean by the disposal of a portion of that given to it in the same manner.

3645. Either this or some other circumstance must interfere to cause the difference of intensity in the charge of the two.

3646. It is worth observing that the sum of the two is very nearly the sum of the power originally in i, just before the division. Which seems to shew that Oxygen and Hydrogen are alike in force and amount of Electric induction through them.

3647. Now charged ii containing the Hydrogen Neg. and then divided it with i or Oxygen. Both the oxygen and Hydrogen were dried before they entered the apparatus, and each apparatus was exhausted and filled twice with Gas.

3648. ii, Hydrogen; charged Neg.

188 }
178 } rapid falling.
168 }

Then divided with i.

Oxygen, i.		ii, Hydrogen.
	. .	96
90	. .	
	. .	89
85	. .	
	. .	94
79	. .	
	. .	92

3649. Here the sum of power in i and ii being 186 is so much greater than 168, the whole sum when in ii, that it would seem to intimate that Oxygen had less capacity for inductive action than hydrogen, i.e. that half the electricity from ii did not pass into i. But must see how this turns out in other cases.

3650. Again, ii Hydrogen charged Neg.

158
155
147

Then divided with i, Oxygen

	. .	78	
69	. .		touched the two together
	. .	74	
72	. .		

Here the sum of separate forces the same as the first undivided force, as if oxygen and Hydrogen were alike in their inductive capacity.

3651. Experimented to ascertain whether rarefying oxygen would change its capacity.

	i, Oxygen was charged N.	= 68°
Put on the Air pump and Oxygen remd. until	= to 18 inches Mer.	= 65°
further rarefaction until atmosphere within support[s]	15 „ „	= 67°
„ „ „ „ „	6·4 „ „	= 62°
„ „ „ „ „	2·4 „ „	= 66°
„ „ „ „ „	1·4 „ „	= 58°
„ „ „ „ „	1·4 „ „	= 58°
then common air let in to full pressure of	30·4 „ „	= 60°

3652. Hence no appearance of difference by rarefaction, except that when brought down to 1·4 inches mery., it could not retain the 66° but fell to 58°. The other variations I conclude to be accidental.

3653. Now compared air and hydrogen: i, *Air;* ii, *Hydrogen*. ii was charged Neg.

		325°
		314°
		316°

Then divided the charge with i.

	. .	215
216	.	

The sum of 429 is so much greater than 316 that I think there must have been a charge left by accident in No. i; or else hydrogen has far greater capacity than air.

3654. Began again, previously discharging both; then charged ii Neg. Was

		370°
		340°
	divided	
	. .	200
192	. .	
	. .	210

Here again the same result as before, and very striking, the amount of difference being very great.

3655. Again, ii, Hydrogen charged Neg.

		165
		158
		157
	divided	
	. .	80
79	. .	
	. .	82
75	. .	

Here the half sums are equal to the whole sums, but the first intensity was much lower, and that may make a difference.

3656. So now took a higher charge for No. ii, in the outset making it Neg.

		283 for ii, Hydrogen
		250
	divided	
	. .	137
125	. .	
	. .	135
124	. .	

4625. Graham has lent me Thilorier's Carb. acid apparatus and I have been experimenting with it in cooling metals and ascertaining their relation to Magnetism. The metals were in pieces, kept a while in the mixture of Ether and C.A. with solid C.A. in it, and moved either by platina wires or by wooden tongs. The temperature according to Thilorier would be about 112° below 0° of Fahrenheit.

4626. Antimony
Arsenic
Bismuth
Cadmium
Chromium
Cobalt
Copper
Gold
Lead
Palladium
Platinum
Rhodium
Silver
Zinc

These gave no deflection by a double astatic needle that was exceedingly sensible to the smallest particle of Iron or nickel.

Great care was taken to avoid the effect of a downward current of air at the side of the cooled metal.

4627. *Iron* and *Nickel* seemed as magnetic at the lowest degree as at common temperatures. *Carbon* (dense) from a gas retort was not rendered magnetic.

4628. Native *Iridium* and *Osmium*—and crystals of *Titanium*—were a little magnetic at common temperature, I believe from the presence of a little iron. They were not more so when cooled to—112°, so that the metals Iridium, Osmium and Titanium do not seem magnetisable at that temperature.

4629. Metallic Manganese from Mr Everitt. Slightly magnetic and polar at common temperatures—not more so when cooled—must now examine its purity.

4630. Then tried various metallic combinations, for which see the next page[1]. Thus—

4631. Hæmatite
Crystd. Green Vitriol
Dry Green vitriol
} did not acquire any magnetic power.

4632. Galena
Realgar
Orpiment
Dense Native Cinnabar
Sulphuret of silver
„ „ copper
„ „ Tin
„ „ Bismuth
„ „ Antimony
} Gave no signs of Magnetism.

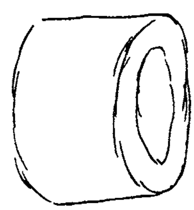
4633. Chloride of Silver
„ „ lead
} fuzed into lumps. Gave no signs of magnetic force.
Iodide of Mercury

4634. Fuzed protoxide of lead
„ „ Antimony
„ „ bismuth
„ White Arsenic
Native oxide of tin
„ „ Manganese
} No signs of Magnetic force.

4635. All the facts were negative. This is important as a correction respecting Cobalt and probably Manganese.

5 JUNE 1838.

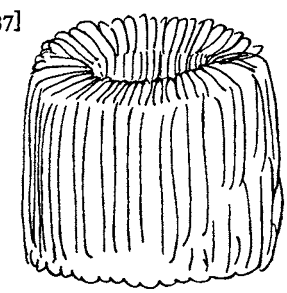
4636. Experimented with the view of finding some action on insulating dielectrics analagous to the currents by induction produced in conducting bodies.



4637. A ring of iron in form of a cylinder about 2 inches high, 2 inches external diameter and 1¼ inch internal diameter had 80 feet of silked copper wire rolled onto it as in the figure*. It may be called ring I. Another ring, of wood, of equal size, had as

[1] Immediately below.

* [4637]



much as 162 feet of covered wire rolled in a similar way on to it, and may be called ring W.

4638*. Cores of lead, shell lac and sulphur were cast, being cylinders about 2½ inches long and 1 inch thick, which could be placed in these rings at pleasure. The follng. arrangement was made. I the iron ring; L a lead core, touched at the ends by wires going away to a delicate galvanometer G, of which the needle moved in the direction of the dotted line when the current was as indicated by the arrow R. V is a voltaic battery of 10 prs. of 4 inch plates, Wollaston's double coppers, well excited, and P is the wire from the Pos. end; N the wire from the Neg. end.

4639. When contact was made at NA and BP, at the moment of completion the needle moved towards the dotted position. When contact was broken it moved the reverse way.

When the lead core was in a wrapping of paper, to make insulation more perfect, the effect was the same.

4640. When a mere copper wire was used in place of the lead core, the effect was the same.

4641. When the wooden ring W was used, the same effect was produced but in a smaller degree.

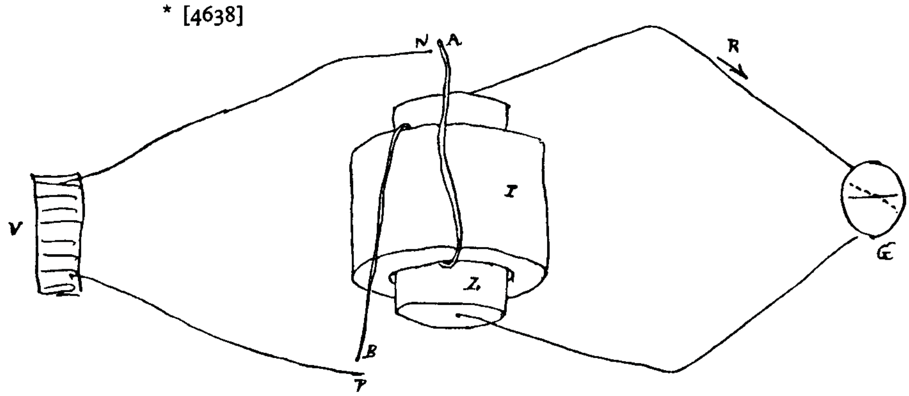
4642. Now used a core of shell lac, having small discs of tin foil pasted on the ends, and to my surprize obtained effects on making and breaking contact, smaller in degree but also in the reverse direction to that given when conductors were used.

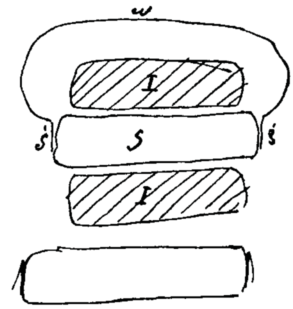
4643. Used a sulphur core with the same effect.

4644. Removed the sulphur and allowed only air as the core; still the same effect.

4645. But suspecting that the iron ring used in these cases might have a little unneutralized magnetism, I removed the wires connecting the galvanometer and the core, and then found that the same effect was produced. So that it was merely a little magnetism acting on the galvanometer needle, and not any effect of a current passing through its wires.

* [4638]





4646. Made the follng. arrangement. I, I the section of the iron ring; S the shell lac core; *w* a copper wire having 2 pieces of gold leaf hanging at *g, g* close to the ends of the core. On making and breaking contact of the ring wire with the galvanic battery, *no effect* on these gold leaves could be observed.

4647. Then suspended little strips on the tin foil coating the ends of the shell lac, but on making and breaking contact, no indication of repulsion could be observed. No signs that the dielectric became for the moment excited in a way to correspond with the formation of a current in the conducting core.

4648. So I cannot thus find any action on insulating bodies analagous to those which occur in conducting bodies: no transverse induction.

7 JUNE 1838.

4649. I was using the double plate Electrical machine and large Leyden battery to-day for the deflagration of some wires, and observed the following remarkable phenomenon.

4650. The battery was charged as highly as it would bear, and on being discharged through the wire, fell at once and *perfectly,* generally speaking. But on one occasion it was left with the electrometer divergent, and on Anderson proceeding to bring the charge up to a full degree, he called to me after eleven turns of the machine to say that the battery would not charge; the electrometer was down.

4651. I had during this time been engaged with my head near and thought it at first just possible that the points of the hair might have kept the charge down, but could not understand it.

4652. Continuing my experiment, the battery was again left charged, and the electrometer divergent after a discharge, and now I watched things; and, telling Anderson to work the machine, as he did so, the electrometer fell. It took nine turns of the machine to *reduce* the charge left in the battery to nought.

4653. Thus the battery had been left charged *negative* after these two discharges, and to an amount equal to 9 and 11 turns of the machine, from 20 to 25 turns giving a full charge.

4654. This effect most important and curious, and I think I see the cause, so as to be able to produce it at pleasure.

4655. Have made many explosions to-day with wires, etc. but have not obtained any thing like a residual *Neg. charge,* though I used long wires, helices, cores, etc. Often obtained a residuum of one half or two thirds of the charge, when very fine wires were used, and also several other interesting indications.

14 JUNE 1838.

4656. Have made many more explosions of fine wires, but did not obtain any result like that noted on the 7th instant.

21 JUNE 1838.

4657. Experimented on the effect of Polar arrangement of the particles of crystals on their specific inductive capacity, i.e. on their inductive capacity in different directions.

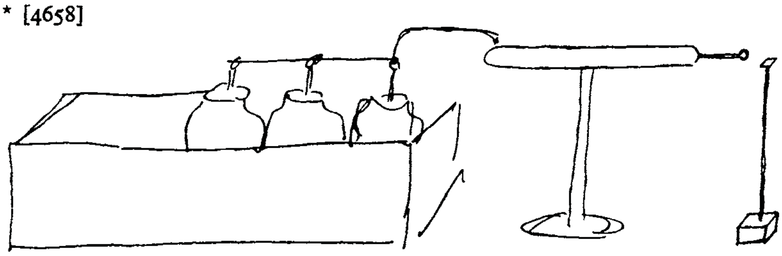
4658*. Arranged our large battery of 15 Leyden jars and connected it with the insulated brass conductor, at the further end of which was a rod terminating in a brass ball _____ of an inch in diameter. This ball was to be the inductric object and to have its power sustained by the battery connected with it.

4659. The carrier ball of the Coulomb Electrometer was the inducteous body.

4660. A stem of shell lac, fixed on a foot, had a little plate of shell lac at the top which served to sustain cubes, etc. of crystallized matter. The stem was _____ inches in length and only so thick as to have sufficient strength to support the cubes. It was so flexible that slight force was sufficient to bend it a little in any direction.

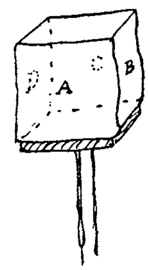
4661. When a cube was to be examined, the battery was first charged by 8 or 10 turns of the cylinder machine. Then the carrier ball was rendered neg. inducteously and used to charge the repelled ball of the Electrometer. Then the mutual repulsion of the balls was noticed. Then a cube was put upon the shell lac support and adjusted so that a horizontal line pass thgh. the middle of it and of the inductric ball at the end of the conductor. Then the cube was approached close to this ball, the carrier ball of the electrometer brought against the opposite face of the cube, the carrier ball uninsulated by a wire applied at the back in the same straight line—uninsulated—removed—the cube withdrawn 4 or 5

* [4658]



inches, and the carrier introduced into the Electrometer and its force measured at the constant angular distance of 30° as before. The following* is the arrangement of the inductric ball *a*; the cube *b*; the carrier ball *c*; and the discharging wire *d*.

4662. The first substance experimented with was iceland spar. I have had a cube of this substance cut; it is very clear, has no flaws or fissures, but there is a thin stratum of spar in another direction across it at one edge. It is only 0·5 of an inch in the side, being the largest that could be cut out of a considerable rhomboid. Two of the faces are perpendicular to the axis of the crystal, the other four faces are parallel to it. It is not varnished at present but in its natural state.



4663. This cube was put upon the shell lac stand so that the axis of the crystal was horizontal, as for instance in the direction from A to C. It is evident that each of the faces A, B, C, D could in turn be placed towards the Inductric ball and the carrier at the same time applied to the opposite face, the cube itself being the measure of the distance between the inductric and inducteous surfaces, and *its matter* the dielectric between.

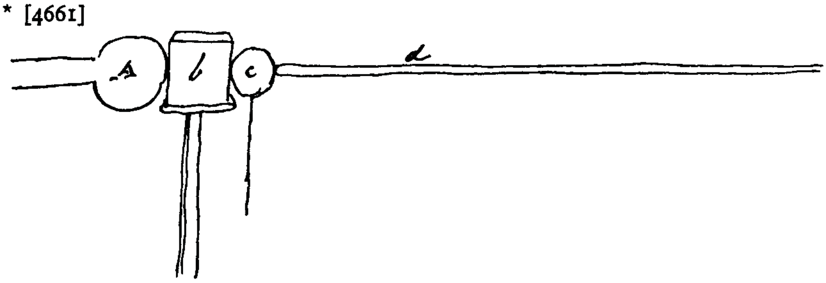
4664. As before said, the process was to approach a face of the cube into contact with the inductric ball, bring the carrier ball against the opposite face in a constant position and direction, uninsulate it, insulate it, remove it; then remove the cube a few inches, and measure the force of the carrier. Then turning the shell lac stand 90°, another observation was made through the cube. Again the shell lac stand was turned 90° and the operation repeated, and also a fourth time, until observations had been made in four directions, twice along and twice across the axis of the crystal.

The balls in the Electrometer were 178°, Neg. charge.

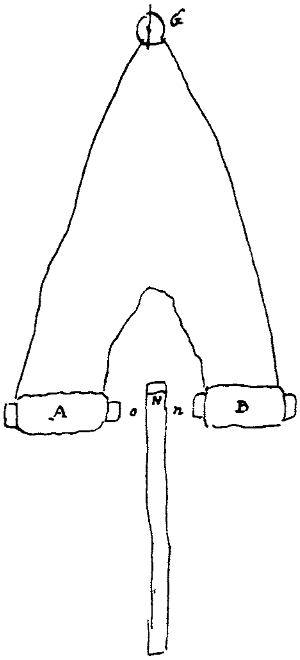
1.	Force of the carrier—lines of induction				along the crystal axis			110°
2.	„	„	„	„	across	„	„	150°
3.	„	„	„	„	along	„	„	180°
4.	„	„	„	„	across	„	„	178°

After the last removal of the cube, it was examined as to its own state, and I found it was electrified, having received a charge like that of the Inductric surface. Hence the rise in force from

* [4661]

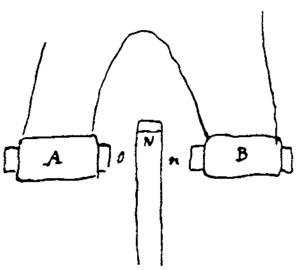


4886. Expts. on interposition of different dielectrics across the course of the Magnetic curves.



4887. A and B are two helices with iron cores, N the north pole of a bar magnet placed between, G is a galvanometer—very delicate. A and B are so arranged that as the magnet moves from one to the other, by a little lateral displacement, the currents formed in them shall coincide, i.e. that as it moves from B and towards A, the current occasioned by the falling of the mag. forced in B and that occasioned by the rising of the Mag. force in A shall accord and join in their effect to influence the galvanometer needle.

4888. A pendulum was arranged to vibrate isochronously with the needle of the galvanometer. The interval between the Magnetic bar N and each of the iron cores was about 1½ inches. The motion of the mag. pole isochronously with this pendulum and to not more than the eight[h] of an inch to and fro between the two helices, i.e. from one towards the other and back again, quickly deflected the Galvanometer needle considerably. Hence but little weakening of the effect on one helix, conjoined with a little strengthening on the other, was quite sufficient to produce a sensible effect.



4889. Had prepared plates of different substances for the purpose of being introduced between the Magnetic pole and the ends of the iron cores in the helices. For instance, the pendulum was set swinging—then a copper plate introduced at *n* without touching N or B, and kept there for the time during which the galvanometer needle would swing one way. It was then withdrawn for the time due to the swing the other way, introduced for an equal time, withdrawn again and so on, in hopes to accumulate any effect at the galvanometer needle.

4890. Sometimes two plates were used, one being introduced at *o* whilst that at *n* was out, and then taken away whilst that at *n* was in and so forth. Such was the general process of trial.

4891. *Copper.* Whether one or two plates were used, each being 0·7 of an inch thick, there was *no* effect at Galvanometer.

4892. *Shell lac.* One or two plates, 0·9 of inch thick, produced *no* action. Two wedge shaped plates were used to gradually change the effect of introduction; still *no action.*

4893. *Sulphur.* A plate 0·9 of inch thick; *no* action.

4894. A plate of *iron,* though thin, caused great action; a small piece, as a key, also caused action readily. This was by diverting part of the Mag. force of the bar from off the helix cores, but still shews the sensibility of the apparatus to the other kind of action sought for.

4895. It is possible, however, that different results may be obtained when the iron cores are away—for the extra action of iron and its relation to the magnet may over rule any effect due to the variation of the force of induction of mere currents. This will be settled when simple helices shall be substituted for the core helices A and B.

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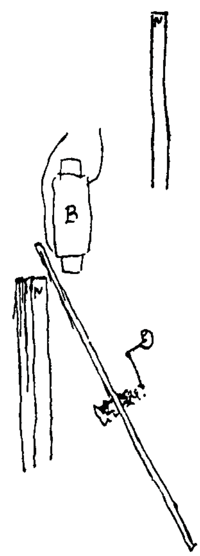
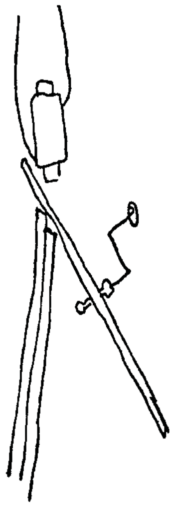
4896. Arranged the Magnet N and magneto-electric helix B thus —the Galvanometer being unaffected by moderate motions of the magnet. The arrangement was such that when the N. pole of the magnet approached the end of the core in B, the north end of the needle in the galvanometer moved westwards, and when magnet receeded from the core, the motion of that end of the needle was towards the east.



4897. Now introduced the edge of a copper disc 1/5 of an inch thick between the pole and the core.

4898. Set the pendulum (   ) vibrating. Then revolved the copper disc quickly during the time occupied by the needle (when in motion) to pass in one direction—stopped the rotation for a time equal to its return—renewed it for an equal portion of time—stopped it again for an equal portion of time, and so on, for 8 or 10 alternations or more. Then examined the Galvanometer needle.

4899. At first I obtained motions in the needle corresponding to the rotation of the copper disc, but by investigation I discovered this was due to the effect of the pressure required to hold it steady whilst revolving, causing a slight bending of the table, and so altering the distance of the pole and the core in a minute degree. As this happened regularly with the vibration of the pendulum, it soon produced a sensible effect.



4900. In fact, giving the same pressure at regular periods, *without* rotating the disc, produced the deflection of the needle.

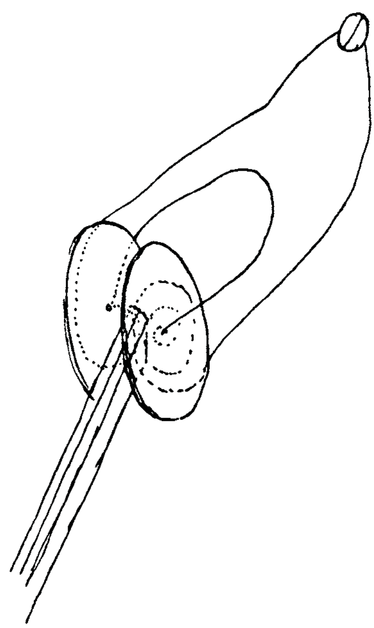
4901. This shews the delicacy of the arrangement; nevertheless it was not able to shew any effect due to the rotating copper, whether movg. one way or the other.

4902. Then put the copper plate oblique to the direction of the magnetic curves thus, to ascertain whether that would make a difference, but still *no effect* was produced.

4903. *Shell lac* disc 0·6 of inch in thickness, rotated in the same manners, produced *no effect*.

4904. Now used flat helices instead of the previous long helices and cores. This was for the purpose of removing the action of iron as regarded the helices. Each helix was of silked copper wire arranged on a cardboard and containing _____ feet of wire. They were supported by feet and arranged thus*. The north pole of a magnetic bar was between them and, as they were four inches or more apart, it could be moved between them or they towards it. When the pole was withdrawn from or introduced between the flat helices equally to each, or when they were moved together from or towards the pole equally, there was no current, the tendency in the helices being contrary to each other. But when the mag. effect on one side was weakened whilst that of the other was strengthened, then the currents produced in both helices combined to influence the galvanometer. So if the pole were moved *from* one helix *towards* the other, a strong current was at once produced. Or when the helices, always kept at the same

* [4904]



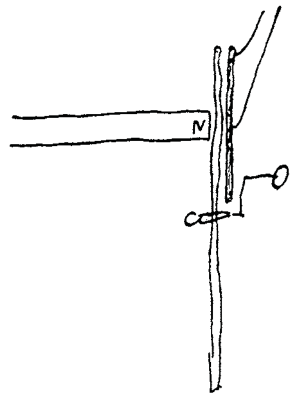
distance from each other, were moved together to the right or left, then a strong current was produced.

4905. *Shell lac.* Then used two plates of shell lac as before with the Mag. Electric helices and cores (   ), but there was *no action.*

4906. *Sulphur.* The plate of this substance employed in the same way. *No action.*

4907. *Copper.* The thick plates employed as before; *no action.*

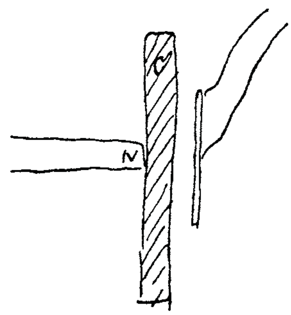
4908. Used one of the flat Helices, the North pole of the magnet and the revolving copper plate thus, but the copper acquired no power of varying the effect by revolution. Whether moving or quiescent, it was equally indifferent.



4909. *Shell lac.* Made a similar adjustment with the revolving shell lac, but it was equally ineffective.

4910. Now made this experiment. Placed a helix and a magnetic pole thus*. On moving the helix to and fro[m] the pole in accordance with the vibrations of the pendulum, currents in its wire were quickly formed and shewn by the galvanometer.

4911. Put a thick plate of copper C against the magnetic pole, between it and the helix: then moving the helix to and fro, currents were produced, just as easily and in the same direction as before, though the copper was half an inch thick.



4912. With the same arrangement, made the helix stationary and moved the magnet to and fro; the effect was the same and as ready as before. Whenever the interval between the magnet and helix was increased, a current was formed in the latter, which was the same whether it was the helix or magnet which moved; and so also when the distance was diminished.

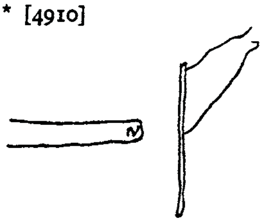
4913. This is a very striking and important result.

4914. When a thick plate of *sulphur* was in place of the *copper,* the effects were exactly the same.

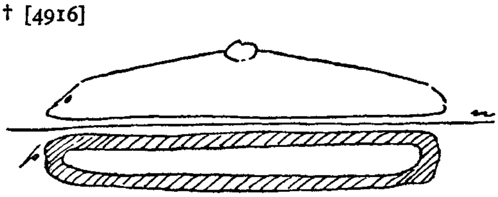
4915. I do not think the intervening bodies have any effect on the magnetic influence, whether they are in motion or at rest.

4916†. Put the case thus. If a wire *n* have a current suddenly sent through it, it tends to and will produce a contrary current in the wire *o*; but suppose a thick copper rod *p,* in form of a ring, put on its other side, will not that divert some of the force of *n* from *o? n* and *o* may be two helices and *p* a thick block of copper between.

* [4910]



† [4916]



4917. If it does, it will shew that neighbouring matter has its influence. If it does not, it will go greatly to shew that Electl. and Magnetic action are essentially different in their nature.

4918. Must there not be *some* effect on non-conductors, lateral to a wire carrying a current, correspondent to the effect on conductors? Or is the action really a distant one and only on like matter or on like kind of power? It may be so.

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4919. The strange case of diamond and carbon, as well as those of tourmaline and boracite, makes one look for something in crystalline structure.

4920. Crystd. metals together in different directions heated perhaps give currents.

4921. Crystals of conducting minerals, haematite, Elba iron, Antimony, etc.

4922. Perhaps Mica a good dielectric across its plate because that direction coincides with the optic axis.

4923. Try cubes of sulphur and shell lac, on faces A, B, C, D, etc.

4924. Excite one face of lac or sulphur cube first—then try the effect.

4925. Bismuth point drawn over Bismuth plate: will it give an electric current?

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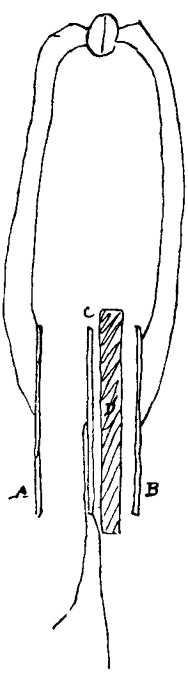


4926. Used three helices and a double galvanometer, i.e. a galvanometer having two wires each 42 feet long, wound together on one frame, so that separate currents could be sent through them—found by trial that there was no connection between the two wires; each was independant of the other.

4927. The coils were arranged parallel to each other and about 1 inch apart. The outer coils were connected each with one of the wires of the galvanometer, and the middle coil was connected with a V. battery, so that contact could be made or broken at pleasure.

4928. It was ascertained first that when only coil A or coil B was influenced by coil C, that either making or breaking contact powerfully affected the galvanometer. But then the distances were adjusted so that the action of the two, being in contrary directions at the galvanometer, no effect was produced there. At that moment two opposite current[s] were produced but exactly equal in effect.

4929. Put a copper plate D, 6 inches square and 0·6 of an inch in thickness, between B and C, and then made and broke contact as before. This caused not the slightest difference in the action of C on A and B. Placed it between A and C; still no difference.

4930. Used a plate of sulphur 0·9 of an inch thick; still not the smallest effect.

4931. Hence these interposed bodies do not in any way interfere with the action, and that is very strange.

4932. The only difference that could escape me might be one of time. The action might be quicker or slower through copper, air, sulphur, etc.; and yet the sum of action being the same and the time of vibration long, the result might be the same on the instrument.

4933. I was puzzled a little at first by the needles changing their magnetic state. But I soon found that the induced current was able to give a magnetic charge or destroy it very easily on such masses of steel as these small needles.

4934. The proof of non action of intermediate matter is not as yet sufficient; for the effects of currents produced in copper and non magnetic metals interfere. These being connected in all things with nonconductors, as for instance in their contrasted state in the induction by magnets, referred to in series ______ (213).

4935. In considering the induction of *currents,* remember the circumferential effect cannot be disturbed in degree, i.e. no power can be drawn from the left of a wire carrying a current by any thing, as copper, or any action, present or exerted on the right. Such is not the case in the induction of Static electricity. Nor is it the case with Magnetic forces exerted as poles. Must consider and relate this.

weak acid, and the other into the strong acid (5674). The effect was most excellent.

5690. *Lead; Lead; and Nitric acid.* The wire in the weak acid was Pos. to that in the strong acid and powerfully so. The alternation of state, when using tube No. 2, was excellent; and the whole experiment, as with copper, is a fine experiment.

5691. *Iron; Iron; and Nitric acid strong and dilute.* That wire in the weak acid is Pos. to the other. The alternations in tube 2 are beautiful, as with copper and lead.

5692. *Tin; Tin; strong and weak N.A.* The tin in the dilute acid is Pos. to that in the strong. Very violent action comes on here in the strong acid, or between it and the water, which mixes all up, and so the alternations are not so well observed with this metal as the former three.

5693. *Cadmium; Cadmium; Strong and weak N.A.* Cadmium in the diluted acid is Pos. to that in the strong acid. Powerful action and evolution of gas quickly mixes all up together.

5694. *Zinc; Zinc; Strong and weak N.A.* Same as Tin and cadmium; that in weak acid was Pos. to that in the strong acid.

5695. Now by way of comparing different metals in strong and weak N.A., I tried *Silver, copper, Iron* and *lead* against each other, for from the effects with the same metal, I thought that any one of these might be made Positive or Negative to *all* the others. They were tried in tube No. 1 The result was that

5696. *Copper* in the dilute acid was Pos. to Silver, Iron and lead in the strong acid and that strongly; that

5697. *Iron* in the dilute acid was strongly positive to silver, Copper and Lead in the strong acid; that

5698. *Lead* in the dilute acid was strongly positive to Silver, copper and Iron in the strong acid; and that

5699. *Silver* in the dilute acid was Positive to Iron and Lead—and almost to copper, i.e. it was negative, but only feebly so, and almost in a balanced state in comparison with the forces developed on the previous occasion.

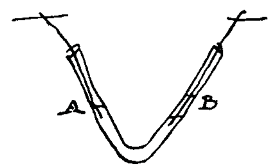
5700. Thus with the exception of silver Pos. to copper, any one of these metals may be made Positive or negative to the other by mere dilution of the acid.

5701. On making similar experiments with Tin, it when in the weak N. acid was strongly Pos. to Copper, lead, Iron and Silver in the strong acid—and when in the strong acid it was strongly Negative to copper, lead and Iron in the weak acid and a little so to silver even. So that these five metals may be made Pos. or Neg. to each other by mere dilution of the N.A. and no change of contact, making 10 Pos. and 10 Neg. changes, with the exception of silver Pos. to copper.

5702. *Cadmium* in the weak acid side is Very Pos., as might be expected, to Silver, copper, lead, Iron and Tin in the strong acid. Cadmium in the Strong Acid is Negative to lead; is neutral or perhaps Neg. to tin; is slightly Pos. to silver and copper and Pos. to Iron when these are in the weak acid.

5703. *Zinc* in the weak acid is Very Pos. to Silver, copper, lead, Iron, tin and cadmium, but in the strong acid is well Neg. to lead, iron, tin and cadmium, but a little pos. to Silver and copper.

5704. So also Zinc, Cadmium, Tin and Lead, and likewise Zinc, tin, Iron and lead, are groups of four metals each which, by means of dilution of the nitric acid only, can be made Pos. or negative to each other.



5705. Proceeded to try different metals, the electrolyte being Sulphuric acid, which was strong in one leg of the tube No. 1 and diluted in the other (5673). As a commencing experiment I diluted some acid by mixing 49 by weight of strong oil of vitriol and 9 of water, to give an acid with about 2 proportions of water; but on putting this acid into the tube on the stronger acid at B, and using Iron wires in A and B, I did not find that this degree of dilution made much difference. Nothing like so much as when a little water were put on B and slightly stirred upon the acid there, and left so as to give strata of different degrees of dilution. Therefore in the succeeding experiments used the latter mode.

5706. *Platina; Platina; Strong and Weak S.A.* That in the dilute acid is a little Pos. to the other. See Nc. acid and Platina (5684).

5707. *Gold; Gold; Strong and weak S.A.* Scarcely any change.

5708. *Palladium; Palladium; Do. Acid.* The weak is a little Pos., as gold or Platina might be expected to be.

5709. *Silver; Silver; strong and weak S.A.* On first immersion, the dilute acid side was Pos., but the current fell to 0°—then after trials gave almost insensible changes—could not say there was a decided difference.

5710. *Copper; Copper; strong and weak S.A.* That in the dilute acid is weakly Pos. to the other. By using tube No. 2 with dilute acid at the top in both legs, I could alternate the state of the wires and make either Positive in the weaker acid; but the effect is by no means so strong as that with the nitric acid (5689).

5711. *Iron; Iron; Strong and dilute S.A.* Whichever was in the dilute acid was Pos. to the other. Could alternate the state of the wires well in tube No. 2 (5674).

5712. *Lead; Lead; Strong and dilute S.A.* Rather irregular, but the one in the strong acid is the most Positive in almost every case. This is a good contrast of the different effects of the action of water as contrasted with those we might expect from effects of contact. The chemical action is here as strong in the stronger acid as in the dilute, and stronger indeed, judging from the sulphate formed.

5713. *Tin; Tin; Strong and dilute S.A.* The strongest acid here determines the Positive side and that constantly. Can alternate the states here and make either wire Pos. at pleasure by making it the one in the strong acid. This is in very striking contrast with Iron or copper.

5714. *Cadmium; Cadmium; Strong and dilute S.A.* Unsteady—no great difference. Sometimes the wire in the strongest and sometimes that in the weakest acid is Positive.

5715. *Zinc; Zinc; Strong and dilute S.A.* At the commencement the Strong acid was Pos. I alternated the ends into strong and weak acid (5674), and for a time the strongest acid side was still the Positive, but free effervescence of gas in the dilute acid came on by degrees and then the weak acid side was the Positive side.

5716. Made a few comparisons of different metals.

5717. *Iron; Cadmium; Strong and weak S.A.* Cadmium in the weak or strong acid was Pos. to the Iron.

5718. *Iron; Silver; Strong and weak S.A.* Iron in strong acid is a little Pos. to Silver in dilute acid. Iron in dilute acid is full Pos. to silver in the strong acid.

5719. *Iron; Copper; Strong and weak S.A.* Iron in strong or weak acid was Pos. to the copper.

5720. *Iron; Tin; Strong and weak S.A.* Tin in strong or weak acid is Positive to the Iron.

5721. *Tin; Silver; strong and weak S.A.* Tin in strong or weak acid is Positive to the silver.

5722. *Tin; Copper; Strong and weak S.A.* Tin in strong or weak acid is Positive to the copper.

5723. *Tin; Cadmium; Strong or weak S.A.* Cadmium in strong or weak acid is Positive to the Tin.

5724. So this acid is in beautiful chemical contrast with the Nitric acid, for none of the metals can be made to pass the others in their Positive and Negative relation by the contrast of strong and weak acid; and whilst copper and Iron are positive in the weaker acid, other metals, as lead and tin, are Positive in the stronger acid.

5725. Experimented with Muriatic acid. It was hardly necessary, for as the strongest acid solution is still much diluted, i.e. contains much water, it was probable that it would always act as a watered acid. But still, ran through the 9 or 10 metals. Used the strongest solution of pure Muriatic acid I could obtain, and employing tube No. 1, diluted on the top of the acid in the leg B and so experimented by dipping the clean wires quickly in and moving them as before (5517).

5726. *Platina; Platina; Strong and weak M.A.* Scarcely any change. If any, the strong acid side is the least Pos.

5727. *Gold; Gold; Strong and weak M.A.* Insensible.

5728. *Palladium; Palladium; Do. Acid.* If any effect, it is that the strongest acid is the smallest trace Pos.

5729. *Silver; Silver; Strong and weak M.A.* Silver in strongest acid is most Pos.—about 30°.

5730. *Copper; Copper; Strong and weak M.A.* Copper in strongest acid is very Pos.

5731. *Iron; Iron; Strong and weak M.A.* The strong acid side is for a moment the most Pos.; then quickly after the weak acid side is Pos. and continues so.

5732. *Lead; Lead; Strong and weak M.A.* The strong acid side is Pos.

5733. *Tin; Tin; Strong and weak M.A.* The strong acid side is Pos.

5734. *Cadmium; Cadmium; Strong and weak M.A.* Strong acid side is Pos.

5735. *Zinc; Zinc; Strong and weak M.A.* Strong acid side is Pos. at the first immersion—the action is quickly so violent as to prevent continued observation.

5736. Thus this solution of Muriatic acid is in contrast to both the Nitric and the sulphuric. The strongest is always the most Pos.

5737. Wished to know what *Potash* would do by dilution. So made a *very strong solution of good potassa fuza* and employed it.

5738. *Platina; Platina; Strong and weak potash.* The wire in the strong solution was clearly a trace Pos. to the other. Is this an effect of mixtion? If so, it is in the reverse direction to that of Nitric acid mixtion (5682). Or is it chemical effect? I took care to make all of the same temperature.

5739. Must remember that such a solution is a pretty good conductor of electricity and therefore shews feeble currents.

5740. *Gold; Gold; Strong and weak Potash.* Gold in the strong is clearly Pos. a little to that in the weak. More than in Platina case.

5741. *Palladium; Palladium; Strong and weak Potash.* Same, and about as Gold.

5742. *Silver; Silver; Do. Potash.* Same, and but little more than Gold.

5743. *Copper; Copper; Do. Potash.* Strong solution side was Positive 30° or 35°. Could alternate the ends so as to make either Positive or Negative.

5744. *Iron; Iron; Do. Potash.* Strong solution Pos. about 12° or 15°—and that with either wire.

5745. *Lead; Lead; Do. Potash.* Strong solution was very Pos. to the weak side; 80° or more. Could alternate the states of the ends.

5746. *Tin; Tin; Do. Potash.* Strong sol. side very Pos. to the weak. Could as with lead alternate the state of the ends.

5747. *Cadmium; Cadmium; Do. Potash.* Strong solution very Pos. to the weak; as lead, etc., could easily alternate the states. The cadmium quickly changed the colour of the strong solution, making it appear (by candle light) of rather a dark colour.

5748. *Zinc; Zinc; Do. Potash.* Strong solution very Positive. Could easily alternate the states of the ends.

5749. *Zinc* was Pos. to tin, cadmium and lead, whether in the weak or the strong alkali. *Tin* is Pos. to cadmium, whether in weak or strong alkali. *Cadmium* is Pos. to lead both ways; but most when in the strong alkali. So there are no inversions of the order of the metals here.

5750. Potash is like M.A. and contrasted with N.A. and S.A. in several respects.

5751. I endeavoured to select two metals which, being in one order in a cold acid solution, should be so differently affected by heat as to have that order reversed in the same solution when hot. The following were the trials and results.

5752. *Tin* and *Iron* in *cold dilute S.A. (5369)*. The tin was slightly Positive to the Iron. In the *same acid heated to boiling* the tin was perhaps mostly Positive, but the Iron was sometimes pos. on the first immersion and very distinctly so.

5753. *Tin* and *Lead* in *cold dil. S.A.* (5369). The tin was Positive a little when both metals were perfectly clean on their immersion (this cleansing requires much care). In hot dilute sulc. acid heated to boiling, the Lead was a little Pos. to the tin.

5754. So here there was a change in the order of the two metals by the simple action of heat alike on both, but the change was slight, and it would require to be very sure about the first effect of the two in the cold acid, as it is not in the same order as in dilute N. Acid.

5755. Tried to find a pair of metals which should be reversed in order by the action of strong or dilute acid upon them. For as Iron in weak S. acid is *Pos.* to that in strong S.A., and as lead or tin in weak S.A. is *Neg.* to that in strong S.A. it did not seem impossible to find a pair which should, when both were in strong acid, have A Pos. and B Neg., and when both were in weak acid, should have A Neg. and B Pos.; which could hardly consist with the theory of contact.

5756. When *Iron and Silver* were put into the *same strong Nitric* acid, for the first instant the Iron was Pos., but immediately after, the silver became Pos. strongly and continued so. When they were put into the same weak nitric acid, then the Silver was Negative. This therefore was at last a strong contrast.

5757. When Iron and copper were in Strong Nitric acid, at the first moment the iron was Pos., and then the copper permanently so. In the weak Nitric acid the copper was permanently Neg. So here the same sort of contrast.

5758. *Silver and lead* in Strong and weak N.A. The lead was Positive in both cases.

5759. Then with Strong and weak Sulphuric acid.

5760. *Tin and Iron* in strong and weak Sulphuric acid. The tin Positive in both cases.

5761. *Tin and Cadmium* Do. The cadmium Pos. both ways.

5762. *Tin and Zinc* Do. The Zinc Pos. both ways.

5763. *Iron and lead Do.* The Lead Pos. both ways. There were differences in degree but not in kind. Generally the Pos. metal was most Pos. in the Strongest acid, but that was not the case with Tin and Zinc and sulphuric acid.

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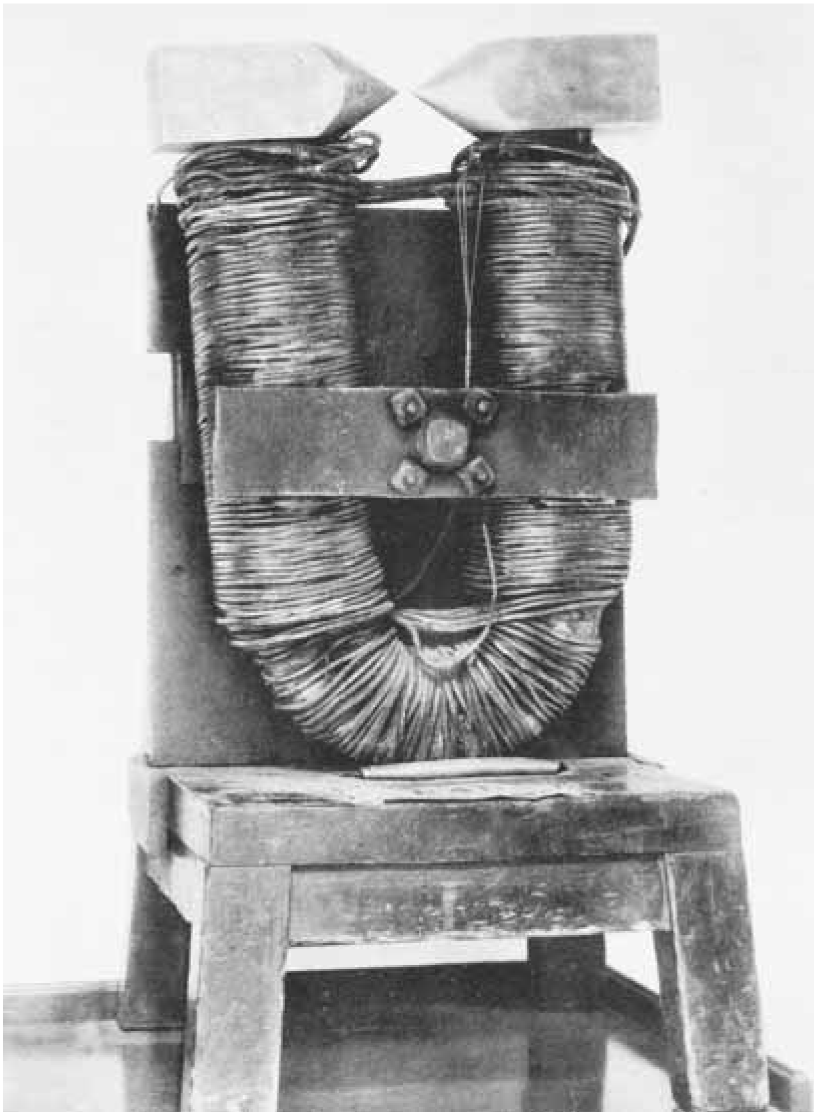
FARADAY'S DIARY

VOL. IV



FARADAY'S GREAT ELECTRO-MAGNET

The powerful electro-magnet (pars. 7874, 8408, November 1845), constructed from the link of a great chain cable, and used by Faraday in his experiment on diamagnetism

FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

made by

MICHAEL FARADAY
D.C.L., F.R.S.

during the years 1820 – 1862
and bequeathed by him to the
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FOLIO VOLUME V OF MANUSCRIPT

1845

PLATES

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FOLIO VOLUME IV
OF MANUSCRIPT
(CONTINUED)

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5764. Made some more experiments of the same kind. Thus, *Nickel* and *Silver* both being in weak Nitric acid, the nickel was Positive; both being in strong nitric acid, the silver was positive; but the state was preceeded by a moment in which the Nickel was Pos. So this is no case of inversion in strong and weak acid, except in consequence of the investing solution in the Nickel.

5765. *Nickel* and *Copper* in Strong Nitric acid. Copper was Positive from the first moment, strongly and constantly so, but in the dilute Nitric acid the copper was slightly but clearly Negative to the nickel. So this a good case of inversion with the same metals and the same acid. How then can we refer any of the effects to contact, which remains the same throughout?

5766. Again, *Zinc* and *Cadmium* being in strong nitric acid, the Cadmium is Pos. strongly and clearly to the zinc—but being in dilute nitric acid, the zinc is very pos. to the cadmium. So here a case as beautiful of contrast—against contact and for chemical action.

5767. Suppose a contact man were to say that it is only the very strongest acid that is able to render a metal negative to a piece of the same metal in dilute acid, and that the first portion of water added to the nitric acid makes it another thing as respects its *contact power*—then how far can this be carried; for Iron in a dilute nitric acid consisting of 1 vol. acid + 20 water is Positive to Iron in Strong Nitric acid, or in a dilute Nitric acid consisting of 1 vol. strong Acid and 1 vol. of water, or in a more dilute acid consisting of 1 vol. strong acid and 3 volumes of water, or in an acid consisting of 1 vol. strong acid and 5 vols. of water. Silver also, in the most dilute acid, is positive to silver in the four stronger states of the acid. Now how can this agree with the theory of contact?

5768. Or if it is said that the force of contact of the acid, or rather its difference of force, becomes gradually greater as the acid is more and more dilute, the metal in the stronger acid always being Negative to the metal in the weaker; i.e. in the cases, as of nitric acid, where the weak acid is the one making the metal Positive (the reverse of course being supposed to be the case where the

1-2

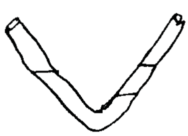
stronger acid determines the Pos. side (5712, 5713)); then how can the following cases be accounted for?

5769. *Copper Copper* being in Strong Nitric acid, and in nitric acid diluted with three volume[s] of water, the latter was Positive to the former, and so far the results agree with the former results and with the notion just expressed. But *Copper* and *Copper,* being in the solution of 1 vol. Acid + 3 vol. water, and another solution of 1 vol. acid and 20 vols. water, the latter was Negative to the former, instead of being Positive as in the cases of Iron and Silver. So that here an Acid of 1 vol. strong N.A. and 3 vol. water made the copper in it Positive to other copper, whether it were in a *stronger* or a *weaker* acid of the *same nature*. Surely contact could not change in this manner. And it was not the first addition of water which did this; for an acid consisting of 1 vol. Nitric acid and one vol. of water is with copper and copper as dilute acid to the strong acid, and as strong acid to the most dilute, i.e. 1 A. + 20 water. It is only when further dilute that it loses its relation of Neg. to the weakest acid and becomes Positive to both it and the strongest acid.

5770. *Lead Lead*. The case is the same and better with Lead, for the Acid of 1 vol. A., 3 vols. water makes lead in it well Pos. to lead, either in the weakest acid and the strongest acid. With it also dilution with 1 vol. of water is not enough to bring on this beautiful intermediate acid. The proportion of 1 and 3 is probably not the best—time would be required to work it out.

5771. Made some experiments with Sulphuric acid which I will enter, but must repeat them carefully: if correct, they afford still more curious contrasts.

A was strong Sulc. acid.
B, 1 vol. strong S.A. + 1 vol. water.
C, 1 vol. strong S.A. + 20 vols. water.



5772. *Tin Tin* in these acids, two together in tube no. 1. The tin in A was Pos. to tin in B or C, and tin in B was Pos. to tin in C. So that in the strongest acid was always Pos. to that in the weaker acid as before (5713).

5773. *Iron Iron* and these acids. The iron in A was Neg. to that in B or in C, and the iron in B was Neg. to that in C. So here the iron in the weaker acid was always the Positive. A striking contrast to the Tin and I believe a true result (5711). Though not in favour of contact.

5774. *Lead Lead* in these acids. Lead in A was Pos. to that in B or in C (5712). But lead in B was Neg. to that in C. So that B acid rendered the lead Negative either to lead in acid stronger or weaker (5828).

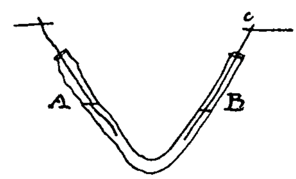
5775. *Copper Copper* in these acids. Copper in A was a little Neg. to copper in B, and it was in the former case Neg. to copper in C (5710). But copper in B is Pos. to copper in C. So copper in B is Positive to either stronger or weaker acid (5829).

5776. It is these two cases of contrast in Lead and copper that I must carefully repeat.

5777. Endeavoured to ascertain, if possible, whether there was really any thermo electric effect between Metals and fluids such as Potash, acid, etc. etc., or in fact any electrolytes, and especially those consisting of aqueous solutions.

5778. Began with a very strong solution of Caustic potassa. It was made of potassa fuza (good) and was cleared by decantation.

5779. *Platina; Platina; strong sol. Sul. potassa.* All was quiet and the needle at 0° when the whole was at common temperatures. By heating the side B, I could deflect the needle about a degree, 1°, B being Positive. On heating A, it became Pos. full 15°, and this went down on cooling. Reheated B side and it became Pos. 10°—cooled it and it went back. Again heated B side—it was at the first moment the least degree Neg., but became Pos. as it boiled. Found that the boiling had thrown a little spurt of potash up to the junction of the Platina and copper at *c*, and this made that side much Positive, which, though it went down somewhat, still in part continued. One would have thought the metallic contact there would have been quite enough to have prevented such a circumstance having any power to produce a current, but it did not. Cleaned this away, and renewing the contact, left all quiet for half an hour to wear out irregularities. All was then cold and the needle at 0°. Heated A: it became Pos. about 5°; cooled it: it went down again. Heated B: it became Pos. about the same; cooled it: it went down again. As the heat rose the effect appeared apparently in proportion to it; but when all was hot and nearly boiling, on making it quite to boil it became much more Pos., as



if boiling itself did something, for the difference in the heat of the fluid must have been small at that moment.

5780. Boiling by sending steam, etc. up to the higher part of the wire would heat it quickly, and by diminishing conduction from the immersed end would cause it to be hotter. Perhaps that was the cause of the increased effect.

5781. If the effect is *thermo electric,* then Platina heated in sol. Potash is *Pos.* to Platina in the cold part of the same solution, but not more than 5° by diffnce. of about 60° and 212° F.

5782. *Gold; Gold; and sol. strong Potash.* At first the needle at 0°. Heated A: it became about 3° Pos., or when boiling, 5° or 6°. On cooling A the needle returned to 0°. Heated B: it became Pos. in turn about as much; on cooling, the needle returned to 0°. The fact of the boiling increasing the deflection is rather in favour of the idea that it is really thermo electric.

5783. The distance of the metal ends in these cases was about as much as in the various former experiments on chemical force, and therefore the chemical and the thermo effects are so far comparable. But the potash was very strong and therefore a very good conductor, far better than any of the diluted acids used. The *thermo* effect here is therefore by so much magnified.

5784. *Iron; Iron; Strong sol. Potash.* Was neutral and at 0°: then heated A and it became Pos. to 40° or more. On examining it, could see a cloud forming at it, when left quiet, apparently of prot oxide of iron. The effect gradually fell as temperature fell. Heated A again, which raised the Pos. state somewhat, but it looked as if there were an investing effect at A. Left all a minute, and then B of a sudden and spontaneously became Pos., though it had not been touched or heated—heated B to boiling, which raised it Pos. to 80°—left it and that state gradually went down—heated B again and the state rose again, but not so high as before.

5785. The wires looked bright when brought out, but were tarnished at the part in the air above the solution.

5786. So the Iron is evidently a good case of a current produced by chemical action and not by contact. It is in good contrast with those of contact, as Platina and Gold.

5787. Proceeded to use the strong solution of *sulphuret of Potash* (5263, 5268) in place of Potash, to compare supposed thermo current of it on Gold and platina, as in Potash. It is a most excellent conductor of feeble currents.

5788. *Platina; Platina; Sulrt. Potash.* Some motion, shewing some difference in wires, and that something besides heat can act with them at present. There was but a small and uncertain effect and after a while the needle was at 0°. Warmed and boiled side A —the Platina there became steadily *Neg.* 10° or about—increased to 20° gradually, i.e. after the spirit lamp was withdrawn. Cooled A and the needle went back to 0°. Warmed B: it became a very little Neg., perhaps 1°—removed the lamp and left it and this increased a little—renewed the heat and the Neg. effect diminished —then left alone, it again increased gradually up to 7°—it fell slowly, but applying a little heat diminished it faster. Cooling at first made it still more Neg. and then it fell to 0°. Heated A again: it did not become sensibly Neg. now during the time of heating, but afterwards on being left to stand rose to Neg. 12° or 15°. Heating lowd. it to 0°; then being left it became Neg. again— heating again lowered it about 1° and then being left, it became Neg. again.

5789. The effect is I believe chemical and not thermo electric. I think it is principally due to the action of the air (a current of which the heat tends to form in the tube) upon the hot sulphuret —the effect of the heat, when applied to lower the Neg. state, may be due to its forming currents, and so by mixing up the solution to destroy in part its polar state. Being cooled, all fell to 0°.

5790. At all event, the heat did not render the wire heated Pos., whether it be a thermo electric effect or not.

5791. *Palladium; Palladium; Sulrt. Pot.* strong solution. At first was 0°. Heated B: it became the least trace *Neg.,* which went off in part during the heating—after a while it was at Neg. 2°— reheated B, which reduced its Neg. to 0°—stillness brought it up to 3° again—is the air acting here—cooled B: its Neg. went down to 1° or 2°. Heated A: it became Pos. up to 3°, but on cooling returned to 0°; on reheating A again, it became Pos. 2°. Here therefore there is no evidence of thermo effect or even of chemical effect. The two wires are not quite alike, but still the whole difference is very small; 2° or 3° only.

5792. It certainly does seem in many experiments as if Platina had

6150*. Arranged thus: H is a double helix, of Palmer's apparatus, either with or without an iron core. G is a pretty fair galvanometer. When the ends of the wires P and N were put in contact with a voltaic pair, G was deflected on making contact and also on breaking contact—but not on continuing contact. So induction could act and insulation was right. But when P and N were made to touch the Edge and axis of my revolving copper plate moving between the poles of a horseshoe magnet, no effect took place at G, though an *induced* current was running round where the voltaic current was running before.

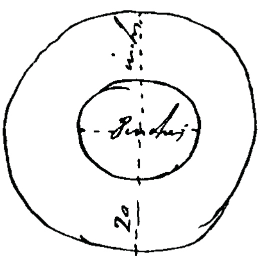
6151. Whether the iron was in or out of H made no difference.

6152. Perhaps the exciting apparatus was not strong enough or the test part not delicate enough. Yet when the wires of G were put at once in connection with my revolving wheel, then the Galvanometer was deflected, perhaps 30° even.

6153. So no evidence here of any peculiarity in the induced current, yet it ought to have it if voltaic induced currents have it.

6154. Use own powerful ring Electro magnet for the first magnet. Henry's helices and larger helices in place of H, and a more delicate Galvanometer.

31 AUG. 1840.



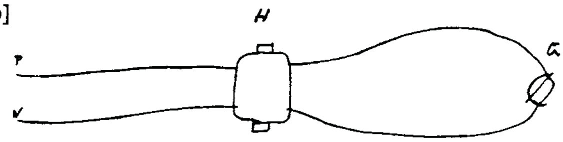
6155. Have arranged to examine the inductive action of an induced *current*. I arranged a powerful Electromagnet. Then I set my revolving copper wheel at work and carried its current round a large coil of Henry's; and then induced by that upon another coil, whose current circulated round a delicate galvanometer. The arrangement as seen nearly in plan was as below †.

D, a Daniell's battery of 3 cells, 18 inches high.

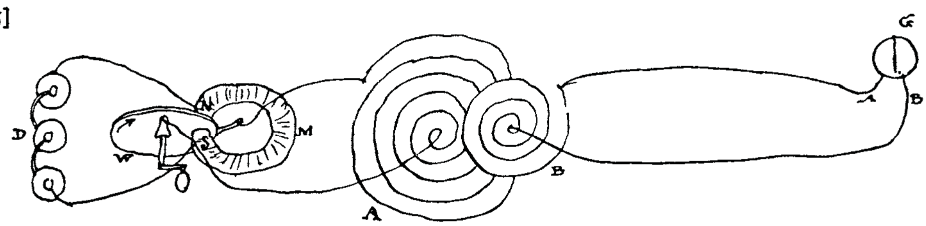
M, own large ring Electromagnet: as connected, N and S now the north and south ends. This includes the Primary current.

W, my revolving copper wheel, with collectors at the axis and periphery, passing away to Gassiot's large flat coil A.

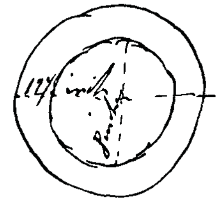
* [6150]



† [6155]



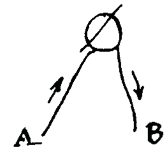
A, Gassiot's coil of 300 feet of copper plate 1¼ inches wide, covered with cotton and forming a coil 20 inches in external diameter, having the direction given by the curve. W and A include the whole circuit of the first induced current; my current or Henry's 2ndary current.



B, Daniell's coil of 200 feet of copper ribbon, forming a helix or coil of 12½ inches in diameter, covered with cotton. This had the direction shewn and was placed concentric with and over the first. The forms of these really were as below[1].

G, a delicate Galvanometer indicating thus: when zinc was at A and Platina at B and the tongue between, the deflection was as shewn and the current as marked. When the deflection was in the reverse direction, the current was the other way.

6156*. Now direct revolution of the wheel affected the galvanometer, and so did the reverse revolution—in the opposite direction. So the secondary current thus obtained could induce a tertiary. Direct revolution is that marked by the arrow on the wheel.



6157. Direct revolution gave this current and reverse revolution this. So with direct revolution the currents throughout would be as marked above[2].

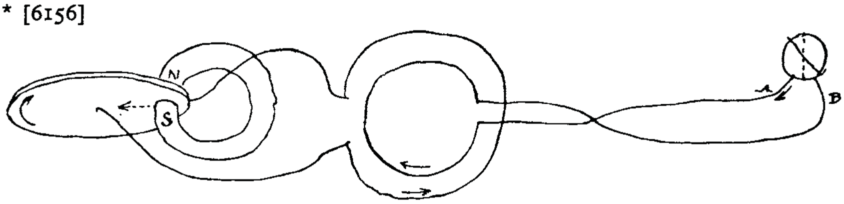
6158. But now the constant secondary current *did not* induce a constant tertiary current, for the Galvanometer came to rest, and though the secondary current continued in the direction of the arrow in the helix, yet the contrary tertiary current ceased instantly. So these currents are not peculiar in any such quality.

6159. Then it was to be expected that the secondary current would induce at its beginning and its ending, like other currents, and in contrary directions; and this was found to be true; for keeping the wheel in continual direct revolution and making and breaking contact at the periphery of the wheel, it was found that on making contact the galvanometer deflection was thus and on breaking contact thus.

6160. So the secondary current is like any other current in this, that at its beginning it induces a current in the neighbouring wire in the *reverse direction,* and in ending it induces a current in the

[1] i.e. in margin. [2] i.e. in diagram below [6156].

* [6156]



same direction. But it is unlike the primary current in this, that it induces most strongly at its beginning, whereas the primary induces most strongly at its ending, and hence the reason why, as Henry experiments, the difference between the action of the primary and the induced currents.

6161. When all the connexions were complete, causing direct motion of the wheel made the tertiary current thus. Reversing the motion of the wheel caused a reverse current thus, and these being alternated made the Galvanometer needle swing very well.

6162. But even stopping the wheel causes a tertiary current, the reverse of that of beginning revolution; and reversing the motion is in fact doubling the effect which stopping the revolution of the wheel would alone do.

6163. So causing direct and reverse motion of the wheel is the most effective way of shewing the tertiary currents. But still every current can induce two currents, one at its beginning and another at its ending, and none merely during its continuance.

6164. The superiority of the current induced at the beginning of the secondary current rather than the end may depend upon the circumstance that both the circuits concerned are entirely metallic, for this allows the secondary current, when it is about to stop, to induce in *its own wire,* which it would rather do than in the neighbouring wire, and so on.

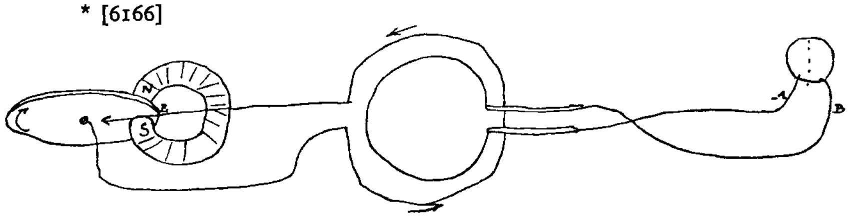
6165. Probably when, with the machine now constructing, the secondary current is made to begin and stop by making and breaking contact at the edge of the wheel more effectually than now I can do, the breaking contact may be as strong.

SEPTR. 1, 1840.

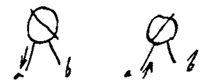
6166*. The arrangement was as before and as above[1]. The direction of the permanent induced current when the motion of the copper wheel was continued is as marked by the arrows above[1].

[1] i.e. in diagram below.

* [6166]

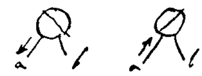


6167. When the wheel revolved direct, the Galvanometer was thus. When wheel revolved reverse, thus. This agrees with the former observation ( ).

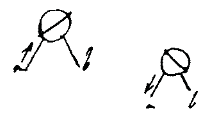


6168. I caused the secondary current to begin and cease, not by beginning and ceasing to rotate the wheel, but by continuing the *direct rotation* of the wheel and making and breaking contact at the edge of it, as at E. In this way the current had more sudden terminations.

6169. Making contact caused deflection by the tertiary current, thus; and breaking contact cause deflection and current thus. So that making contact is equivalent to beginning to rotate the wheel, and breaking contact to ceasing or reversing rotation of the wheel. All this agrees well together.



6170. I now kept the wheel still, but made and broke contact at the battery, and there was certainly motion at the needle of the tertiary current. Making contact sent galvanometer this way and breaking contact this way. But when the contact at E on the wheel was broken, the same effect took place as if it were the action of the large electromagnet at a distance and not any induced electrical current.



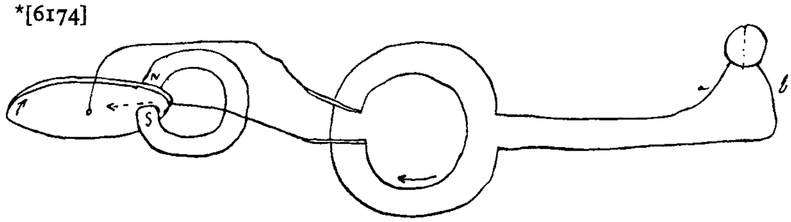
6171. Indeed, such making and breaking of contact ought to do nothing, for then the radius of the wheel and the magnetic curves do not move *across* each other, but the curves are projected end ways, so to speak, through the radius. It is as if a wire were brought *right up* to a magnetic pole and not carried *by it* and *across the curves*. In the former case, no current is produced; in the latter case, one is formed.

6172. But try this again unexceptionably.

6173. To prove the great point that no current induces during its continuance, I connected the Daniell's battery with the first coil (Gassiot's), and though the secondary current at making contact was very powerful, yet the *continuance* of the primary contact gave no effect at all.

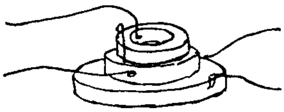
6174*. I turned the two helices round so that Daniell's was in the wheel circuit, the battery and the magnet being reconnected as before ( ), and Gassiot's helix the one induced upon. The wheel therefore sent a secondary current which, in a coil of 200 feet, induced a tertiary current in a coil of 300 feet connected

*[6174]



with the Galvanometer G. The arrangement was as in the next page[1].

The effect was about the same as in the former case. The direct motion of the wheel caused deflection thus, and the reverse motion thus. So that here, on making direct revolution, the secondary and tertiary currents were, as before, in opposite directions.

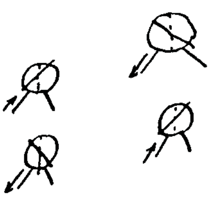


6175. Placed Solly's helix on Daniell's and connected them into one consistent helix of 263 feet in length. Now the effect stronger.

6176. Repeated the making and breaking of *battery* contact (   ) and obtained an effect in the same direction as before. But when the secondary circuit was interrupted permanently, still the same effects. So must be Magnetic induction at a distance.

6177. I wished to obtain the inductive effect of the breaking of the secondary current by itself and quite clear from any other action of making or reversing that current, and proceeded thus. The wheel was turned *direct* continually, the other contacts not yet being made; then they were made and broken in the following order: contact made in wheel circuit—made in galvanometer circuit—broke in wheel circuit—broke in galvanometer circuit—made in wheel circuit, etc. etc. etc. In this way the galvanometer or tertiary current depended entirely upon the breaking of the wheel or secondary circuit. On repeating the action several times, so as to accumulate the effect of the breakings, upon the galvanometer the effect was thus for the current induced by the breaking, and this is just the reverse of that due to making. It was not nearly so strong as the latter, according to appearances.

6178. As to revolution, stopping and reversing the motion of the wheel, the effect with the present arrangement was thus:



Revolving the wheel direct caused tertiary current thus
Stopping the above revolution
Revolving the wheel reverse
Stopping the reverse revolution

and there was the *pull* at the needle each time of stopping as at the time of beginning to revolve.

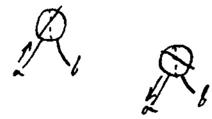
6179. So the *secondary current* being continued and during its continuation *does nothing,* and these currents are just like other currents.

[1] i.e. in the diagram above [6174].

6180*. Now threw the Electro magnet and wheel out of use and arranged the Daniell's battery with the compound Daniell-Solly coil (   ), and allowed that to induce upon the Gassiot coil (   ).

On making battery contact the galvanometer was thus . . . .

On breaking battery contact „ „ thus . . . .



So the induced or secondary current was on making contact the *reverse,* and on breaking contact the *same* in direction as the primary current.

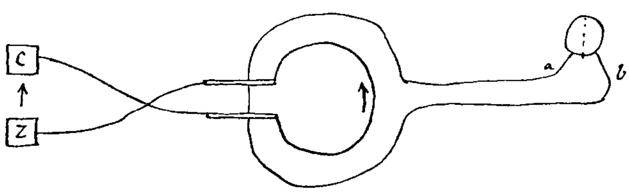
6181†. Put in our great cylindrical helix of thick wire so that one of the helices in it should, with the Gassiot coil, form the secondary current circuit, and the other helix in it with the Galvanometer form the tertiary current circuit. But whether the iron core was in it or not, I could not perceive clear indications at the galvanometer on making and breaking the contact at the battery.

6182. The helix consisted of two thick wire coils, each perhaps about _____ feet long. Perhaps this was not length enough for a good induction.

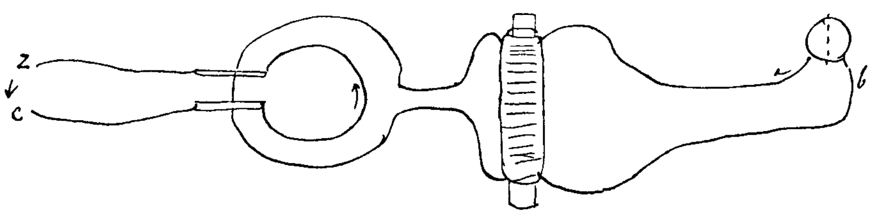
6183. There is I think great power lost at each inductive step.

6184‡. Arranged things thus for the Tertiary current. Daniell's battery with Gassiot's large coil (   ) formed the primary circuit. Daniell's coil over Gassiot's, with Solly's coil and a galvanometer, formed the secondary circuit; and Gassiot's long fine wire helix in the middle of Solly's coil, with a Galvanometer, formed the tertiary circuit. Both galvanometers, A and B, indicated the same way, i.e. thus.

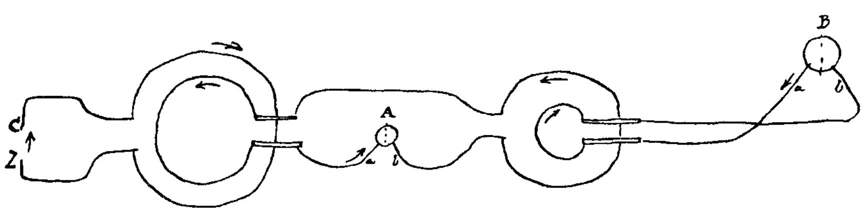
* [6180]

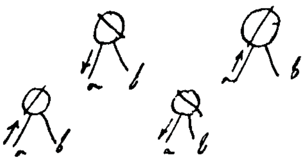


† [6181]



‡ [6184]



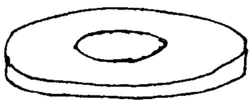


6185. Now had a result; on making battery contact, B Galvanometer moved thus, and on breaking contact, thus.

6186. Again with the same arrangement.

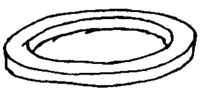
Making battery contact, A was thus and B was thus. So each induced current was the reverse of the inductric current, as in former cases of *making contact*. Only, as the making and ceasing of the secondary current must have been at the same moment that it induced the reverse current in the tertiary circuit, so the making influence seems by far the most powerful, and there are natural reasons, plenty, why it should be so.

6187. So all my visions of new kinds of current are gone. See (6143). See also 6100-4, 15, 22, 3, 8 up to 6187.



6188. *Gassiot's coil* consists of 300 feet of copper ribbon, 1¼ inch wide, covered with cotton and made into a circle having an external diameter of 20 inches and an internal diameter of 8 inches.

6189. *Daniell's coil:* 200 feet of copper ribbon 1½ inches wide, in a coil 12½ inches external diameter and 8 inches internal diameter; the copper ribbon covered with cotton.



6190. *Solly's coil:* copper ribbon 1½ inches wide, covered with cotton, made into a coil 9¾ inches external diameter and 7¾ inches internal diameter; there are 27 circumvolutions at a median length of 28 inches, making 63 feet of ribbon.

6191. *Our double helix of thick wire.* One wire 37 feet and the other 42 feet long and _____ of inch thick.

6192. *Gassiott's Wire helix.* 6 coils of 40 feet each; also one coil of very fine wire about 4000 feet long.

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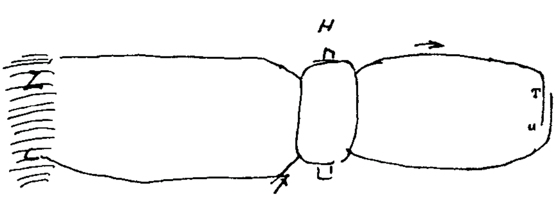
6193*. Gassiot made an experiment here to-day to shew me that the induced current always went in a peculiar direction and against expectation across air under certain circumstances.

H is a double iron helix, the one described in the preceeding page[1], and the iron wire core was in it. Z, C were three Daniell's cells. The terminations of the second wire at T, U were of fine copper, parallel to each other and about 1/20 of an inch apart. On

[1] i.e. at par. 6191.

* [6193]



making or breaking contact at *x*, no discharge of the secondary current happened at T, U at common temperatures; but when a lamp flame was applied there, then the current induced on breaking contact at *x* almost always passed as a spark or brush.

6194. It is striking to see the effect of the hot air: how great it is. Holding the spirit lamp under immediately makes the spark pass at the hot place.

6195. It is not the carbonaceous flame only that does this, for the hot stream from the end of a blow pipe does it also.

6196. A very striking circumstance is that, as the discharge always takes place at uniform temperatures, either from the end at T or at U, so it is always at that end which is negative. Thus, if the secondary current were moving as marked, then the discharge would be at T. If the current were reversed, then it invariably took place at U. Ascertained this very carefully by several examinations of the coil, etc.

6197. This constancy must be important. We should have expected that the discharge would have been from the Positive point to the Negative large surface. Perhaps it has relation to Belli's results (Exp. Researches, 1520). He found the negative electricity more easily dissipated than the positive. See also my similar conclusion at 1501 of the Exp. Researches.

6201[1]**.** Have been making a few expts. on electricity of steam, to see whether it might not be from friction against metal, as the metal cock or pipe. Have Boiler of the London Institution, which has going from it a pipe with 2 stop cocks. One of them I directed over the hot chamber of a furnace, to procure dry steam in the jet, and the other went into the air.

6202. The steam, whether at the hot cock or the air cock, and whether collected against an insulat[ed] conductor or a plate or by a wire or a flame in the jet, was always either not electrified at all or else *Positive*. Often not electrified apparently when it might have been expected to be electrified, and then appearing to be so. The electrometer moved up and down as if the state taken on by the successive portions of steam were capricious; but still the steam was always *Positive* if electrified.

6203. Had an electrometer at a distance of several feet from the cock and sent puffs or a stream of steam and air near it. It happened that 2 or 3 times at first, this electrometer, with a flame on it, was charged *Negative;* but afterwards it over and over again under the same circumstances became *Positive,* and I could not again make it *negative*. Do not as yet see the reason of this.

6204. At the beginning, there were 11 inches depth of water in the boiler; worked about 1½ hours on and off; then there was about 9 inches depth of water left, so that only 2 inches of water had been used. On filling the boiler up again, we found that a gallon of water occupied just an inch in depth.

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6205. Worked again with the boiler, but now supported it on three pieces of shell lac, so as to insulate it.

6206. Put the Gold leaf electrometer at a distance from the end cock and threw jets of steam towards it, as yesterday (6203). A flame was on the electrometer and it was obtained in a *Negative* state, as yesterday, over and over again. Indeed, it was obtained only negative at this distance.

[1] Numbers 6198 to 6200 are omitted in the MS.

6207. Had a new collector—a short wide tube and 3 or 4 wire gauze diaphragms in it. This, with the insulated conductor, acted well at the side cock over the hot air. The sparks from the conductor were good, and continued to come as long as the blast of steam was running against it.

6208. Insulated the boiler, and instantly found it *negative* as soon as the jet of steam was allowed to issue at the side cock (over the fire). Obtained a constant stream of sparks from it as long as the jet continued.

6209. Now opened the valve hole full size; made good fire under the boiler and let steam escape freely. Not the least signs of electricity in the boiler by the gold leaf electrometer—or from the issuing steam by the same electricity[1] and flame. So at low pressures no electricity evolved here—is as if the mere evaporation does not cause it.

6210. Put in the valve and with the lowest pressure began to try the cock issue of steam, connecting the electrometer with the boiler. Found this an admirable method, for the Electrometer gradually became Negative as the steam issued out and the whole was under beautiful management.

6211. Best way by far is not to examine the issuing steam, but the state the boiler is left in.

6212*. The present steam pipe has two cocks, one at the end, the other at the side. Now found that, if the end cock were opened at this pressure, the boiler gave no electricity; but if the side cock were opened, the boiler gave beautiful charge. So the electricity is not caused apparently by evaporation, but at the cocks. This is still for the friction view.

6213. The steam has to turn a sharp angle inside the side cock, but not at the straight cock or end cock.

6214. Here then two steam issues from the same boiler, and one generates electricity, the other not, at this pressure. At a higher pressure both do so, but the side cock is by far the best; the end cock but little.

6215. The steam valve is a circle of 0·5 of inch diameter; the valve and weight together 1 lb. 2 oz. This is not more than 6 lb. upon the square inch for our highest pressure.

6216. Now put in an old side connecting piece to the end cock

[1] ? electrometer.

* [6212]

and then let steam out there. Found the boiler now constantly *Positive,* not Negative—but to a slight degree only. So by the exits may make either the steam or the boiler Positive or Negative.

6217. Put a conductor and gatherer to the issuing steam of side piece and an electrometer attached. Put another electrometer to the boiler. Now the side piece termination gave steam which made its conductor *Neg.* and left the boiler Positive. This was done many times. But when the steam was at the highest force, occasionally had the steam Positive and the boiler Negative.

6218. At last the steam lifted the valve, and this issue now made the boiler negative, though when quite open it could not. The steam I found by Electrometer and lamp was Pos.

6219. But on letting out steam from the end side piece, its steam was Negative. For first it neutralized the effect of the boiler electricity produced by the valve issue, making the boiler neutral, and next when sent against a collector it made it Negative.

6220. So then at this moment *Pos.* steam was issuing from the valve and *Negative* steam from the side hole at the end, and the boiler was neutral.

6221. Hence see the power of the *issue* and that the electricity is generated there.

6222. Again and again the Valve issue made boiler *Negative.* The End cock made boiler *Positive.* The side cock over the fire by itself always made the boiler *Neg.,* as the valve did, but when the side connection was put on () it often made the boiler *Positive.*

6223. The side cock of the apparatus (London Institution and a different shape to ours) was put on at the end of the pipe instead of the other cock. The first time it was used there it made the boiler *Positive,* but afterwards always *Neg.*

6224. Held wires and other things about half an inch from the end-cock in the jet, to see what friction would do; the wires, etc. being connected with a Gold leaf electrometer. In this way, bright copper wire, oxid. copper wire, plunbago, platina and wet string always were Neg., and made Electrometer so strongly. But with wet string put the boiler out of insulation, and then no effect, as if the string had gathered its state from the boiler.

6225. When boiler was uninsulated, still metal of rod gave strong *Neg.* when held in the stream.

6226. Worked from 10 until 2 o'clk., about 4 hours: in this time level of water had sunk from 11 inches to 5½ inches. On filling it up to 11 inches again, 4½ gallons of water were required. The boiler is smaller below than in the middle.

6227. Held glass and shell lac in issuing jet of steam, but perceived no effects.

6228. May perhaps equal a voltaic battery by this mode of obtaining steam.

6229. The ordinary charcoal smoke of the furnace shews no effect, nor does it leave the furnace sensibly charged.

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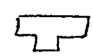
6230. Boiler up and insulated. The side and end cocks the same as before (   ) ; the side cock over hot air stove.

6231. The side cock instantly and always renders the boiler *Neg.*

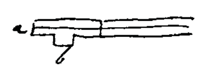
6232. The End cock at first with low pressure rendered the boiler *Pos.,* with little exit of steam, but *Neg.* with more exit. Afterwards, with higher pressure, always rendered the boiler Negative when the cock only was used.

6233. But when an end piece was on the cock, then over and over again gave the boiler *Positive.*

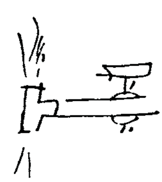
6234. At last, when the pressure of steam was up, the end connector did nothing, i.e. the boiler was not charged. But if the side cock was opened for an instant only, it made the boiler well Negative.



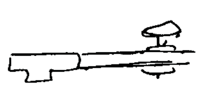
6235. Have a two way end piece thus; when put thus on to the pipe*, it gave no signs of electricity to the boiler. But when put on thus †, it made the boiler fairly *Positive.*

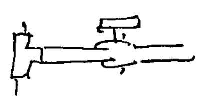


6236. When piece on thus, the passage of steam was so quick through the termination that all went out at *a,* and air actually entered at *b.* So finger drawn to *b.* Expect that steam should not touch sides of channel again when it has passed the place of quickest motion, or it becomes discharged again.



6237. On repeating the last expt., now had no effect on the boiler; all null. Taking off the cross piece and using the end cock alone, it now always made the boiler *Neg.,* as the side cock does, but to a very much less degree than it.

* [6235] 

† [6235] 

7485. *Heavy Optical Glass*—mine. A piece about 2 inches square and half an inch thick, coated, etc. and examd. No effect: the ray of light passing across the lines of inductive action. Then made 2 round holes, small  in the tin foil linings, so as to look through the glass *along* the line of inductive action. Still no effects.

7486. A *cube of quartz* (4671, 4692, 4759, 4773) cut so as to have two opposite faces perpendicular to the axis of the crystal. The cube had been well varnished to keep its surface insulating (4683). The coating in this case was a ball (brass) on each side of the cube and in contact with it thus*, and this is about the size[1].

7487. The cube can evidently have light passed through it in two directions whilst the lines of inductive action are in one given direction, and as the lines of inductive action can be passed across it in three directions, there are of course *six* positions through which the line of inductive action and the line of the light ray can be varied. The cube was examd. in all these positions but without effect.

7488. A second cube of *rock crystal* examd. in the same manner—no effect.

7489. A cube of *Iceland spar* (4662, 4712, 4748, 4773) cut in the same relation to the crystal and examd. in the same way—no effect.

7490. Now two coatings were made of fine wire gauze—a disk of it being soldered into a ring of thick wire, and these were used as the coatings or surfaces to effect induction; but they were now put against the faces of the cube through which the ray passed†, so that the lines of inductive action and the ray were *parallel*. There are of course three such positions of each cube, but in none of these was any effect observed.

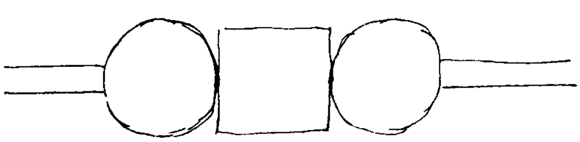
7491. *Air*. The light ray was passed both *along* and *across* the lines of inductive action, but there were no effects produced.

7492. *Oil of turpentine* in the long glass trough (7434). The light was passed *along* and *across* the lines of inductive action, but with no effect.

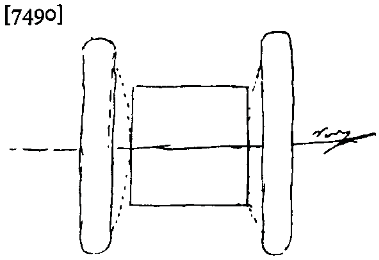
7493. Now an arrangement was made so that a discharge could be made either along or across water or an electrolyte contained in the glass trough (7434)—the discharge being either that of a simple spark or of a jar charged highly. Of course this was for

[1] Reduced to 3/4 scale.

* [7486]



† [7490]



the moment *a current effect,* but then it was a current of very *high intensity,* almost infinitely higher than any the Voltaic battery could give directly.

7494. With *Distilled water.* No effect with any of the discharge, either when the discharge was along the course of the ray or across it.

7495. *Sat. sol. Sul. Soda.* No effect in any case.

7496. *Sol. Sulc. acid* (7475). No effect in any case.

7497. So all these experiments are *nil* as to any effect produced by induction or electrolization upon electrolytes or nonconductors which can be rendered sensible on or by a polarized ray of light.

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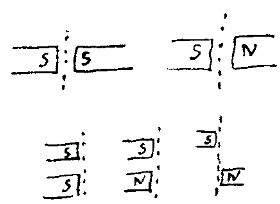
7498. To-day worked with lines of magnetic force, passing them across different bodies (transparent in different directions) and at the same time passing a polarized ray of light through them, and afterwards examining the ray by a Nichol's Eyepiece or other means. The magnets were Electro magnets, one being our large cylinder Electro magnet and the other a temporary iron core put into the helix on a frame—this was not nearly so strong as the former. The current of 5 cells of Grove's battery was sent through both helices at once, and the magnets were made and unmade by putting on or stopping off the electric current.

7499. First, *Air.*

Considering the plate or portion of air along which the polarized ray passed as a fixture, then the following variations of the positions of the magnetic poles were made: Similar poles were on opposite sides—opposite poles were on opposite sides—similar poles were on the same side—opposite poles were on the same side—and opposite poles were at the opposite ends, or at least so that the lines of inductive action were along the ray. With *each* variation of position, the possible effect at the *breaking* and the *making* of contact; and of the *constant current,* and of the intermitting current, was tried. Yet with *Air,* no effect could be observed.



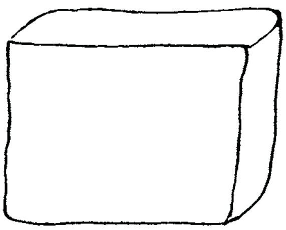
7500. *Flint Glass.* A piece of flint glass about 2 inches square and half an inch thick, polished on all sides, was tried with all the above variations of conditions (7499), but with no effect. It was also shaken so as to move it, and it was tried in all positions, but in vain. It was very full of striæ, and was in such a state of tension as partially to depolarize the beam of light.

7501. Three different cubes of flint glass from 1 inch to 3/4 of an inch in the side were also tried—but results all *negative.*

7502. *Rock crystal.* A cube of this substance (4773, 7486), being that formerly used, was placed under all the conditions above (7499), and examd. in its three positions—but *no effect* was observed.

7503. *Calcareous spar,* transparent. Two cubes of iceland spar (4773, 7489), being those formerly used, were also examd. as above (7499), but without effect.

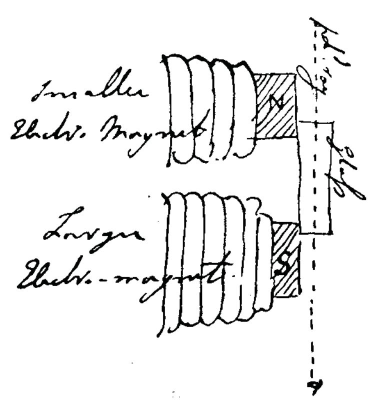
7504. *Heavy glass.*

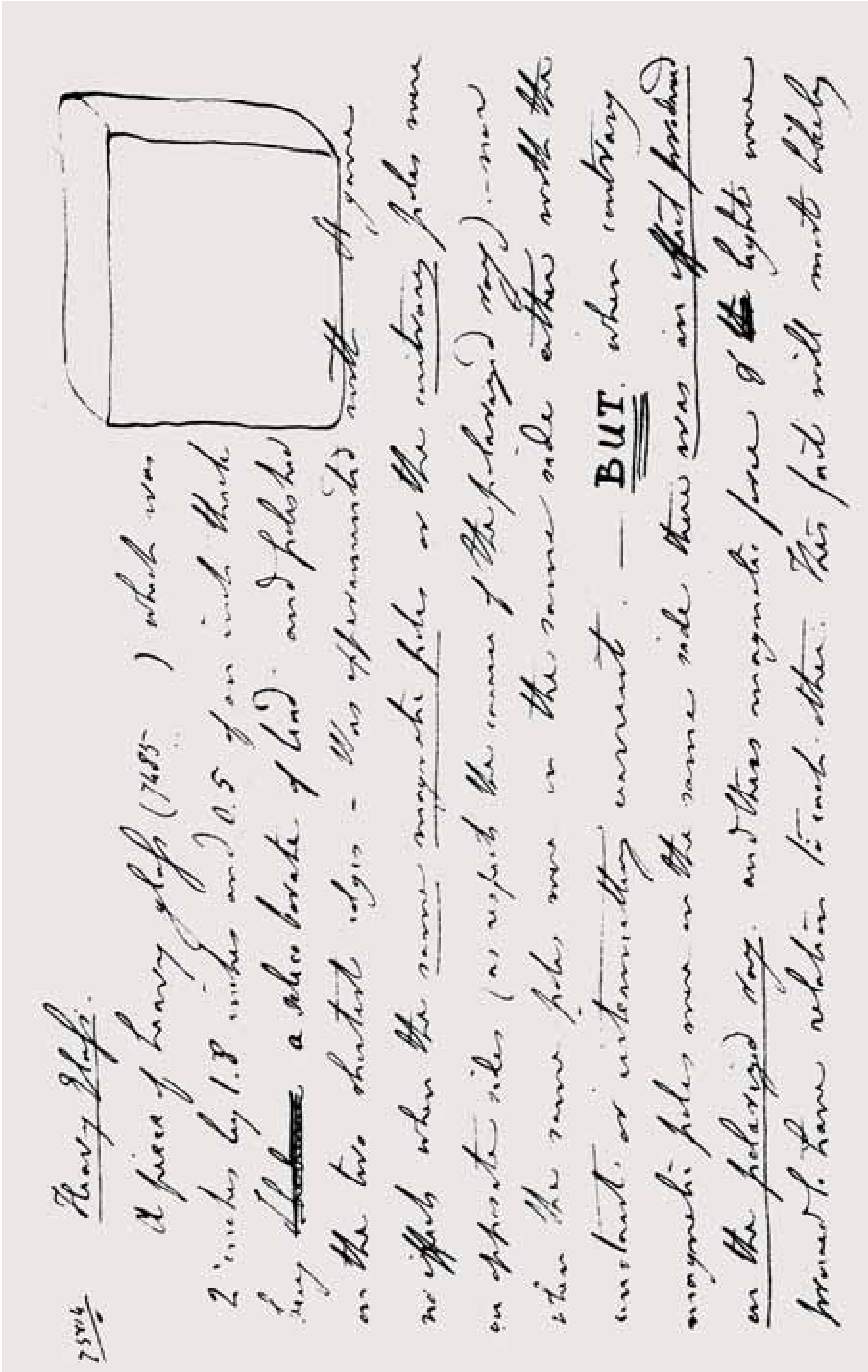


A piece of heavy glass (7485) which was 2 inches by 1·8 inches, and 0·5 of an inch thick, being a silico borate of lead, and polished on the two shortest edges, was experimented with. It gave no effects when the *same magnetic poles* or the *contrary* poles were on opposite sides (as respects the course of the polarized ray)—nor when the same poles were on the same side, either with the constant or intermitting current—BUT, when contrary magnetic poles were on the same side, there *was an effect produced on the polarized ray,* and thus magnetic force and light were proved to have relation to each other. This fact will most likely prove exceedingly fertile and of great value in the investigation of both conditions of natural force.

7505*. The effect was of this kind. The glass, a result of one of my old experiments on optical glass, had been exceedingly well annealed, so that it did not in any degree affect the polarized ray. The two magnetic poles were in a horizontal plane, and the piece of glass put up flat against them, so that the polarized ray could pass through its edges, and be examined by the eye at a Nicholl's eye piece. In its natural state, the glass had no effect on the polarized ray, but on making contact at the battery (7498) so as to render the cores N and S magnets, instantly the glass acquired a certain degree of power *of depolarizing the ray,* which it retained steadily as long as the cores were magnets, but which it lost the

* [7505]





7504. Heavy Glass.

A piece of heavy glass (7485) which was 2 inches by 1.8 inches and 0.5 of an inch thick being a silico borate of lead and polished on the two shortest edges - was experimented with, but gave no effects when the same magnetic poles or the contrary poles were on opposite sides (as respects the course of the polarized ray) - nor when the same pole was on the same side either with the constant or intermitting current. - **BUT** when contrary magnetic poles were on the same side there was an effect produced on the polarized ray, and thus magnetic force and light were proved to have relation to each other. This fact will most likely

The entry recording the discovery of an effect of magnetism on light. September 13, 1845. Par. 7504 (*full size*)

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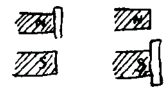
instant the electric current was stopped. Hence it was a permanent condition, and as was expected, did not sensibly appear with an intermitting current.

7506. The effect was not influenced by any jogging motion, or any moderate pressure of the hands on the glass.

7507. The *heavy glass* had tin foil coatings on its two sides, but when these were taken off, the effect remained exactly the same.

7508*. A mass of soft iron on the outside of the *heavy glass* greatly *diminished* the effect.

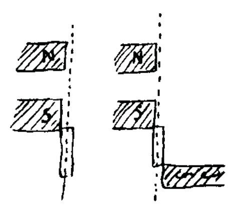
7509. When the heavy glass was opposite the end either of N or S, as thus or thus, then the effect was much less than when it was as figured above (7508).



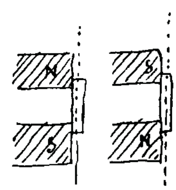
7510. All this shews that it is when the *polarized ray* passes *parallel* to the *lines of magnetic induction,* or rather to the *direction of the magnetic curves,* that the glass manifests its power of affecting the ray. So that the heavy glass in its magnetized state corresponds to the cube of Rock crystal; the direction of the magnetic curves in the piece of glass corresponding to the direction of the optic axis in the crystal (See Exp. Researches, 1689-1698).

7511. By the Negative results obtained with the crystalline cubes (7502, 3) with the present degree of magnetic force, it would appear that crystals have no special power in one direction more than another. But this will want investigation with more powerful magnetic forces.

7512. Now tried various positions of the heavy glass to the magnetic poles. The upper magnetic core, or that marked in the next figure N, was very feeble as compared to S. Must get a better magnet.

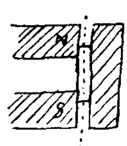


7513. When the heavy glass was in this position, there was no sensible effect on the polarized ray, but when a core of soft iron was held at the outer edge of the glass, so as to determine a certain amount of magnetic curves through it, then there was a trace of effect.



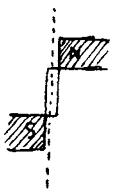
7514. By changing the direction of the electric current, the position of the magnetic poles could be changed—but this made no difference in the effect on the ray of light. Hence it would appear that, provided the magnetic curve and the polarized ray are parallel to each other, it does not matter in which way they proceed. Yet

* [7508]



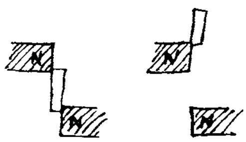
one would rather expect *some difference* in the condition and therefore in the result of things; though the amount of action might be equal, a difference in kind may perhaps be expected.

7515. Now again (7504), put the same Mag. poles on the same side, as thus*, but could not perceive any effect on the ray of light. When the heavy glass was placed thus†, then I thought I could perceive a very small effect.



7516. Placed opposite magnetic poles at the two ends of the heavy glass. Effect very good. I observed the optical effect *before* I examined the magnetic poles, and foretold by it that they were opposite, which I afterwds. found them to be.

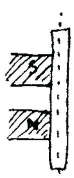
7517. When the N pole was moved into this position‡, then there was no effect on the polarized ray; but when it was moved further on into this positions§, then the effect was as good as ever.



7518. When the same magnetic poles were at opposite ends of the heavy glass, there was no effect on the polarized ray, but when the glass was moved on to the edge and beyond one of the poles, then there was a little action.

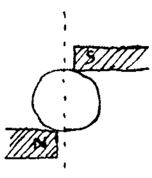
7519. Placing the *same* magnetic poles on opposite side of the heavy glass produced no effect.

7520. All these effects agree well with the general result before expressed (7510).



7521. A larger piece of *heavy glass,* 5 inches square and 3/4 of an inch thick, when tried in the best position, gave a good result. Hence is not peculiar to the one particular piece of glass.

7522. 2 Larger plates of *heavy Glass,* 8 inches square, did not shew the effect, but then their edges want cleaning and polishing, for the ray could not well be observed through them in their present state.



7523. Placed the Magnetic poles thus and certain fluids in a glass between them. When instead of the fluids a piece of *heavy glass* was there, the effect was good.

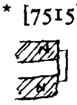
7524. *Oil of Turpentine*—shewed its own rotating effect on the polarized ray but *nothing* in addition.

7525. *Distilled water*—as above (7523). No effect.

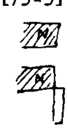
7526. *Dilute Sulc. acid,* 1 Oil vitriol + 3 water—no effect.

7527. *Sat. solution Sul. Soda*—nothing.

* [7515]

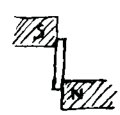


† [7515]



‡ [7517]

§ [7517]



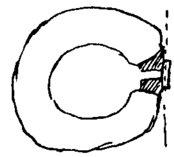
7528. Sat. solution *proto sul. iron* with a little sulphuric acid to render it clear—nothing.

7529. Now went back to some of the other substances to retry them in the favourable condition, as thus.

7530. *Flint glass,* the large piece (7500)—nothing—but many striæ here. Also the cubes of flint glass (7501)—no effect.

7531. *Air*—no effect.

7532. Employed our large *ring electro magnet,* which is very powerful and has of course the poles in the right [position], only they are very close, not more than _____ of an inch apart. When the *heavy glass* was put up against it, the effect was produced better than in any former case.



7533. *Air* and the Ring magnet—no effect.

7534. *Flint glass*—the large piece (7500), or the cubes (7501)—nothing.

7535. *Rock crystal cube* (7502)—nothing, in any of the three directions.

7536. *Iceland spar* cubes (7503)—nothing.

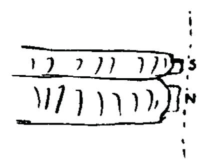
Have got enough for to day.

16 SEPTR. 1845.

7537. See as to Magnetic induction, etc. through *crystals,* 4919—4924; and for the crystals, 4657-4885, especially 4773.

18 SEPTR. 1845.

7538. Have now borrowed and received the Woolwich Magnet, a cylindrical Electro-magnet far more powerful than ours. When in action it holds easily a half hundredweight at each end of the core, and almost a second half hundred besides. This magnet and ours were arranged thus, and excited by five pair of Grove's battery, and the poles were N for the large magnet, and S for ours. Polarized ray as before.



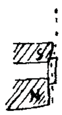
7539. First wrought with the original piece of *heavy glass.* This and many other pieces of heavy glass which I have are numbered and correspond to a catalogue giving their composition. In speaking of these glasses, I will put this number in red ink[1], which

[1] These numbers are printed below in italics.

number will refer to the list over leaf[1] (7540). The first experiments to day were made with No. *174*.

7540. Glasses (heavy) No.

No.			
114.	Crystd. Boracic Acid 2;	litharge 4;	Red lead 2.
116.	Do.	Do.	Do.
119.	Boracic acid 1;	nitrate of lead 1;	Silica 1.
131.	1 Prop. of B: A:	1 oxide of lead;	1 Silica.
167.	Do.	Do.	Do.
168.	Do.	Do.	Do.
174.	Do.	Do.	Do.
186.	Do.	Do.	Do.
192.	Do.	Do.	Do.
212.			
216.			

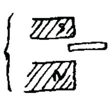


7541. Heavy glass (original or *174*) when placed thus, produced a very fine effect. The brightness of the image produced rose gradually, not instantly, due to this, that the iron cores do not take their full intensity of magnetic state at once, but require time, and so the magnetic curves rise in intensity. In this way, the effect is one by which an optical examination of the Electro magnet can be made—and the time necessary clearly shewn.

7542. When the piece of glass was put edgeways between the magnetic poles, there was scarcely a sensible effect.

7543. When contact is broken of the electric current (7505), the image disappears far more suddenly than it rose. Shewing in some degree the state of tension in which the iron is held by the Electro current.

7544. A second piece of the same heavy glass (7541) produced the same good effect, and also a third and a fourth piece. As all these pieces were not from the same original plate, but from two plates, the effect shews that the result is not peculiar to the first piece that I used.

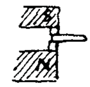


7545. One of these pieces, 174, was oblong and polished on all sides and edges; its dimensions were 0·42 of an inch thick; 1·3 inch broad; and 2·2 inches long. On being placed with its thickness in the course of the polarized ray, its effect was scarcely sensible; when its breadth was in the course of the ray, the effect

[1] i.e.par. 7540.

was good; but when its length was in the course of the ray, the effect was best and nearly twice as much as with the breadth.

7546. When this piece of glass was put between the Mag. poles edgeways, as just said the effect was almost nul; but when two pieces of iron were introduced, so as to extend the Mag. poles towards each other until they touched the glass, then the effect was good.



7547. So when the same piece of glass was put broadways (7545) and then the Mag. poles built up to it with Iron, the effect was greatly increased.

7548. These effects shew the influence of increasing the intensity and concentrating the force of the magnetic curves.

7549. By building up the Magnetic poles on each side of the glass, *above* and *below* the line of the polarized ray, so as to get the ray into the middle of the magnetic curves, the effect was greatly improved.

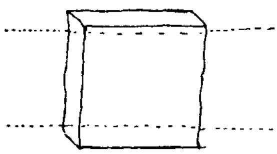
7550. I find that the *new quality or force* impressed on the heavy glass by the Magnetic curves is a *circular polarizing force*—for when without the Magnetic curves, the Nicholl eye piece is in that position which extinguishes the polarized ray—and when by inducing the Magnetic curves and peculiar state the image becomes visible, then revolving the Eye piece a certain quantity extinguishes the image. On taking off the magnetic influence an image again appears, and to put this out the Eye piece has to be revolved back to its first position.

7551. Further observed that when the Magnetic influence was exerted on the heavy glass, and the Eye piece so far revolved as to extinguish the image, that then further motion in one direction (downwards of the Nicholl handle or index), in bringing into sight an image, gave it of a *red colour*—and on the contrary, that on revolving the eye piece in the other direction (or raising the handle) produced an image, but of a *blue* or complementary colour.

7552. Are not these the properties of the circular polarization of quartz, as distinguished from those of oil of turpentine or fluids?

7553. Now worked with *various specimens of heavy glass,* of which the following are the numbers (7540)—*all gave very good effect*. Hence plenty of cases of action.

114	feebly rather.
116	
119	
131	
167	
168	glass very dark from reduced lead.
174	the piece first used.
186	very fine effect.
192	
212	
216	



7554. A large piece of heavy glass, 8 inches square, had a very good but peculiar effect. Two edges were polished and therefore I could examine it near the two unpolished edges. In these parts it had naturally a certain power of rotation belonging to it. This power was *increased* by the magnetic force or curves in *one direction* but *diminished* by them in the contrary direction; i.e. if the glass was turned round end for end, the ray always passing along the same line, these contrary effects were produced. This happened with *both edges*. Must examine this hereafter (7677). Is nothing.

7555. Proceeded to examine *different substances* and search for the property in them.

7556. *Flint glass.* The square plate (7500, 7534) shews the action: but it is so full of striæ and irregular in tension as to give much depolarizing effect by itself, and so hides the Magnetic effect.

7557. Three cubes *of flint glass* were examined—all were in such an unannealed state as to give depolarizing effects naturally (7501); still, *the effect* was visible in one of them.

7558. *Rock crystal.* The cubes (7502, 7535) tried in all six positions of each, but obtained no effects, probably because mass not large enough or Mag. curves strong enough.

7559. *Iceland Spar.* The cubes (7503, 7536) tried in all positions, but no effects produced.

7560. *Sulphate of lime.* A clear crystal held in the ray in the position which does not affect it—then the Magnetic curves put on—but obtained no effect. A plate of clear sul. lime tried in the same manner without effect. The thickness of the crystal not more than 1/3 of an inch, and that of the plate very small. So no extent of mass here for the action of the magnetic forces.

7561. *Water* distilled—in a small square glass cell—the extent of water in the direction of the ray or Mag. curve was 3¼ inches. It gave indications of the peculiar action on light—feeble—but very distinct to my eye.

7562. *Dilute Sulc. acid.* 1 oil Vitriol, 3 water in the same cell. Sensible effect—but not better than water, if so good.

7563. *Absolute Alcohol*—in the same cell—think there is action, but is less than with water.

7564. *Ether*—in same cell. Not a sensible action.

7565. *Camphine* or *oil turpentine*—in the same cell. First revolved the Nicholl eye piece (to the right (7607, 7609)) to compensate the rotating power which the camphine itself possesses in its ordinary state, and obtained a feeble coloured image. Then put on the magnetic curves and obtained *the effect;* i.e., I could either deepen the red colour of the image or alter it from one colour to the other, according to the condition of the image in the first instance.

7566. Examine as to the colour oil of turpentine images have of themselves, and compare it with colour of these images obtained by Magnetic curves (7551).

7567. *Air.* Could obtain no effect.

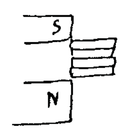
7568. *Glass.* Should be perfectly well annealed to shew this effect. Is there any real magnetic relation between this circumstance and the fact that iron when most annealed takes up best the Magnetic effect?

7569. Is it possible that similar electric currents are circulating both in the particles of the Iron and the particles of the glass? Or rather, perhaps, may it not be that in the iron there are circular currents, but in the glass only a tension or tendency to circular currents?

7570. In this supposed analogy, may for the *electric currents* substitute their *equivalents of Magnetic force.*

7571. Now experimented with HEAVY GLASS only, to make out the circumstances and laws of action.

7572. Four pieces of heavy glass were put together so as to make up a depth, for the passage of the ray and mag. curves, equal to



the length of one of the pieces—the contact surfaces were wetted with water, so as to get rid in some measure of repeated reflections of light and consequent loss. The effect was *very good,* and though not so good as the effect of an equal depth of one piece, was I think comparable to it, the difference being due to the transparency of the one piece as compared to the dullness of the compound mass.

7573. Took away one piece and left *three;* the effect good but less than of four.

7574. Took away a second piece and left *two*—the effect still less.

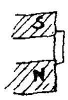
7575. Took away a third piece and left *one*—the effect small but sensible (7545).

7576. It appears therefore that the *mass* of the *dimagnetic* may be in several pieces and does not require to be continuous—that, as in depolarizing bodies as Oil turpentine, sugar, etc., the depth of the substance determines the effect on the ray.

7577. I find that it is easier for the eye to distinguish the effect of the new power conferred on the *dimagnetic* when the image is (by the Nichol eye piece revolution) rendered slightly visible upon one side or the other of utter darkness. The Magnetic curves then cause increase or diminution of the light of the image, and either is more sensible to the eye than the effect when one begins to observe with a dark field of view (7635).

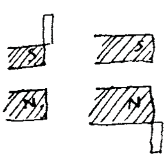
7578. Must observe bodies feeble in power in this way.

Effect of Positions of Glass.



7579. The former best position still best with these powerful magnets (7538), and the effect excellent. When the image was made a little sensible by elevating the Nicholl eye piece handle, the Magnetic curves made it far more sensible.

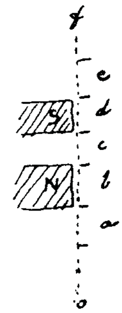
7580. When the glass was thus* or thus†, there was no sensible effect.



7581. But when carried on further on either side, i.e. beyond the S or the N pole, then there was effect. It was, however, the reverse of the former effect, for the image which was brightened by the glass in this position‡ was darkened by the glass in either of the positions just given.

7582. If I brought in a faint image by raising the handle of the eye piece, and then made the glass travel from *o* to *f,* when at *a*

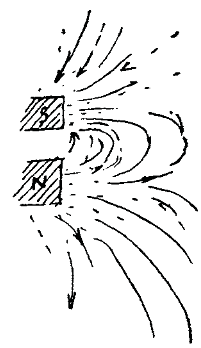
* [7580]  † [7580]  ‡ [7581] 



it darkened the image—when at *b* the image was in its natural state—when at *c* the image was brightened—at *d* it was natural and at *e* darkened again. Or, if I adjusted the eye piece so that there was no image in the normal condition, then the glass at *a* gave an image which, as the glass gradually moved along the line, first diminished and gradually disappeared entirely; then the contrary image appeared and reached its maximum brightness (in position *c*), after which it diminished, disappeared and was finally replaced by an image the same in character as the first.

7583. The same order of effects took place if the glass were moved in the reverse direction, or from *f* to *o*.

7584. I tried the glass in all position[s] in relation to itself by turning it round, but that made no difference; the effect is due, not to any permanent condition of the glass, but to its position among the magnetic curves.



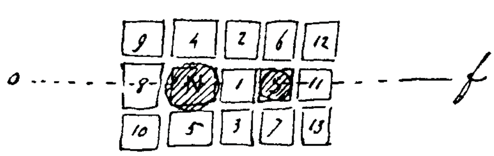
7585. This shews what I had anticipated and was sure must happen, that in a polar power like electricity or Magnetism, turning the ray or the curve (Magnetic) end for end must make a difference equivalent to N and P[1] magnetism or Pos. and Neg. Electricity, or any other form of expression which represents + and —. It is easily seen that the magnetic lines of force at *a*, *c* and *e* go through the glass parallel to the polarized ray, but those at *b* and *d* go across it. So the positions of *b* and *d* are nul in their effects on this ray, but the others are active. In these three positions, however, there are differences, for the magnetic lines of force at *c* have a direction which is exactly the reverse of that of the lines at *a* and *e*. This change in the direction of the magnetic force reverses the direction of the circular polarization of the glass in the different positions.

7586. So it is clear and consistent that, if the Magnetic poles were both changed, then the direction of rotation in the glass in its central or best position would be changed also.

7587*. Now placed the glass in various positions, which may be easily understood by the figure, in which a front view of the ends of the Magnetic poles is given: *o f* is the line of the polarized ray, and the squares represent different places of the glass plate, through

[1] Query S.

* [7587]

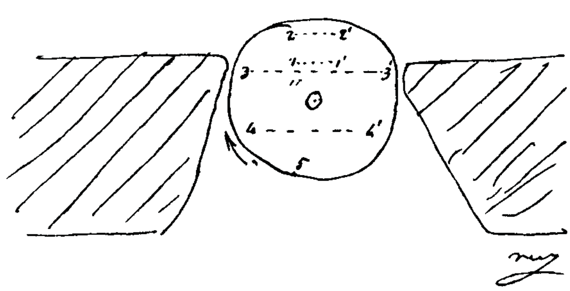


still the parts between, which travelled, did not cut or intersect magnetic curves but went along them or in their line.

8975*. It is evident that if the part of the disc between the wires were to cut or bisect the lines of mag. force, currents would be produced and carried off to the galvanometer. As it was, weak current[s] were produced and these may be important and must be examined. They were shewn to be independent of friction, or thermo effects, or of the battery, or of any things else except the magnetic force, for when that was off, the currents instantly stopped. Their general nature was as follows. When the ends were applied at 1, 1′ there was a certain current—at 2, 2′ there was the same kind of current—at 3, 3′ also the same current but stronger—at 4, 4′ the same current—at 4, 3′ less current but the same direction—at 5, 3′ same current but very weak, and at 5, 2′ no current; at 5, 2 the reverse current began feebly to appear. These were all with the rotation in a certain direction. When the rotation of the disc was *reversed,* the current[s] were produced just as stated, but their *direction* was also reversed. The currents were not apparently from or between the center and the circumference, but across the whole diameter of the disc. The horizontal diameter was that in which they were strongest, and one near the vertical diameter that in which they were weakest. I do not as yet see how these can be produced by cutting the magnetic curves, but they may be, and that must be examd. If they are, then a little adjustment in Azimuth ought to shew it. Examine this.

8976. A cubical glass cell about 1½ inches across was placed in the Magnetic field and then filled with a strong solution of Tartaric acid. A polarized ray was sent through this tartaric acid at right angles to the direction of the lines of magnetic force, and a strong ray of lamp light, unpolarized but condensed by a deep convex lens, was sent through it *parallel* to the lines of magnetic force. Then the rotating power of the acid upon the polarized ray was *first observed and noted*—after this the cross abundant ray was sent through the tartaric acid. It produced not the least effect on the first ray. Lastly the magnetic force was superinduced; still the first polarized ray remained entirely unaffected. So the rotation in one direction does not seem at all affected by calling into action

* [8975]



the rotation in a cross direction or by throwing on the Magneto rotation. Seems to be no division or deviation of forces.

8977. I should like to make this experiment with a cross ray of powerful sun light and also with that polarized—using oil of turpentine for the fluid.

8978. *References-*

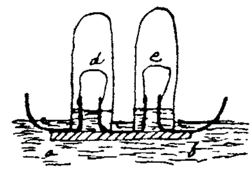
Arago—chem. action of light—Comptes Rendus, xvi, 402.

Becquerel. Effect on bodies by sun's rays. Do. xvii, 882.

25 MAR. 1847.

8979. *Platina wire ignited in Hydrogen* (9275).

Grove says that Hydrogen prevents in a great degree the ignition of platina. What is the cause?



Made the following expts. *a b* is a board, *c, c, c* are three thick copper wires fixed to the board, *d* and *e* are two equal lengths of the same platina wire which, when a Grove's battery was connected by the system, became equally ignited in the air. Two jars (glass) equal in size were placed over the wires *d, e* in a water trough—the jars containing either the same or different gases. When they contained the same gas, the Electric current ignited both wires equally; when air was in one and nitrous oxide or carbonic acid in the other, there was but little difference—but I think that the wire was a little hotter in *Ns. oxide* than in Air and also in *Carbonic acid* than in air.

8980. When *Air* was in one jar and *hydrogen* in the other, then the wire in the former was white hot and yet that in the hydrogen was not ignited—so the effect of the hydrogen very distinct. When the battery current was sent through *d* or *e* alone, i.e. without the other, it required only three pair of plates to ignite the wire in *air* fully and *six* pairs of plates to produce the same effect in hydrogen.

8981. When the current was sent through both platina wires, the gas in both jars expanded from the heat of the wire communicated to it, but the expansion was *far the quickest* in the hydrogen jar, though the wire there was not so hot as the other. The common air also expanded but was full twice the time before the expansion ceased. When the current was stopped, the hydrogen also contracted much quicker than the air, though both at last returned to their original volume.

8982. BUT the amount of the expansion was greatest in the air—least in the hydrogen, perhaps in the proportion of 5 to 3 or thereabouts—and so here a key to the whole of the effects.

8983. The hydrogen both abstracts and communicates heat quicker than the air, which may be due conjointly to its great relative mobility, as shewn in the issue through apertures, and its capacity for heat. Hence it can more quickly carry off the heat of the wire—and so keep it cooler than the wire in air—hence also its quicker expansion—hence also its quicker contraction, because it can give heat more quickly to the glass jar which is the cooling agency—and hence also its smaller amount of expansion on the whole—for though it receives faster than air, it also communicates faster than air, and the cooling therefore at the surface of the jar is quicker then than in the air jar, and so the wire is kept at a lower temperature.

8984. The initial condition is soon acquired and then the hydrogen is less expanded than the air, and the wire in it is comparatively cold.

8985. As to the *rays of light* emanating from an *ignited* wire stretched between the poles of the great magnet and parallel to the lines of Magnetic force.

8986*. The rays proceeding from this wire at right angles to it were received on a prism (triangular), refracted and examined; no difference appeared between the effect whether the magnetism was *on* or *off*—or whether the prism was applied as at *a* or at *b*. No indication of tangential rays. Colours good.

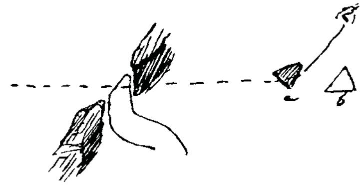
8987. A piece of heavy glass was place[d] in the magnetic field close to the wire and between it and the prism. This caused no change.

8988. A screen of copper with a horizontal edge was placed near the wire, so that the rays from the under side could be cut off (by moving the head) before those of the upper disappeared—still no change.

8989. By a similar screen the rays from the upper edge or side were cut off first—but still no effects peculiar.

8990. The heavy glass was interposed, but with no effect.

* [8986]



8991. A thick copper edge was interposed but with no effect.

8992. An edge of heavy glass was interposed, but with no effect.

8993. A copper screen with a *vertical* slit was set up before the wire and the ray passing through it examined in all directions by the prism, but nothing particular was observed.

8994. Heavy glass was placed in the Mag. field in the course of this ray, but still there was no particular effect.

8995. All the above variations were made, and the ray being polarized by a Nicoll's eye piece, both in a vertical and a horizontal plane, was then examined by a second eye piece or analyser—no particular effects were observed.

8996. The direction of the magnetism was changed, but still no new or particular effect was observed.

8997. Therefore no effects here as if any thing like *tangential* rays issued from the wire. All seemed to be direct, and in whatever direction they flowed from a given spot in the wire, still to be the same.

22 APRIL 1847.

8998. Iodide of nitrogen is very well made by putting a quantity of Iodine into water—rubbing it in a mortar into powder and then adding ammonia by degrees until it is in great excess—rubbing at intervals to break up the iodide—then pouring off the solution and putting on fresh ammonia, and leaving it until the next day. The iodide seemed well made and there was very little colour in this second ammonia—shewing that the action was complete in the first instance.

8999. Whilst thoroughly wet, the iodide appears as if it might be rubbed in the mortar or on wood, etc. without any risk of exploding. Still, on stirring up a quantity that had been left in a glass with ammonia for several days, using a glass rod for the purpose—there was once a feeble decrepitation, as if a small part of the mass had gone off. But I think it must have been a grain like a crystal which had gradually formed and aggregated—and not an explosion of the *powdered iodide.*

9000. The iodide will keep in sol. of ammonia for a long time; neither then does gas seem to be sensibly given off from it and the ammonia becomes brown only very slowly. The brownness seems due to this, that as decomposition goes on, the iodine set free combines in part with hydrogen to form hydriodate of ammonia, and this salt in solution can dissolve iodine though pure ammonia cannot.

9001. Some Iodide made January 21, 1847, was kept in solution of ammonia until this day (three months)—being put on paper and dried, it was excellent in quality and thus can be very well kept without danger. Some portions of it, when dried, were very explosive—one of these, being dropped into water and gradually wetted, could then be rubbed and struck without exploding; so that whilst wet it seems very safe at common temperatures.

9002. When the *Iodide of Nitrogen* is kept in pure water, it seems to decompose slowly and more quickly than if in ammonia—the water then becomes brown—but the insoluble portion is still the *iodide of nitrogen,* and in this way it may be kept for several months.

9003. When put into a tube with the water (plenty of water) and heated, the iodide begins to decompose at a heat of 100°—150° with numerous sharp decrepitations—gas (nitrogen) is evolved and iodine set free. If there were much iodide, it might in such a case explode simultaneously and with violence. It is easy in this way to tell whether the black powder is iodine or the iodide of nitrogen. (Alcohol also distinguishes the two, dissolving the iodine and not the iodide.)

9004. If ammonia be added to a coloured aqueous solution, it acts on the iodine (free), and the liquid which floats above the powder is colourless.

9005. A portion of the *iodide* with water was washed with alcohol once or twice and left at last in strong alcohol. The iodide mingled freely with the alcohol—the alcohol soon became coloured—and I think the iodide decomposes faster in it than in water. It was left until the next day. The solid matter was iodide an hour or two after the Alcohol was added. The next day the Alcohol was very brown and the solid matter almost entirely gone—the little that remained contained iodine and did not decrepitate.

9006. Some of the Iodide of nitrogen, moist, put into *ether* and shaken about in it. Gradually the water was removed from it and then the body was as a heavy insoluble powder in the ether, wetted by it—and acting on it much as it acted on alcohol, i.e. slowly giving a coloured solution. By the next day, the solid was all gone and also much of the ether, and the fluid remaining was very brown with iodine.

9007. In the same manner a portion of the Iodide was put into *pyroligneous ether*. The action was like that on alcohol. Next day, no solid was left, but a dark ioduretted fluid—all iodide decomposed.

9008. Put some of the wet paste of iodide into *oil of turpentine*. There was no signs of action. The two would not mix, and even the next day the oil of turpentine was not coloured, but the iodide seemed nearly gone and the water which had wetted it remained as a drop at the bottom of the other fluid.

9009. Added *Acetic acid* to a little of the iodide with water—immediately there was much action and the fluid upon mixture was filled with minute bubbles of gas (nitrogen). The solid iodide disappeared far more rapidly than in water or alcohol, and each of the solid particles, as long as they remained, produced in decomposing little bubbles of air (nitrogen). The colour of the acetic acid soon deepened from the presence of free iodine. It was very evident that the acetic acid very much hastened the decomposition of the iodide.

9010. Solution of *Tartaric acid* had the same effect.

9011. Dilute *sulphuric acid*—had the same effect. Hot dilute Sulphuric acid dissolves a little iodine; on cooling it is deposited, and the fluid becomes colourless.

9012. Dilute *Muriatic acid* acts as the former acids but more rapidly. The solid body is quickly gone.

9013. Dilute *Nitric acid* acts more quickly than any of the former acids. The iodide was very quickly gone, but a little solid iodine was left for a time.

9014. A solution of *chlorine*—the iodide is rapidly decomposed—a gas (nitrogen) rises rapidly and a solution nearly colourless is soon produced—probably of chloriodine.

9015. A solution of Potassa easily wets the iodide—the action is slow but decomposition does go on and gas rises from the particles. As long as any black powder remains, it is *iodide of nitrogen*.

9016. I have not been able to find any substance as yet that can dissolve the iodide of nitrogen.

9017. Zinc in contact with the substance under water acts slowly and gradually appropriate[s] the iodine.

24 MAY 1847.

9018. Placed some good iodide of Nitrogen between platina electrodes (of a Grove's battery of 5 pair of plates) the fluid between being sometimes pure water—sometimes Ammonia strong solution—and sometimes Alcohol. Also let the iodide rest alternatively on the Positive and the Negative Electrode—but in no case could I get any particular or useful results as to the *iodide* of nitrogen. It appeared to be a non conductor and was affected only in the ordinary way, or by the bodies evolved by the battery.

9019. Also made the Neg. Electrode, when in contact with the iodide, pure mercury, but obtained no useful or new results.

9020. No signs of resolution of the iodide into any new substances in these experiments.

JUNE 26, 1847. AT OXFORD.

9021. Sir William Hamilton and self talked over the relations of two electric currents at right angles to each other, when, according to Ampère, they have no mutual action. I have expected some effect between them analogous to that state of magnetism which must be the equivalent of static electric induction, but could never discover any: Sir William Hamilton, I find, expects an effect on mathematical principles. Must try again in various ways.

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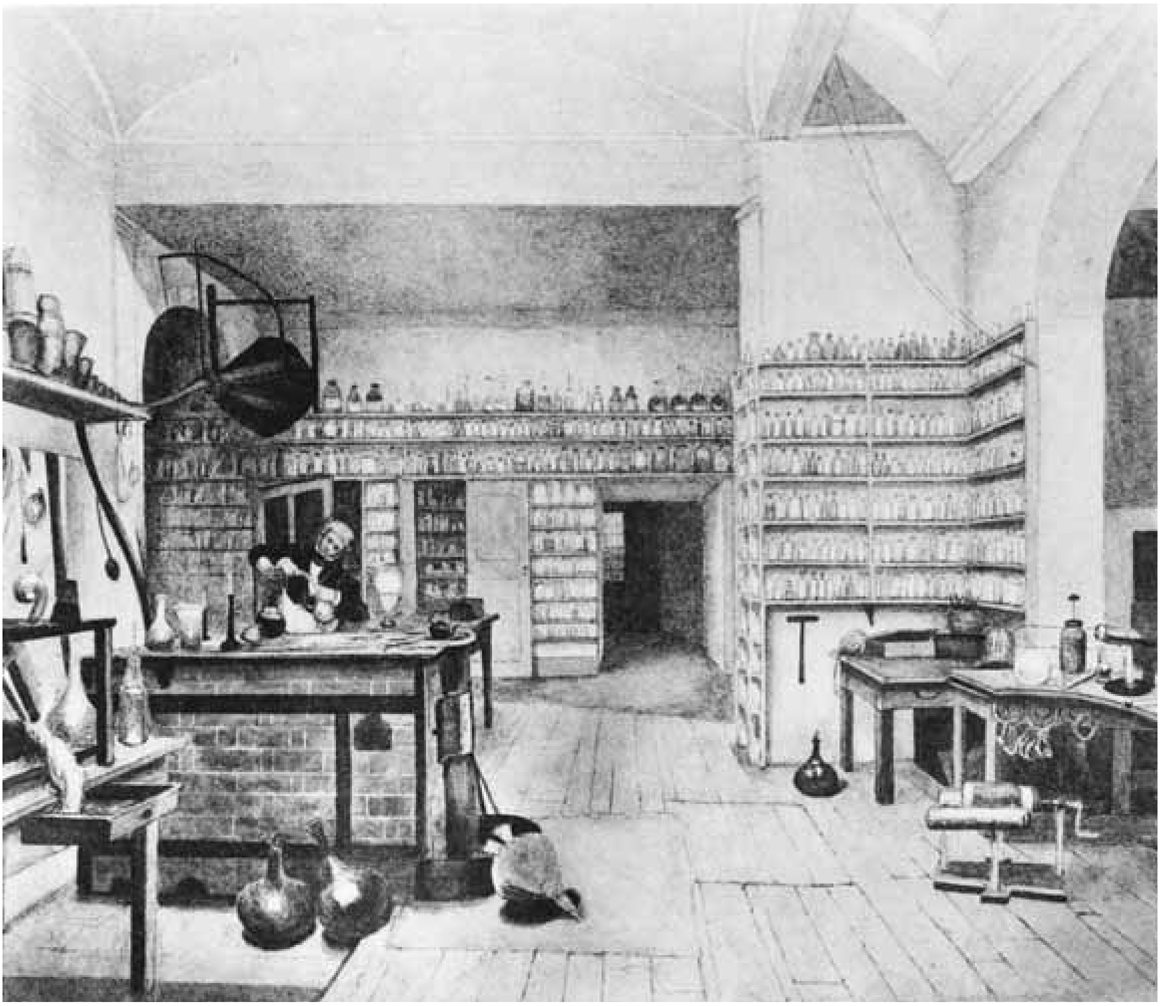
FARADAY'S DIARY

VOL. V



FARADAY IN HIS LABORATORY AT THE ROYAL INSTITUTION, 1852
From a water-colour drawing by Harriet Moore, in the possession of the Institution

FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

made by

MICHAEL FARADAY
D.C.L., F.R.S.

during the years 1820 – 1862
and bequeathed by him to the
ROYAL INSTITUTION OF GREAT BRITAIN
Now, by order of the Managers,
printed and published for the first time,
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SIR WILLIAM H. BRAGG, O.M., K.B.E., F.R.S.
Director of the Laboratory of the
Royal Institution

VOL. V

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SEPT. 6, 1847 – OCT. 17, 1851

HR DIRECT

2008

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FOLIO VOLUME V
OF MANUSCRIPT
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9022. Have arranged a lime light, using oxygen and hydrogen, and have arranged to gather a good bundle of the rays by means of convex glasses, so as to give a good ray which could be sent through a helix of 500 feet of wire, either free or between the magnetic poles. The Iron poles were pierced so that the ray pass in the middle of the magnetic field.

9023. Have also arranged an interceptor, which could cut off the ray and then let it pass again, and which by its connexions with the helix through which the ray had to pass and also with the galvanometer, could intercept and renew the circuit—so that the circuit could be completed either as the rays began to pass through the helix and be interrupted when the rays were cut off—or vice versa, and that with great rapidity, so as to have very many alternations in a minute or even in a second. Perhaps 400 or 500 in a second—quite able for that. 1 revolution of great wheel = 360 interceptions.

9024. On proceeding to experiment, it was found that a more perfect insulation of the wires and helix proceeding to the galvanometer must be made from the Magnet and battery before the experiment could be made in the magnetic field. Otherwise the arrangement was good.

9025. The lime light ray in its common state was sent through the helix with air in it—also with heavy glass—prism of rock crystal—and a tourmaline with one side cut away parallel to the axis—but in no case was there any effect produced at the Galvanometer. The circuit was in all these cases ascertained to be perfect for *each* experiment by trial with a thermo electric circuit.

9026. The ray was polarized by a Nicol eye piece—but still no effect.

9027. The ray was in all these cases intercepted by the instrument, but neither at the rising of the ray or ceasing of the ray in the helix and media was any thing produced.

SEPTR. 7, 1847.

9028. Have had the interceptor, helix and parts included in the galvanometer circuit insulated by ivory attachments, so as to preclude derived currents. Yet when all in place and the battery on to the magnet, there was an effect at the galvanometer—the needle joggled about to the right and the left, 10° or 15°, and came to a stand suddenly and quickly changed its direction, as if there were a series of irregular feeble currents generated in the helix between the poles. By further insulation of the wire and parts, I am almost sure these were due to variations in the magnetism of the great magnet, and these perhaps due to some variations in the intensity of the current of the battery, caused perhaps by the fluid in the cells, etc. I cannot say distinctly. Sometimes these effects at the galvanometer were more and sometimes less. Allowance was made for these as well as could be in the following experiments.

9029. *Clear heavy glass* in the helix of 500 feet of wire (9022) either in the magnetic field or out—with the lime light (9022) on or off—ray intercepted or not (9023)—polarized or not. Gave no signs of an electric current under any of these variations by the influence of light.

9030. The dark green solution of proto ammonio tartrate of Iron (9037) was put into a tube with flat plate ends and that into the same core—it was so dark that no portion of the light appeared through it. Yet whether in or out of the magnetic field—with common or polarized light—continuous or intermitted—beging. or ending ray—there was no sign of a current due to the action of light.

9031. A prism of *dark quartz* in a similar helix under all the above variations gave no result.

9032. A *piece of heavy glass*—dark from reduced particles of lead within—under the same circumstances gave nothing.

9033. I think that I may trust the reality of these negative results, notwithstanding the small variations of the galvanometer (9028). Perhaps it might be worth while to try the sun's ray some fine summer day with the heavy glass and the Tartaric rotating solution—and then also use either Violet or red ray alone.

Reflexion of light from a wire.

9034. Some fine flat silvered copper wire, such as is used for whip handles, etc., was procured and 430 feet of it was wound round a flat smooth board about a foot square, in separate lines, resembling a deformed helix of which the strands or part were out of contact with each other. The ends of this wire were then connected with a delicate galvanometer, and one of the faces of the board exposed to the sun's rays. In this way, about 210 feet of the wire could be submitted to the rays either in a vertical or oblique position, and with the rays along or across the wire, but no trace of any electric current due to the act of reflecting the light could be in any case perceived.

9035. Then the silver wire on one side the board was browned by a solution of sulphuret of potash, so as to cause absorption of great part of the light—but still no trace of any current produced by the act of absorption could be perceived.

9036. Still, I think, there must be some relation between these functions of light and electric forces.

9 SEPTR. 1847.

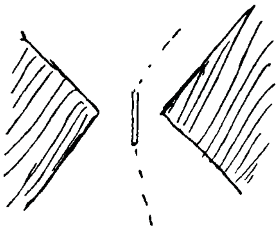
9037. When Tartaric acid and water are made to act for a few days on iron filings—and then ammonia in excess is added till resolution of the precipitate occurs—a dark green solution is procured—which is a good absorber of light—is magnetic—and probably rotative. By leaving this solution exposed to air, it became red brown at the surface as the iron peroxidizes and this action after some days extends to the bottom of the liquor.

15 OCTR. 1847.

9038. Experiments on the reflection of light by a flat wire carrying a strong voltaic current in the Magnetic field.

9039. It did not seem impossible or improbable that whilst a metal carried an electric current, its condition might influence the reflective force of the particles at its surface. A platina wire, fine, was therefore rolled flat and soldered on to ends of two thick and bent copper wires in such a manner that, whilst the latter being fixed firmly in a block of wood supported the platina wire, the platinum wire could be placed in any direction between the magnetic poles. The platinum wire was about half an inch long, and was heated by a battery of 5 pr. Grove's plates, which could easily raise it to whiteness if needed.

9040. The Electro magnet was served by 10 pr. Grove's plates and was very powerful, being the large Magnet with poles placed

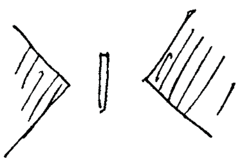


anglewise so to increase the forces. In the first instance, the platina wire was placed across the magnetic axis and then an ordinary ray of light sent down upon it from a lamp and the reflected ray observed either by the naked eye or by a Nicoll's Eye piece. But whether the magnet was on alone or the Electric current through the wire alone, or both on together, no effect on the ray could be observed, either whilst the actions were continued or intermitting, or at the beginnings or endings.

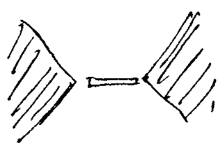
9041. In the first case the ray was sent in a vertical plane parallel to the wire and the incidences were varied from high to low angles—but there was no effect in any case.

9042. Then the incident ray was polarized by a second Nicol's eye piece and the reflected ray observed in several positions of the examining eye piece—but no effect under any combination of the magnetic and electric force was produced.

9043. The ray was polarized in a vertical plane and also in a plane at right angles to this (the ray *always* being in a vertical plane parallel to the wire) but still there was no effect.



9044. Then the wire and the magnet, being still in the same position, the *ray* was made to pass in a plane transverse to the wire and consequently parallel to the magnetic axis—and all the former variations of the ray were gone through, but without producing any sensible new phenomena.



9045. After this the wire was placed in the Magnetic axis and the ray sent parallel to it, but no effect could be produced under all the former variations of the experiment.

9046. Lastly, the wire being parallel to the Magnetic axis, the ray was sent across it, or transverse to the axis—but without any sensible effect on it.

9047. The light from the wire itself (when heated enough) was observed under all these conditions, but in no case did it seem affected.

9048. Whenever the Magnet were first made and then the electric current sent through the wire, there was a sharp small metallic sound from the Mag. field, but it was difficult to say whether it came from the wire or from the poles of the magnet. It was manifestly due to their joint action, for making the magnet active alone, or sending the electric current thrgh. the wire alone, did not produce it. If the current was sent thrgh. first and *then* the Magnet made effective, there was *no* sound—but probably because the magnetic force is not generated suddenly as the electric current is, and suddenness is doubtless necessary to the *sensible sound* (9099).

9049. I removed the fine platina wire and replaced it by a moderate wire, and also a coarser wire, but in neither case was there any of the peculiar sound—as if the fine wire or concentrated current was far the most effectual.

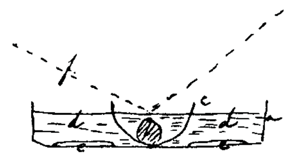
9050. The magnet action being continued, there was the sound both on *making* and on *breaking* contact—and the latter is the most distinct.

9051. The phenomenon is the same, I conclude, as that which De la Rive has described.

9052. The sound occurred whether the fine platina wire was transverse or parallel to the magnetic axis.

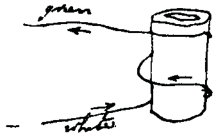
23 OCTR. 1847.

9053. As to reflexion of ray, arranged a glass basin with a glass rod across it from side to side, so as to divide it into two places, and then put in a mixture of 1 vol. Sulc. acid and 4 vols. water until it just covered the glass rod. In this case there were 2 masses of Electrolyte connected by a thin portion covering the rod. Two platinum plates were in the masses, to be connected at pleasure with a battery of 10 pr. of Grove's plates. A wrapper of bibulous paper was put round the Glass rod, so as to keep the part of the fluid within it clear from bubbles arising from the plates; and reflexion of the ray was effected at the part over the rod, where the Electrolyte was thinned and consequently the current most powerful.



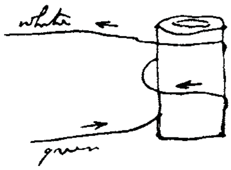
9054. Above, *a* is the basin, *b* the rod seen in section, *c* the bibulous paper, *d, d* the electrolyte, *e, e* the two electrodes, and *f* the ray.

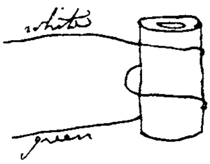
9055. Now whether this ray were at the polarizing angle with the surface of the fluid—or above or below it considerably—whether it were a common ray or a polarized ray—and whether examined by the naked eye or an analyser, *no difference of effect* could be



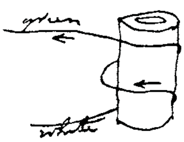
the reverse (in the wire) of that produced by the Electro thermic effect (10065) or the same as that figured in the margin.

10069. The helix with its copper core was now inverted, so that the white wire end was uppermost. Now, on ascending, there was nothing clear, though perhaps a trace of needle movement to the left hand—but on descending am almost sure there was motion about the 1/4 of a degree to the *right hand*—stopping at once as the descent of the core and helix was stopped. This is contrary to the direction of the former motions (if they were real); but still, that should be the case, for now the current ought to be reversed in the Galvanometer wires if due either to Gravity or to terrestrial magnetism, that it may be *the same* in the helix in relation to its position in space, or rather in respect of the lines of Gravitating force. This is easily seen by the diagram and gives great encouragement—but I must very carefully verify the effect and discriminate the true cause.





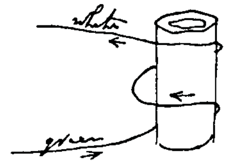
10070. Now removed the copper core and used the helix alone with the white end wire uppermost. The result was very doubtful, but if there was any effect, it was the same in kind as in the last experiments, i.e. as if the core were there. I think there was effect but am doubtful. It may well be that there should be effect, for one part of the coil may act as core to the other parts, just as a current of Electricity in a wire, when stopped, induces in the carrying wire exactly as it can do in a neighbouring wire.



10071. Reversed the position of the helix so as to have the green end of wire uppermost, but no core. Very little action, if any. But on using stronger spectacles, think I can distinguish an effect. The galvanometer needle seems to go to the *right* whilst the helix is descending, and to the *left* at stopping, and the effect is rather plain, unless I deceive myself. This would still indicate the same undeviating and consistent effect.

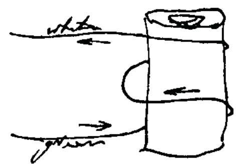
10072. When using glasses, it is very necessary to be careful of any magnetic effect they may produce: but being aware of the effect, it is easy by perfect stillness at the galvanometer to avoid error.

10073. I now changed the ends and placed the helix as before (   )—there is a trace of effect—less clear than in the last position (10071). The galvanometer needle was deviated to the left during *descent,* as figured in the margin, where the current is recorded by the arrow—and was to the right at the stopping. All this is consistent.



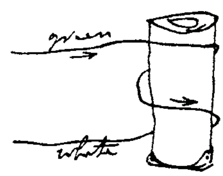
10074. Put a *bismuth core* into the helix: it was about 3¾ inches long and about 3/4 of an inch in diameter. When the white wire end was uppermost, there was no clear effect on the ascending of the helix and core—but on descending, a little effect, the end of the Galvanometer needle moving a little to the right at the end of the descent. The helix falls with a sort of blow on the cushion. I think it is not possible that this blow on the table can mechanically cause the effect on the needle.

10075. Reversed the helix and bismuth, so that the green end upwards; there were traces of motion on the Galvanometer needle to the *left* at the end of the descent, and then they stop, as the motion of the helix stops. It is not to the right as before, but the blow on the table is as before and therefore that can not cause the effect in a mechanical manner. The two effects are consistent and as, when the helix has white end wire uppermost, the Galvr. needle went to the right at the end of the descent, so the current if it existed must have been as in the figure, and consistent with all the former results.



10076. It is to be remembered that the core can probably shift a little at the moment of the fall on the cushion. Perhaps the motion of the core *through* the helix may be an important condition—perhaps not. If there is any induction of an electric current, it can only be in the vicinity of the rising or falling matter.

10077. Now employed an *iron* core in the helix; it also was 3¾ inches long and 3/4 of an inch in diameter, being the size of the bismuth core—the green end wire of helix was [uppermost]. The effect was I think produced, but as little as in any former case. As helix descends, the needle motion, if any, was to the right, or as marked in the diagram—it was to the left at stopping.



10078. I reversed the helix and core, so that the white end was upwards. The effect was very trifling, but I think needle end went to the left as the helix descended and then at stopping of the latter went to the right.

10079. Now these results with iron are consistent with each other —but give *opposite directions* to the former results. They must therefore be carefully repeated. If true, they will perhaps shew two things. First, that the magnetic effect of an iron core is very small indeed, and cannot produce or account for the former results. Next, that if Iron produced any magnetic effect, it was probably an effect in the contrary direction to that produced by Gravitating force.

10080. As to Magnetic action. I do not see how the helix moving (in falling or rising) amongst magnetic lines of equal force and parallel in direction, can have any magnelectric currents produced in it. To produce these currents, the moving wire must cross the lines of magnetic force, but from the constitution of the helix and the parallelism of its fall, for as much wire as crosses the lines of force in one direction there is an equal quantity crossing them in the other direction, and so no current can be produced. If a ring of metal be moved in any direction through or across lines of magnetic force of equal intensity, no current can be formed in it provided it be kept parallel to itself. If it revolve about a diameter, the case alters, but that is not the case of the helix.

10081. I do not see how an iron core can alter the effect, unless indeed it receed from or approach to some magnetic beam or column in the building, and then its own magnetism can be increased or diminished, and it will of course produce currents by induction at these times.

10082. If there should be any effect of Gravity convertible into Electricity (10018)—then we might perhaps find delicate means of shewing the gravitation of one body to another, and verify Cavendish's and Baily's conclusions, by comparing the action of the earth with the action of a heavy ball of lead. For the motion of the moving core (representing a gravitating body) in a horizontal instead of a vertical direction would entirely eliminate the action of the earth, and leave the bodies in the direction of the axis of motion to act on each other alone.

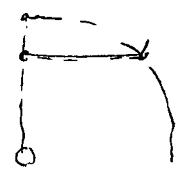
10083. Perhaps in that way a convenient and measurable test and indication of gravitating action of different bodies might be contrived and usefully applied.

10084. Then, under such suppositions, would the centers of gravity of two bodies, as two balls, when acted on by *the earth,* be also the centers when they were acting on each other i.e.

considering their size and shapes? It could not well be if the shapes were extended or irregular, i.e. the center of gravity of a globe or of a solid ring of equal weight would be the same for the earth's action, but would they be the same to a *near small* globe in every position of the ring?

10085. If Gravity has relation or connexion (as I suppose) with electric forces, and therefore with magnetism; then it is possible that the rotation of the earth on its axis, by its counteracting gravitation in the equatorial direction, may have *something* to do with terrestrial magnetism and the direction of its polarity. And if so, then the rotation of a globe of copper ought to develope some magnetic effect in the direction of the axis of rotation. It should be rotated on an axis parallel to the *dip,* or else inductive magnelectric currents would be produced. In any case, the effect expected can only be small, and therefore interfering sources of error must be carefully excluded, and the amount of action magnified as much as possible. Astatic needles with long intermediate fibres or rods and carefully sheltered, on fixed supports, would probably be required.

10086. But if gravitating force is convertible only whilst motion is being *acquired* or *lost,* then the mere rotation of a body and its tendency to expand at the equator should do *nothing* unless it really expands or moves. Unless indeed some other consideration is developed, like that of the moon in its orbit round the earth, where whilst moving from *a* to *b,* it may be considered as *falling* from *a* to *c.*



10087. From the experiments (10067, etc.), it would seem just possible that there is an effect and that a current of Electricity is induced round the line of gravitating force joining two bodies together, when one of the bodies moved thrgh. the active exertion of that force, and that the law of force may be as follows. If a man were falling with his face towards the earth, the electric current which would tend to form round him would be in the same direction as the hands of a watch which he should be looking at, or screw fashion. If he were to retreat from the earth, in the contrary direction. So that if he were to move face forward either to or from the earth along the line of gravity, his hands, moved as the hands of a watch or thus, would indicate the direction of the induced current.

So all was consistent with theory and the known laws, and the effects are good illustrations of the currents produced by cutting the magnetic curves of the earth.

10321. The machine being as just described, if it were gradually depressed to the horizontal position, the current evolved became less and less and at last diminished to nothing.

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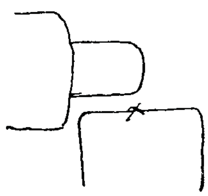
10321[1]*. Employed our large cylinder Electromagnet, urged by two pair of Grove's plates well charged, as a magnet. Had a long wire covered—reaching from my electrometer 33 feet off to the magnet and back again, in which any currents that might be induced were to be observed; the middle part of this wire or that at the magnet, which I will call *x*, was taken over the edge of a board about 3 inches wide and then by the sides of the board and so away to the Galvanometer; the board served as a handle or a foot and the part *x* could be placed and fixed in any position relative to the magnet.

10322. Another wire, arranged in a similar way on a board, was connected when necessary with a battery of five pair of Grove's plates so as to supply a wire carrying a current when needle[2]; the part corresponding to *x* I will call *y*. So *x* is the wire in which currents or effects were to be sought for and *y* is an electric current which, with the magnet, were to act on it.

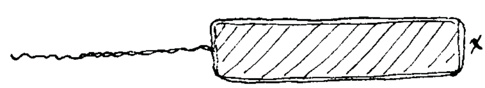


10323. Placed the wire *x* parallel to the magnet axis but by the side of the pole: then made the magnet active: that did not induce any current in *x*, for it and its prolongations are in a plane passing through the magnetic axis. Hence the opening magnetic curves would not cross the wire but form and move parallel to its plane, and this is a position of indifference. By raising or lowering *x*, it then cut the magnetic curves, and a weak current was induced. The effect was very small though visible—the magnetic pole is not strong enough. Considering that *x* is only 3 inches and that in the connecting wire and Galvanometer there are perhaps 600 or 700 feet of wire, more effect could not be expected.

10324. Then the current in *y* (10322) was approached to and

[1] 10321 is repeated in the MS. [2] ? needed.

* [10321]



removed from *x*: but there was no sensible effect either when the magnet was active or not. As there was no effect without the magnet, probably it could not be expected with it.

10325*. In place of wires *x* and *y*, used two flat copper wire helices containing each probably 32 feet of wire. Helix *x* was placed at the summit of the Magnetic pole, so that the magnetic axis was in its plane, and this being an indifferent position, the Galvanometer was not affected by making the magnet active or inactive. Helix *y* was then connected with the 5 pr. of plates (10322). This affected *x* by approximation and recession, as it ought to do, *but* when the magnet was made active there was *no difference*. No signs of a *permanent action* when the magnet was on.

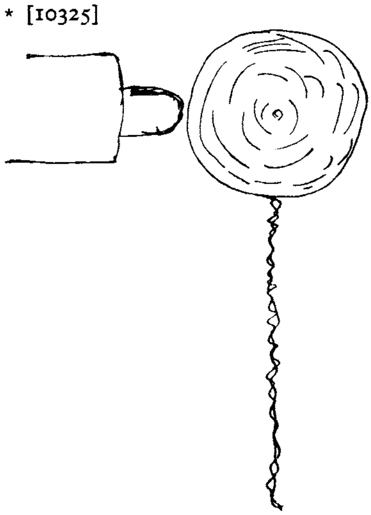
10326. Now put helix *x* with its plane tangential to the end of the pole and in contact with it—the pole and the helix were concentric. No permanent action. Then brought up helix *y* with its current. It acted on *x* exactly the same whether the magnet were active or not, and there was no permanent action. Nothing new.

10327. Dismissed helix *y*, and gave the whole power of the battery of five pair of plates to the Electromagnet. Retained helix *x* as just described, i.e. a tangential plane at the end of the magnetic pole. Then a piece of iron approached end on towards the magnetic pole, by concentrating the Magnetic curves, produced abundant electric current in *x*. But a good thick piece or bundle of bismuth bars did nothing of the kind. *No diamagnetic action.*

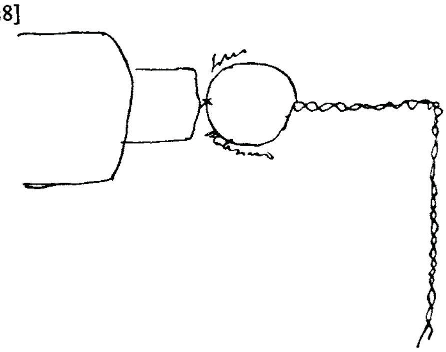
10328†. Made a thermo electric combination of Iron and platinum (8427[1]) and replaced the helix at *x* by it, so that the loop should be in an indifferent position (10323). Then put on and off the Magnetic power. There was no action at the Galvanometer either

[1] ? par. 8426.

* [10325]



† [10328]



at the moment of charge or permanently. This negative result agrees with the former old experiments.

10329. Have a good thermo electric battery of 40 alternations. Placed this so that one set of terminations were close to the Magnetic pole and the other 1½ inches away. *No action* either on making or breaking or continuing the battery contact with the magnet. Great care is required here to place the thermo electric battery and also its connexions in an indifferent position *first*.

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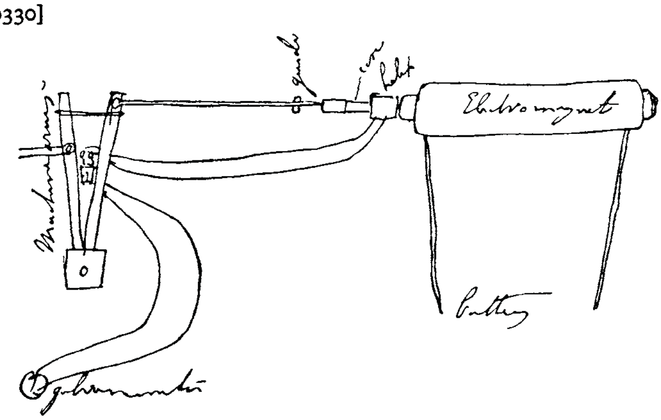
10330*. Have had the Machine arranged according to the principles indicated (10315, 6). A rod attached to the arm of the lever carries the bismuth or other core at its extremity—by the motion of the arm the core enters into and withdraws from the helix (10189), and the latter is close up to the end of the cylinder electro magnet (10321). The core in advancing passes almost into contact with the end of the electro-magnet and then withdraws from it, receeding about 2 inches. The magnet and the helix are, necessarily, fixtures in respect of each other; the core is the moving part. The magnet was urged by 3 pr. Grove's plates and was far superior in power to the two bar magnets, probably 10 or 15 fold as strong. The commutator divided the cycle as before described at (10318). The connexions were all good and a thermo current easily passed through them. The Galvanometer needle was displaced only 2° when the magnet was excited, being about 12 feet from it. For the purpose of avoiding the communication of tremor from the machine to the helix or magnet, the former was on the Lecture table in the lecture room and the two latter on a separate table—a guide which supported the rod was the only thing by which motion could be communicated to the helix table besides the floor.

* [10330]



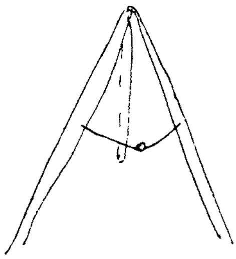
10331. *Bismuth core.* When the magnet was not excited, there was no effect produced at the Galvanometer by working the machine. The core probably performed 5 complete movements into and out of the helix per second, i.e. approached the magnet five times and was withdrawn five times. Again, if the magnet was excited and the machine *not* worked, there was no action at the galvanometer save the 2° of displacement (10330).

10332. When the magnet was excited and the machine worked, there was abundant action at the needle, but this was soon referred to causes of error: it was sometimes one way and sometimes the other—and it diminished very greatly when the wire guide was unclamped from the table, and from all the appearances I concluded that it was the effect of a tremor or other motion varying the position of the helix and magnet isochronously with the inversions of the commutator.

10333. When the bismuth core was away and the machine worked with the excited magnet, there was very little effect at the Galvanometer though the helix and magnet were in place, but the carrying rod did not then rub with the same force upon the guide (10330). When a spring of wood was made to press upon the rod, still there was no effect, but also there was not the same shake as when the heavy core was carried by the rod, for its stopping suddenly made a considerable tremor.

10334. Tied the copper core on cross ways to the end of the carrying wire and cap, so that there might be the same mass moved but yet the core be out of the helix, and *now* there was much deflexion at the Galvanometer. So believe that all the action was from Sources of error.

10335. Sometimes at the beginning of working, when all had been just and well arranged, there was no effect at the Galvanometer though the bismuth core passed to and fro in the helix under the influence of the magnet. *I think at these times there would have been signs if the bismuth had been able to induce a contrary current to iron.* In a few moments vibration of the whole table came on and then the abundant but irregular action due as I think to sources of error.



10336. Further precautions. Instead of letting the core rod rest on a guide on the table, I have erected a triangle on the floor over the table and by a fine copper wire made fast to two of the legs and passing once round and under the core, I have slung it so that it can move only parallel to itself and yet its weight is not on the table sustaining the helix and magnet. Have also fixed the axis screw of the machine. Have adjusted the commutator so as to change at the beginning and also at the end of one direct *to* or *fro* motion. The machine works very steadily now, and the connexions with the Galvanometer are perfect. The helix and Magnet are bound together and 2 pr. of plates used for the magnet.

10337. *Bismuth core* in its place. I think it does not touch the helix or its table any where. When the machine was worked without the magnet there was no effect at Galvr. When the Magnet was excited without working the machine, the needle was deflected as yesterday about 2°. Still, on using both Magnet and machine there were some irregularities of action. So strutted the magnet table better and now all looked well. When the machine worked, there was a little regular motion of the needle, slow and steady, looking like real bismuth action. The end of the Galvanometer needle went West when the outer terminations were connected according to the standard condition (   ) at the Galvanometer.

10338. A piece of iron wire of equal length with the bismuth core, being attached to it so as to act as an iron core, did do so powerfully, and the needle end went Eastwards. *This is therefore the standard of direction for Magnetic action* with these connexions.

10339. *Bismuth core again,* iron being away. The action now irregular and that two and three times in succession, needle end going sometimes west and sometimes east with same galvanometer contacts. This must be due to some irregular motion at the helix. Then again a very regular case came on of needle end to the *West* (10397, 416).

10340. *Copper core in place.* Needle end went well regularly and strongly to the West. Again, with a slower motion, same effect. This happened a third and a fourth time. Is this a peculiar effect of copper or is it some error (10382)?

10341. *Gutta Percha core.* Nothing. No direct effect and no error. So the shake of the machine with a light core in produces no error. So that error in any case cannot well be through motion communicated through the floor.

10342. *Sulphur in glass tube.* The least degree of effect. The needle end to the west as with copper.

10343. Suspect still a knock or a slight rub of the heavy cores against the sides of the helix; must have the copper and bismuth made smaller and quite clear of the helix, for Monday.

10344. *Foolscap paper core*—no effect.

10345. *Platina wire core*—Do.

10346. Copper core again—well fitted and centralized, gave same effect as before.

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10347. Have adjusted the commutator—and made machine more steady and connexions are good with Galvanometer. Helix is steady to the magnet and the cores of copper and bismuth have been turned smaller so as to leave no fear of their striking or rubbing inside of the helix as they pass in and out.

10348. *Copper core* in and well centered. When the machine was worked, the near needle end went at once and well to the *West.* This happened again and again. I could feel no vibration or knock on the helix by the finger and I find that is a very sensible test of rubbing or a blow there. Even when motion of the machine was slow, the needle end went West with the standard condition of contact at the Galvanometer. The fact very clear—is it due to iron or what? (10371, 10382).

10349. *Iron.* Placed a wire of iron alongside and with the copper core and again worked the machine. The needle end went *Eastward,* very powerfully. This therefore is the action and effect of *iron.* I believe the other is the true action of copper, and as it is contrary to the iron action, it cannot be due to iron or ordinary magnetic impurity in the copper. Must either upset or establish this point and find its relation to supposed polarity of bismuth in Weber's experiments.

10350. *Bismuth* core—attached and centered unexceptionably, but there is no effect at the Galvanometer. This happened again and again. So the copper effect is not the bismuth effect and probably not a diamagnetic effect; but what is it? (10339, 10397).

10351. Is not the effect due to currents induced in the approaching and receeding metal, which would be of course concentric with the prolonged axis of the Magnet (the electro cylinder urged by 2 pr. of Grove's plates (10330)), and is not the reason of the activity of Copper and the inactivity of bismuth to be found in the difference of conducting power, just as that produces differences in revulsive effects (10408), and are not the revulsive effects and these due to one and the same mode of action and so the revulsion phenomena proved and confirmed?

10352. Iron manifestly acts by its magnetic condition—being a bad conductor it would allow of a very feeble development only of these currents—but that would be almost infinitely overpowered by its results of true or especial magnetic force. But then, if bismuth were rendered polar reversely to iron, it ought in conjunction with these induced currents to shew some effect surely; and that it does not seems to indicate that it is not reversely polar to iron when near a magnet or in the magnetic field.

10353. In the non production of these phenomena by badly conducting metal we see how *time* comes in as an element related to and connected with conducting power. For the currents could probably be induced in bismuth if time were allowed or could be allowed in proportion to its bad conduction power.

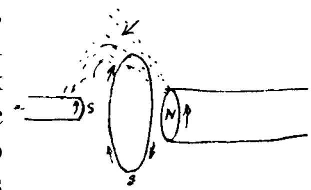
10354. Bismuth is crystallized and Magnecrystallic, but the core is a mass of crystals not symmetrically arranged but in all directions. It would be a great point to ascertain the effect of a core of bismuth built up of crystals, all the axes of which should be parallel to the axis of the core.

10355. If my view right, then a core of copper *wire* parallel to the axis should give nothing or next to nothing—a core of copper discs should give the effect—and a core consisting of a solid copper wire helix should give the effect without the external helix, i.e. by itself. Also such a core should give no effect in the helix if the ends not connected and should give the effect if the ends are connected.

10356. So also (perhaps?) if tin and lead cores, being solid, do not give the effect, cores built up of disc[s] of tin and lead ought to do it, just as they give powerfully the revulsion effect—which by such means can be made sensible when it cannot be found in massive pieces.

10357. So the copper in these experiments puts on the diamagnetic character of polarity apparently instead of the bismuth, but it is not in reality a polarity, or at least not a polarity antithetical to that of iron or magnetic bodies, and though it simulates the action of a magnet (in the contrary direction), it is by another mode of action. For as the copper cylindrical core *approaches* towards the end of the magnetic iron core of the Electromagnet, the electric currents induced in it will be the reverse of those which correspd. to the typical current in the magnet as in the figure*; whereas as the copper core receeds from the pole, they will be in the *same* direction relative to the joint axis of the two cylinders, the core and the magnet. With Iron, the currents which act are always in the same direction, namely as those in the magnet—but they strength[en] on approach and weaken on recession. With a diamagnetic body truly polar antithetically as Iron is, they would be always the contrary to those in the magnet—also strengthening on approach and weakening on recession. With the copper there are both currents, so that it is as the supposed polarity of bismuth as it approaches and as the polarity of iron as it receeds.

10358. Now whether a piece of iron, as an iron wire core *a*, when approached towards the N pole of a magnet, be supposed to act directly by its own inducing force (as an S pole) on the helix spirals *s*, or whether it be conceived of as causing the magnetic lines of force emanating from N to draw in or collapse and so to travel across the spiral, the induced current will in both cases be in the same direction and as represented in the figure. If on the other hand *a* were a core of bismuth that could assume reverse polarity and so have an N pole raised up opposite the N Electro magnetic pole, still that pole, acting either directly on the spiral in its motion or acting by diverging, opening out and diluting the lines of force from N passing across the magnetic field, would in either or both cases produce the same effect, i.e. a current in the contrary direction to that figured above. But a core of copper cannot be conceived of as acting in such a way, for though as it approaches the currents set up in it are contrary to those in the



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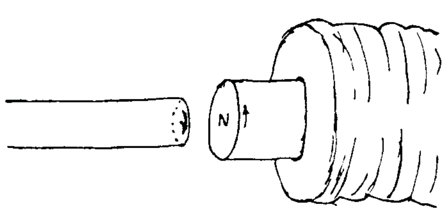
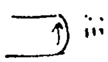


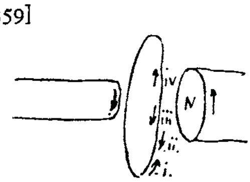
magnet, and therefore the same as those supposed in the bismuth, *they do not continue;* and the moment the advance ceases they cease too, and the copper is neither as iron or bismuth therefore; the magnetic curves in the magnetic field are not either converged or diverged as they would be with iron or bismuth core, but exactly (as is supposed) as they were before the copper was there or advanced. On the recession of the copper the reverse happens to exactly the same amount.

10359*. So the advance and recession of the iron first concentrates and then diverges the lines of force as respects the helix, which gives rise to currents in two directions, and these by the commutator are gathered up into one consistent current. The advance and recession of the bismuth should also diverge and then concentrate the lines of force and so give two currents in contrary direction to the former, which being gathered up by the commutator, should make a current the reverse of the former. What should the copper do? If it act by the currents set up in it, then as it approaches the magnetic pole, the currents formed in it should be of the contrary direction to the magnet currents and should induce a contrary current to themselves in the helix, i: when it ceases to move, these currents in the copper will cease and their cessation will induce a current in their own direction in the helix, ii: as the copper receeds from the magnet, a new current will be set up in it, parallel to and in the same direction as that in the magnet, and this will induce in the helix wire a current, iii, in the contrary direction to itself: and when it stops again, the current in it will fall and cease and in falling will induce a current, iv, in its own direction in the helix wire. Thus there will be four currents induced in the helix wire in the cycle of motion ↑ ↓ ↓ ↑, but the second and third of these will be in the same direction and coalesce into one, and the fourth and first will also be in the same direction and therefore coalesce into another; and these two in contrary direction will be gathered up by the commutator and converted into one regular current.



10360. Now the commutator is so arranged that the galvanometer connexions are inverted just before the motion *to* or *from* ceases (10437, 10463). Hence as the copper approaches, the current in the helix is carried off: just before the copper stops,

* [10359]



the commutator changes and the current due to stopping, ii, is then gathered up; then the copper is withdrawn and produces current iii in direction as ii, and the commutator still takes it up; but just as that current is about to cease and before the new one produced by the stopping of the copper, iv, exist[s], the commutator changes, and then gathering it up, joins it on to the one produced by the approximation of the copper in the next cycle of motion (10410).

10361. So the results are the *same* as if the copper were a magnet antithetical in its character to one of iron; but it is very manifest that it is not such a magnet, but acts on different principles. It can probably only act thus when a mass: a *mass made polar* for the time *in contrary directions,* whereas a magnet or a diamagnet should consist of particles or molecules made polar for the time and always in the same direction.

10362. No doubt a larger law of action would bring both or all three cases under one expression, but still that would not as yet shew that bismuth is a diamagnet—even though it might produce the phenomena of copper. It has as a diamagnet to produce the *antiphenomena* of iron.

10363. I must look at Weber's results to see how they build in with these considerations and what the results are.

10364. I made several experiments now with weak magnetic bodies, looking at the results with a very different mind to what I have had before.

10365. i. *Saturated solution of Sulphate of Iron (proto)* in a thin glass tube. Well arranged, but it produced no sensible action at the Galvanometer. The motion of the machine was not the quickest but pretty good.

10366. i. *Green bottle Glass*—a thick dark green tube—indifferent in action—if any result, it was in the least possible degree as Iron (10400).

10367. i. *Crystals of Proto Sul. Iron* in the tube. A doubtful trace of action—if any, is as iron but the action is very small or none (10401, 10435).

10368. i. *Iron filings* in the tube. Exceedingly powerful, as iron.

10369. i. *Iron scale oxide.* Do.

14-2

10370. i. *Red oxide. Colcothar.* Very feeble indeed—the least possible trace of action—as iron.

10371. i. *Copper core* again (10348). As before—contrary to iron and strongly; the needle end could be sent 30° West, whereas the solutions or crystals of iron and the Red oxide hardly sensible.

10372. Hence, either this machine gives a bad method of shewing magnetic polarity as compared to the mere act of attraction of a magnetic needle, or else *time is required.* Must use the crystals of sul. iron—slowly—to see what time does.

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10373. Whatever the final relation (10362), there is this difference probably between the inductive action in copper and magnetic including diamagnetic action (if it be as Weber thinks); that section across the core, as into discs, does not retard but may even help (as in revulsive phenomena) this action, whereas it interferes with magnetic action because of its polarity seriously. Also, that division parallel to the axis of the core, as when it consists of small wires, will interfere much with the first but not with the latter. This may depend in part upon the point that what is Massive in the one case is molecular in the other, namely the currents real or imaginary, and this may be connected with the point that one is polar and keeps its state, whilst the other is not, after the first moment, but falls to 0°.

10374. Is Magnetic action *across space,* through air, water, a vacuum, etc., but *between contiguous particles* in iron, nickel, etc.? And if so, can a given space, as that occupied by a piece of prot-oxide of iron, be traversed by the power partly in one way and partly in the other? And if so, what would be the condition of iron space as the iron is heated in the Mag. field from cold to white heat?

10375. What a strange thing an electric current in an electrolyte would then be and indeed is—acting across *space* laterally but from particle to particle only axially.

10376. Consider the motion of a magnetic pole and wire carrying an electric current round each other. The motion of the pole round the wire is a motion of the pole *along* lines of magnetic force. Its lines of magnetic force *cross* the wire lines of force.

10377. The lines of force of M. poles are flexible and change in their direction as another pole is brought near, so that there are obstacles in the way of working with two magnetic poles. But the lines of force in a wire carrying a current are probably pretty permanent, depending on the current. So consider state of and work with two currents in wires.

10378. Ought not two wires or currents at right angles tend to move in the direction of *their length?* Or if one fixed, ought not the other to move? If so, ought not such wires, one having a current and the other not, affect each other? And may not this be the relation which I have tried so long to discover (   ) in two such wires?

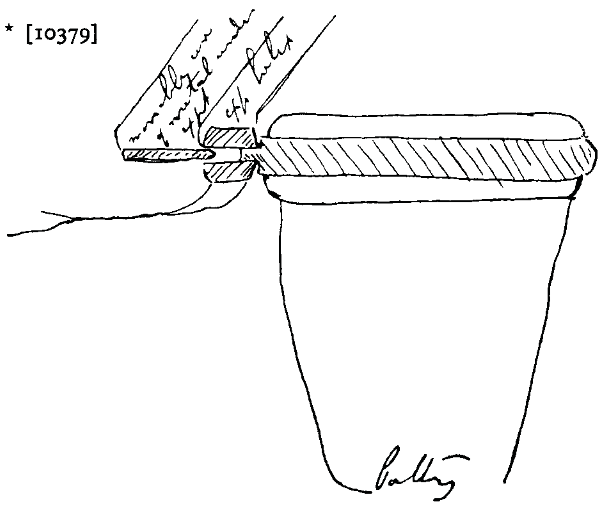
Consider and work out all this.

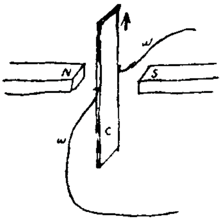
6 NOVR. 1849.

10379*. I have had one end of the iron core of the large cylinder Electro magnet (10330) turned smaller for about 1 inch in length, so as to fit the interior of the experimental helix (10189); the latter therefore is now fixed on to the former and tied to it, so as to make the two fixed in relation to each other. The hollow part of the experimental helix is of course the space through which the experimental core of bismuth or other metal is to travel. Now the Electro magnet and exp. helix make one thing and are supported on a steady stool, quite independant of the table on which the moving machine stands—or the exp. core which the machine carries. So that any communication of motion which may disturb the relative position of the Electro magnet and the Exp. helix is cut off. The experimental core is slung as before (10336) by the triangle and copper wire.

10380. The machine axes have been cleaned and oiled. The Electro magnet is now excited by 5 pr. of Grove plates well

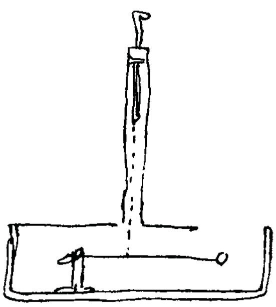
* [10379]





11433. Experimented with the endeavour to develope *static electrical phenomena* from magnetic forces—reasoning upon the general nature of the lines of magnetic force. Let N, S be the two poles of two magnets or of a horseshoe magnet; lines of force will exist between them in a horizontal direction. Let *c* be a plate of copper placed equatorially or across these lines and able to move up or down—and let *w, w* be two stationary wires touching the edges of the moving copper and leading away to a galvanometer. As the copper plate is moved *up,* a current will go across it in *one* direction through the conducting wires away to the galvanometer, and as it is moved *down,* a similar current will go in the other direction.

11434. The point is, suppose *c* a non conductor or *w, w* non conductors: would such motion, though it could not cause a current, tend to excite Pos. and Neg. Electricity correspondant to a current, and would the edges of *c* and the touching points of *w* leave each other in different electrical states through the tendency of the lines of force to produce a current? For though the current produced in conductors is of very feeble intensity, still it is by comparison very abundant in quantity, affecting the Galvanometer as a machine could not do unless very powerful, and if this current were stopped, the intensity might rise. Experiment only could determine this.



11435. *Electrometer:* a small disc of silvered paper was attached to one end of a fine well drawn thin stem of shell lac, and then this stem suspended horizontally by a single fibre of cocoon silk from a shifting rod inserted in a vertical tube. The tube was fixed on a box which enclosed the horizontal stem and preserved it considerably from air motions. Two stops were adjusted at the hinder part of the stem to limit the oscillations of the needle part. So when the silvered paper was charged, it was well insulated by the shell lac and kept its charge well; and it also was very free to move by attraction and repulsion. It was charged by exciting a piece of shell lac, then charging a small carrier by the lac and finally touching the suspended silvered paper disc with the carrier.

It was a good electrometer. The disc of silvered paper was about 1/3 of an inch in diameter and the shell lac arm supporting and insulating it about 3 inches long from the silk suspender.

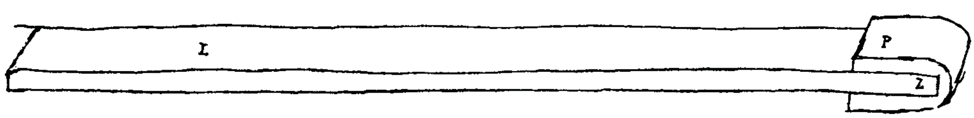
11436. The rubbers which were to be stationary in the magnetic field may be called S, S (Stationary); they were of various substances and thus constructed*. LL is a piece of wooden lath nearly the full size[1] and P is a piece of Gutta percha, which being made warm, has been applied to the end so as to present a regular rounded termination fit for a rubber. Other similar pieces of lath were finished of[f] at the rubbing end with shell lac, sulphur, etc. Others with flannel, white silk, tin foil, which was attached on to terminations previously made of Gutta percha. One was covered first with four thicknesses of flannel and then thin copper foil, drawn tight over that and nailed down.

11437. The part to move may be called R (running), and was a strip of flannel or a piece of white silk—or tin foil—or even a flat bar of shell lac or Gutta percha. It was to represent the moving metal plate (11433).

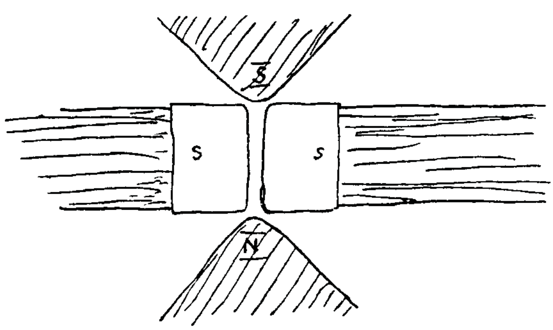
11438†. The following was the plan of arrangement at the poles of the great magnet, excited, when needful, by 20 pr. Grove's plates. S and N are the poles of the magnet and the initials correspond to the S. and N. ends of a magnetic needle. S, S are two rubbers fixed in their place by blocks and weights placed upon their further extremities to keep them steady. So the lines of force are powerful in the space between them. When experimenting, the moving body or R is to be placed between these ends, which then are to be held so as to press it—and when the magnetic power is on, the R is to be drawn up, so as to run between and rub against S, S. This may be repeated 5 or 6 times, always taking care that the motion of R is in the same direction when rubbing.

[1] Reduced to 3/4 scale.

* [11436]



† [11438]



11439. S, S, flannel faces on Gutta Percha ends—R a slip of flannel. So all the rubbing surfaces flannel in its ordinary cold state and no magnetism on—there was no excitement of Electricity. The flannel was too damp. When warmed and dried, then it became excited irregularly; but when excited, preserved its charge well enough to be thoroughly examined.

11440. In these experiments, such bodies as shell lac and sulphur become charged and keep their charge tenaciously, but they may be discharged (   ) by breathing well on them and then wiping the surface with the same part (over a finger) of a silk handkerchief or a linen cloth in its ordinary state. S, S as well as R always has to be examined before an experiment, that they may be known to be not excited. The ends of S, S, when unexcited, will attract the charged disc of the electroscope, but that is because of the conducting wood within the shell lac or other body. The shell lac or sulphur only makes the action of the wood more manifest, because of their high specific inductive capacity.

11441. The flannel strip R may be discharged by laying it between two tin foils and pressing it for a time.

11442. Now the rubbing surfaces S, S, were made *shell lac* (11436) and the runner between, R. Whilst the flannel was unaired, it preserved no charge, conducting too well. But when the flannel was well warmed and aired, though it could be discharged and not excited by drawing it through *linen,* still when drawn through the *shell lac S, S,* it was well excited, becoming positive. Hence it insulates sufficiently to acquire a charged state.

11443. Then the friction was repeated, the Magnet power being on, and again the flannel R became altogether positive and the two shell lacs both Negative without respect to the direction of the magnetic lines of force. In repeated experiments with S, S Shell lac and the runner R flannel, no effects were obtained which shewed any relation to the direction of the Magnetic force. It seemed to be indifferent to these excitations.

11444. The rubbers S, S, being *shell lac* (11436, 11442), the runner R was also a flat plate of shell lac about 0·75 of an inch wide and 0·1 of an inch thick. Being at first discharged, the friction made R Negative and both S, S Positive. This effect was often obtained, but with some variations. Then, employing the magnetic power also, still the same or similar effects came out—there was no appearance of any action exerted by the magnetic force—all being *Shell lac*.

11445. The rubbers S, S being *shell lac* (11436), the runner R was made tin foil, being a band of that substance of four thicknesses. The rubber could not here be found charged, being uninsulated, but the two rubber surface[s] might have given different states. But whether without or with the magnet, the shell lacs S, S, were both excited always and allways *both* Positive. So there is no sign of any magnetic effect of excitation when one body is shell lac and the other a conductor as tin foil (11451).

11446. *Sulphur* was now the rubbers S, S (11436), and flannel the runner R. Before it was air[ed] it did not itself appear to retain any charge—but the Sulphurs became both negative. When it was aired and dried, then it became positive, making the sulphur negative. The magnetic force made no difference.

11447. S, S being sulphur, the runner R was made tin foil (11445). But whether magnet or no magnet, both the sulphurs were left Negative. So here no peculiar signs.

11448. S, S being sulphur, the runner R was made shell lac (11444). *Both* the sulphurs were rendered negative and the shell lac positive, whether with or without the magnet.

11449. *Flannel* on Gutta Percha was S, S (11436) and the runner R also flannel. There was the same uncertain result, whether in or out of the magnetic field.

11450. *White silk* on Gutta Percha was S, S (11436) and the runner R also the same white silk. The runner R became Negative, whether with or without the Magnet. Then made the Runner R two silks (in thickness), but whether with or without the magnet, both these halves of the R became alike negative. They did not acquire different states.

11451. *Tin foil* on Gutta percha for S, S (11436) and *shell lac* for the runner R. The latter or shell lac became positive on both sides, and that whether with or without the magnetic action. The latter appeared to make no difference. So the result the same here as when Shell lac was rubbers and tin foil runners (11445).

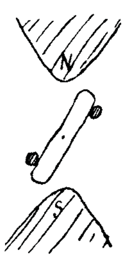
11452. *Copper* on flannel for S, S (11436) and two white silks for runner R (   ). Both the silks of the gunner became positive, and that whether magnetic power was there or not.

11453. With *Copper* for S, S, made the runner R shell lac (11444). Whether with or without the magnet—the shell lac was well positive, on both sides.

11454. With copper for S, S and the two white silk for R (11450), placed the rubbers with the line joining them equatorial, as all along (11438), and then turned the system 90° in a horizontal plane, so that the line joining them should be parallel to the magnetic lines of force and the plane of the rubber axial. There appeared at first to be a difference for the two position[s], the silk being frequently positive for the one and negative for the other, but on numerous repetition[s] and also with a single silk R, I could not find that there was any difference dependant on the magnetic state.

11455. The silk came out sometimes positive and sometimes negative, but I found that just after warming the silk, it came out of friction with copper rubbers Negative for both positions, and that if I then allowed it to cool awhile or wiped it with a slightly damp linen cloth, it was positive when rubbed against copper in either position—or with magnet or no magnet.

11456. *Tin foil* for S, S, and silk, white, with either axial or equatorial positions as above (11454); the silk always positive whether magnet or no magnet.



11457. Plücker says that if a piece of bismuth be placed between the magnetic poles, but so blocked that it cannot go far in the equatorial direction, then that when the current is taken off from the magnet, there is no particular effect produced; but that if the magnet be quickly reversed, there is an effect, the bismuth tending at the moment to be drawn axially, as if it retained opposite polarity for a short moment by its so called coercibility or incoercibility, i.e. its power of retaining a polar state for an instant of time. I tried the effect but could not perceive it. There is certainly a shake at the first reversion, but I think it results from the difficulty of blocking both arms exactly alike, and then the center of Gravity is not truly below the point of suspension, and so motion is produced at the taking of the repelling power. That is done far more quickly by reversing the magnet than by merely taking off the battery (when the magnet falls slowly), and so I think this shake is produced. Perhaps induced currents may have a little to do with it.

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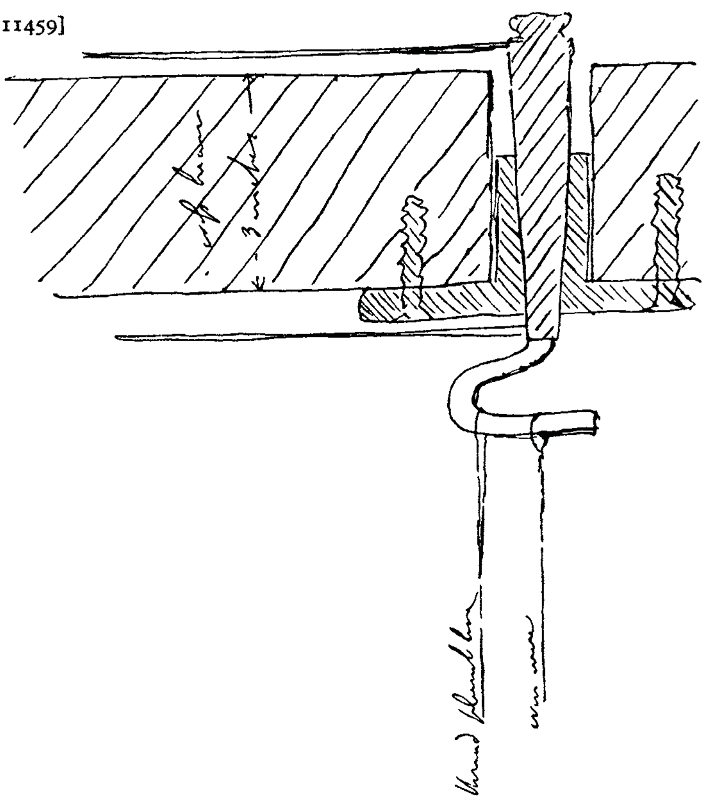
11458. Experimented to ascertain whether a long soft iron wire hung perpendicularly tended to incline towards the direction of the dip. Worked in our lecture room, suspending the wire from a cross bar at the level of the lanthorn light—its lower end being within 8 inches of the lecture table. The wire was freshly, well annealed iron, 27 feet long and 1/66 of an inch thick; the 27 feet weighed 103 grains. A loop was made on the upper end—the whole length suspended vertically from a smooth round pin fixed above—a weight attached to the lower end and then the wire stretched by a direct pull downwards until it was elongated a couple of feet or more. Whilst the weight continued on, it appeared perfectly straight, but on cutting off the wire 8 or 9 inches from the table below, then it shewed contortions and flexures—little above, for there the weight of the wire pulled them out, but more below, the extreme deflection being perhaps half an inch from the true line. Further pulling, though it lessened the flexures, did not quite remove them so as to give a straight line.

11459*. Above was a plate fixed having a vertical socket and

* [11459]



conical pin moving freely and steadily on a vertical axis; below, the pin terminated in a horizontal support of round wire as in the figure, which could be placed in any direction by means of either of the pins serving as indexes attached above and below to the axis; and by an adjustible plate on the upper surface of the wooden beam, the Magnetic meridian could be marked and also the line perpendicular to it. A thread with a small weight at the bottom was first placed by a loop on the support near up to the bend or concavity, and then the stretched wire was carefully transferred from the stretching pin also on to the support, and as near as might be under the center of motion. This being done, the next object was to place the support truly magnetic east and west, and observe the lower ends of the thread plumb line and of the wire by a telescope looking east and west on a scale behind the projection of the wire and thread; and afterwards to turn the pin round 180° so as to place the points of support again east and west and observe again. The thread bob ought of course in both places to give the same position on the scale. The iron wire because of its contortions ought to give two positions, but the intermediate point would correspond to that of a perfectly straight wire—and would correspond with the thread if the wire hung perpendicularly—or would appear to Magnetic north of the thread if the wire were sensibly deflected towards the dip. The thread bob was easily held on one side (by the end being in a glass) when the wire was wanted alone, and having a little water in the glass when it was brought up into its place, the motions of it could easily be diminished and stopped.

11460. Two chief precautions amongst many others were required: one, that the wire should be so hung on its support that no rotatory motion should be allowed on the support above, but the body of the wire turn round through as many degrees as the axis above (or else because of the flexures great errors appeared); and also that the current of air in the room should cease, and time be allowed for motion and temperature and all other things to equalize and give a steady result.

11461. All proper precautions being taken, there were at first signs of some result, but upon repetition of the observations many times and in two different parts of the room, the conclusion was that no sensible deflection of the wire (bearing only its own weight) could be observed. Still, the weight of that wire was considerable (103 grains), and the effect would have been very marked if it had appeared.

11462. I examined this thin long wire by a magnetic needle about 4 inches long to see how far it acted by induction, as a poker. It did *not* present distinct traces of a polar condition. Its lower end attracted either end of the magnetic needle and apparently with equal force. Again, a piece of the same wire about 2 feet long was not distinctly magnetic by position in the dip, but when folded up into half and a quarter that length, then it began I think to act. So also a short piece of three inches in length acted.

11463. It was as if the extreme length compared to the thickness was against a general induction of the mass, but must try this again more carefully (   ). It may be that such an effect may be important in relation to the case of atmospheric magnetism, i.e. that lateral extension is needed in such cases as oxygen, etc. etc.

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11464. It is astonishing to observe how difficult it is to obtain a length of this wire entirely free from magnetism for experiments on the induction of the earth. If a piece, being straightened, is cut with scizzors or tools themselves magnetic, it becomes magnetic. If, being in a vertical position, it be cut with tools not magnetic, it becomes magnetic. If a piece be made red hot in a spirit lamp, cooling as it goes out of the flame, it becomes magnetic if the heated end is at all out of the horizontal line. If the wire, being held horizontal, bends during the heating, it comes out certainly magnetic. If the end heated be *retained* horizontal, or rather in a plane perpendicular to the dip during the heating, still if the other end or parts of the wire be raised or lowered above that plane, the heated end becomes magnetic. Almost any heating and cooling, or cutting or jarring action makes the wire magnetic if the *whole* be not in a plane perpendicular to the dip. Hence it requires great care to ensure that any piece intended for experiments of induction by position should be free from magnetism—and whilst in the dip, almost any action, a knock or blow or rub, will make it magnetic. The iron was very soft—even after ignition and sudden quenching in cold water.

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11465. Made a few more experiments upon friction in the Magnetic field, but with the R or running body (11437) at least 1/2 an inch thick, to allow of any effect that might be due to the length of the part moving *across* the lines of force and therefore between the fixed S, S.

11466. The *copper* on flannel for S, S (11436): the R was a cylinder of shell lac 0·7 of an inch in diameter. The magnet was the same as before—being the great one urged by 20 pr. Grove's plates. The running shell lac was *Negative* and on both sides—so no effect of the magnetic force sensible here.

11467. *Shell lac* on wood was now made S, S (11436)—and R was a copper cylinder 0·6 of an inch in diameter. The shell lac was now *Positive* and both of them were so—very clearly and strongly—and so the reverse of what they were when shell lac was the runner (11466); but in this case the friction was not smooth but with a sound or rattle or chirp, being an *intermitting friction*—and this was probably the cause of the reversion. The copper cylinder had been turned in a lathe—and when cleaned by the sand paper shewed that it was covered with ring irregularities produced by the tool in the lathe. It had not been finished off by a flat edged tool. These irregularities would tend greatly to produce the chirp friction or intermitting friction.

11468. *Shell lac* still S, S, and R a cylinder of tin 0·6 in diameter; it had been turned in the lathe as the copper and shewed the same marks on cleaning with sand paper. It produced chatter friction and left both the shell lacs *positive*.

11469. *Shell lac* still S, S, and R a cylinder of bismuth—the friction was without chatter and both the shell lacs were *Negative*.

11470. Shell lac S, S. R made tin again—friction with chatter and both shell lacs *positive*.

11471. Now rubbed these different bodies together, being the *same* articles but out of the Magnetic field.

Shell lac and	Tin—	no chatter—Lac left Positive.		
„ „	copper	Do. . .	Do.	Do.

Shell lac and bismuth	Do. . .	Do.	Negative strongly
„ „ Antimony	Do. . .	„	Positive
„ „ Fusible metal	Do. . .	„	Negative.

11472. Thus the Magnetic force adds nothing (11475).

11473. But a continuous friction and an intermitting friction produce very different results, at least with *Copper and Shell lac,* for when it is continuous the *shell lac is Negative,* and when it is intermitting the *shell lac is Positive.*

11474. Again, there is an unexpected difference between bismuth and the other metals, for when the friction is smooth, Tin, copper and Antimony left *lac positive*—but bismuth and fusible metal left it *negative.*

11475. Must ascertain however what is meant by the fact that, in the magnetic field, the copper made the running shell lac Negative (11466), though the friction was quiet or continuous—but that out of the Magnetic field it made it Positive (11471). I think the rubbing bodies were the same in both cases (11479).

11476. *Gutta Percha* rubbed against *Bismuth, copper, Tin, Antimony* and *Fusible metal* became Negative in all cases—being at the times of the experiments out of the magnetic field.

11477. *Gutta Percha* and shell lac rubbed together, the Gutta Percha was Negative. So that bismuth would seem to come between Gutta Percha and shell lac.

11478. *Copper* on flannel was made S, S (11436) and a plate of *mica* was made runner between, in the magnetic field. The mica did not become sensibly electrified. Perhaps being cold it did not insulate well enough to retain a charge—but there was no effect to encourage a notion that crystalline structure might aid in developing any effect.

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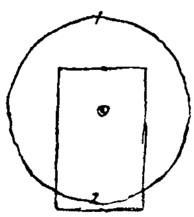
11479. Made further experiments—found at first that shell lac against copper was always left Positive—but then, by varying the rubbing, it was sometimes Pos. and sometimes Negative—though the friction was smooth—and that Magnetism had nothing to do with it was very evident. Careful investigation would no doubt make out the cause of the difference (11475). Abrasion of the shell lac surface has probably an effect.

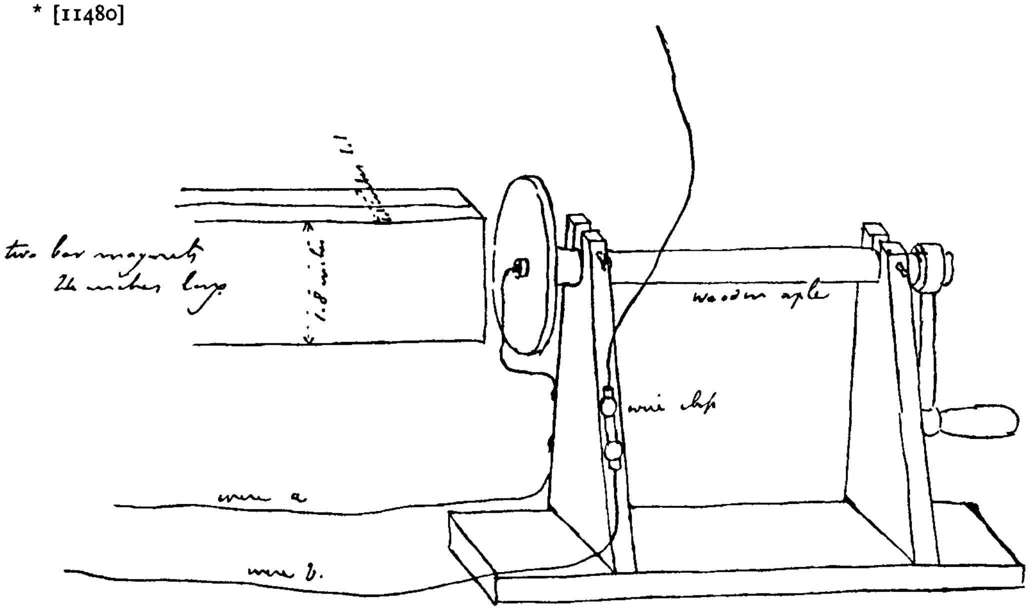
11480*. An apparatus was constructed consisting of a wooden stand and supports carrying a wooden axle which could be turned by a winch. On the end of the axle was fixed a copper termination ending in a male screw, on which different disc[s] of metal could be screwed, so as to be fixed against the copper shoulder and revolve with the axle. The end of the copper screw had a conical aperture turned in it to receive the end of a copper wire *a,* which by its spring and elasticity pressed into it and so made contact there. Then another wire *b* was employed to make contact at the edge of the disc, or any where else upon its cleaned surface where it might be desirable. The wires were held in wire clips fixed to the edges of the support and went away to the Galvanometer at a distance. This disc was intended for revolution within the magnetic field of any magnetic system, and the currents induced in it were to be rendered evident and measured at the Galvanometer. Its diameter was 2½ inches and its thickness 0·1 of an inch.



11481. Employed at first our two large bar magnets with like poles together, so as to make one magnet 24 inches long, 1·8 inches deep and 1·1 inches broad. It was placed end on before the revolving disc at 0·5 of inch distance from it, but was not concentric with it but thus. The one wire, *a,* was always applied at the center of the revolving disc and the other, *b,* at different places on the edge, care being taken by cleaning, pressure, etc. that contact was continuous.

11482. When the wire *b* was applied at 1, and the disc revolved, there was an effect at the galvanometer, but small, only 3° or 4° to the right or left for 20 revolutions, according as the motion was one way or the other. When the wire *b* was applied at 2,

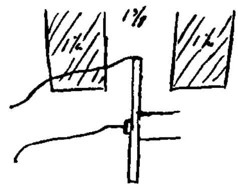
* [11480]



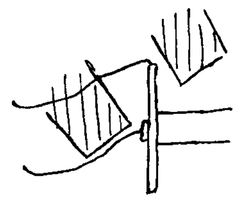
then the effect was greater, being 4° or 5° for the same number of revolutions.

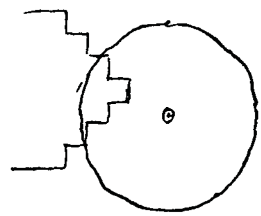
11483. Thus the principle of action was shewn in different points, but the power is not enough to give good and comparative results. The limit is soon obtained and in fact 40 turns here do no more than 20 (11354). So it will be better to use the concentrated field of a horseshoe magnet.

11484*. Employed our larger permanent horse shoe magnet, which has the dimensions marked at the poles and can sustain well 40 lbs. by the keeper and weighs 16 lbs. nearly. When it was arranged as thus in plan, and the wires applied as shewn, ten revolutions of the disc gave a deflection of 6° or more at the galvanometer, and a continuous revolution could sustain a deflection of 18° or 20°.



11485. The frame of the stand prevented me from approaching the magnet nearer if at right angles to the axis of rotation. I therefore turned it round thus, but did not obtain a better deflection; and considering that it is the outer end of the moving radius that produces the chief action, a better was not to be expected. So returned to the former position (11484) in which the disc was midway between the two poles, at right angles to the magnetic axis, and the moving radius about half within the face of the poles and half without. The axis of the disc was however below the middle of the magnet (because of the intervention of the frame) about 3/8 of an inch.

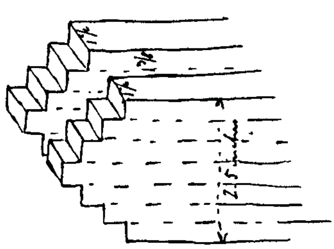




11486. Now the galvanometer wires being applied at the center and at 1, and ten revolutions made by a quick steady motion of the hand, the deviation was 6° nearly in either direction, according to the course of the revolution, and the swing of the needle continued longer than the time of the ten revolutions (indeed nearly half as long again), shewing that the whole effect was included (11354).

11487. Experimented—and wished first to see if a second set of wires from the Galvanometer could carry more power to it from other radii of the disc than that included in the first wire (11404, 5). So had two wires from each of the galvanometer cups or screws; these may be called *a*1 and *a*2 for one cup and *b*1 and *b*2 for the other cup. They were about ten feet long each and from 1/30 to 1/25

* [11484]



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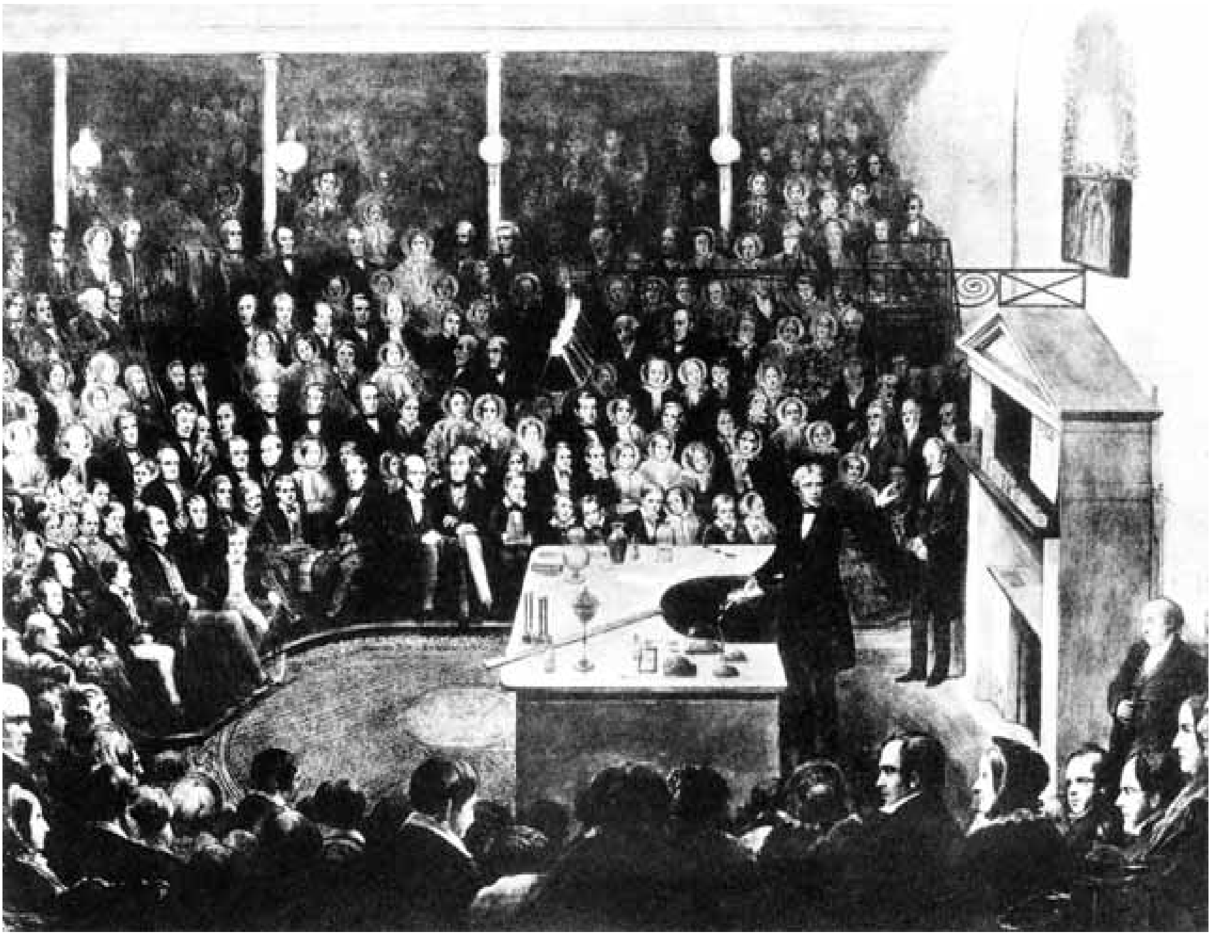
FARADAY'S DIARY

VOL. VI



FARADAY LECTURING AT THE ROYAL INSTITUTION, H.R.H. THE PRINCE CONSORT IN THE CHAIR. CHRISTMAS JUVENILE LECTURES, 1855-6

From a coloured lithograph (made by the artist) after the oil painting by Alexander Blaikley, now in the Hunterian Museum of the University of Glasgow

FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

made by

MICHAEL FARADAY
D.C.L., F.R.S.

during the years 1820 – 1862
and bequeathed by him to the
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THOMAS MARTIN, M.Sc.

with a Foreword by
SIR WILLIAM H. BRAGG, O.M., K.B.E., F.R.S.
Director of the Laboratory of the
Royal Institution

VOL. VI

SECOND EDITION

NOV. 11, 1851 – NOV. 5, 1855

HR DIRECT

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PREFACE TO VOLUME VI

FOLIO Vol. VI of the manuscript contains a considerable number of the actual specimens prepared by Faraday in 1851 to illustrate the delineation of lines of magnetic force by iron filings. The filings were fixed on cartridge paper by means of gum water, sometimes mixed with a solution of "the red ferro prussiate of potassa" (apparently potassium *ferri*cyanide), which was found to leave a blue impression of the pattern after the filings had been removed or worn off. The method of preparation is referred to on pages 9-10 of this volume and also described in par. 3236 of the Twenty-ninth Series of Experimental Researches in Electricity *(Exptl. Res. Electy.*, vol. III, p. 398).

Some of these specimens are still in a very good state of preservation; others have deteriorated, particularly some of those in which the ferricyanide was used: the filings have worn off and the blue colour has spread all over the paper, making the pattern indecipherable. In a number of cases the red of the oxidised iron particles is left against a blue background, and fortunately this combination of colours reproduces well photographically. A selection has been made, and those specimens considered suitable will be found reproduced (by the collotype process) in Plates I to VII.

Folio Vol. VII of the manuscript contains three large folding sheets of graphs, in which Faraday plotted his measurements, made in the summer of 1855, of the variation of magnecrystallic force with temperature for several different substances. The numerical data on which these curves are based will be found tabulated in the Diary (see notes in square brackets on pages 424, 427 and 491). The curves themselves are so large and so finely drawn and lettered that they could not be conveniently reproduced in their original form. They have therefore been omitted.

The frontispiece to this volume is of especial interest. It represents Faraday giving the Christmas Juvenile Lectures of 1855-6, on The Distinctive Properties of the Common Metals. The course was attended by H.R.H. The Prince Consort, accompanied by his two sons, the Prince of Wales (afterwards King Edward VII), who is seated on his right, and Prince Alfred (afterwards Duke of Edinburgh), on his left. Sir Charles Lyell, Sir James South, Dr Warren de la Rue, Dr Bence Jones, Professor Tyndall and others well known at the Royal Institution at the time appear in the audience. Faraday's assistant Anderson stands behind him with hands crossed. The original oil painting, by Mr Alexander Blaikley, was done from sketches obtained at the lectures. Lithographic copies of the painting were afterwards made, from a drawing on the stone prepared by the artist himself. It is from one of these lithographs, hand tinted, which is in the possession of the Royal Institution, that the present reproduction has been made.

T. M.

ROYAL INSTITUTION
February 1935

Continuing the practice in the previous volumes, the cross references to Faraday's published work included in the Contents are, as far as possible, to the collected editions of his papers. The majority of the references in the early part of this table are, it will be seen, to the *Experimental Researches in Electricity;* but the third and final volume of that work appeared early in 1855, and electrical papers published after that date must be consulted in the journals in which they were originally printed. Thus the Thirtieth Series of Experimental Researches in Electricity (the last of that sequence) and certain other papers referred to below, were not included in the collected edition, but will be found in the *Philosophical Transactions of the Royal Society,* the *Proceedings of the Royal Institution* and other journals to which the references have been given.

FOLIO VOLUME VI OF MANUSCRIPT *(continued)*

1851

FOLIO VOLUME VII OF MANUSCRIPT

PLATES

INDEX

FOLIO VOLUME VI
OF MANUSCRIPT
(CONTINUED)

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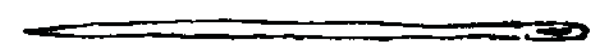
11666. Wanted to know how the lines of force were disposed in and about magnets and Iron generally under certain circumstances of position, and for this purpose used *fine* iron filings upon paper over the magnets, sprinkling them evenly and tapping the paper lightly.

11667*. First a simple magnet, being a large needle of about this[1] size, well magnetized by a horseshoe magnet of power. It gave beautiful curves having perfectly simplicity of form, but is to be remarked that N or S lines issued not from the ends of the needle but far down towards the middle. In order to distinguish the ends of the lines, we may call those at N, N issues or ends or nodes; and the middle part *c* the equatorial center or ventrum.

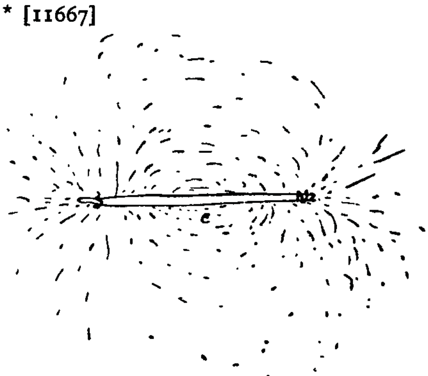


11668. When the needle was broken into two parts, each part by itself acted well as above (11667). When it was put together again, it was no more as one magnet but thus†; there being four consecutive poles and consequently *two* equatorial ventrums, or rather three, but the middle one at the junction very short and compressed and the direction of the curves outside of the magnet the contrary of that of the parts outside to the right and left.

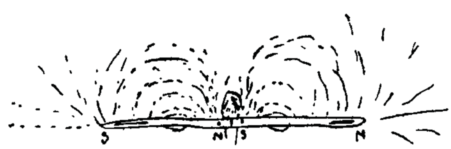
11669. Now indeed it appears that certain of those curves which before were entirely within the body of the magnet are expelled into the air, because of the sudden diminution of conducting condition at that spot by rupture and want of continuity, and that of those which thus come out through the sides of the magnet, part returns and is discharged at the nearest pole and part goes on and dips into the further half. So that the bundle of lines of force are divided generally thus‡ into three parts—a part which goes down the middle of the magnet, right across from one to the

[1] Reduced to 3/4 scale.

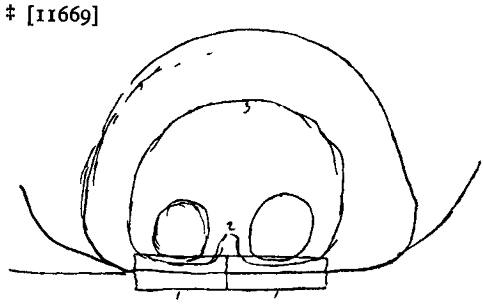
* [11667]



† [11668]



‡ [11669]



other at the break, out at the poles, the circuit being completed in space above as in the unbroken magnet; a second part which issues out at the break, but goes on and dips in again and goes out towards the poles on each side or end, to be completed in the space above but within the region of the former lines; and a third part which, issuing at the break, forms (by returning) two systems. These are the external lines of force which belong to each half needle acting as an independant magnet. The two others are the external (chief) lines of the two halves of the needle acting as a whole magnet. But the first part or portion is the only one whose lines are confined within the dimension of the magnet in the whole of its course.

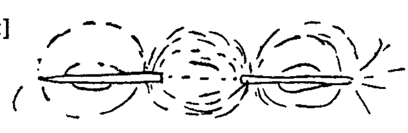
11670. All the lines of force within the magnet are only so at the equatorial centers or ventrums 1, 1; as in a perfect magnet they are within it only at the equatorial part. Of the other parts, some of those which go into the space around at the pole are also in the space around at the junction, where of course they have an opposite direction. Consequently somewhere between 2 and 3 there must be a neutral place or a place where no magnetic force is exerted, at all events as regards direction (11672, 7).

11671. When the two halves of the needle were opened out thus*, the middle air portion of the curves was well developed. But though the filing lines were more developed there, it is certain that less power passed across or on them than before, and less and less as the space was increased. At the same time, more returned back from the inner end of each half to the outer end, i.e. the third portions (11669) increased continually. And they would do so until at last they would become the independant systems of the two completely separated halves, which then would exist as two magnet[s], each having its own equator or maximum place of inner curves which would contain just as much force as the original equator, provided the magnet could be broken without a letting down of the state within.

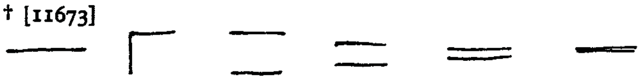
11672. Then the neutral place (11670, 7) would be at an infinite distance.

11673. The two halves of the needle were carried through a series of successive positions, so as to make them represent the horse shoe and compound cases; the positions were these† in the

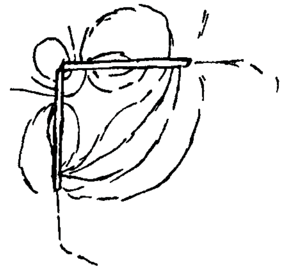
* [11671]



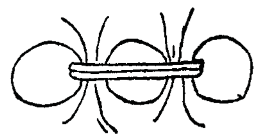
† [11673]



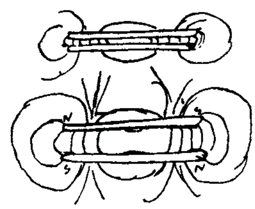
first instances. Now when in a line, with unlike poles together so as to make one long magnet—the curves have been noted. When placed at right angles, of course unlike poles together, then the curves were generally as represented. It is easy to see how this disposition arises and is developed from the former (   ) and how some of the curves of the third portion are now removed into the second, going across the air both at the poles in contact and at those which are now 90° instead of 180° apart.



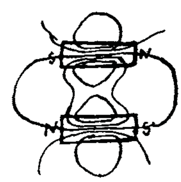
11674. The two halves were now brought together, as if one had made a further movement of 90°, so that they laid side by side with like poles together. In this case the course of the lines of force between the magnets were lost sight of and only those depicted remained for the filings to shew. It is easy to see how they arise from the former disposition, but the system is now weak and it must be remembered that by far the greater number of lines of force are passing directly across from magnet to magnet and are not sensible here without close inspection.



11675. To make these visible the little magnets were opened out slightly and then the lines were thus—in which it may be seen how, as the magnets are placed wider apart, the equatorial ventrum of each small magnet begins to appear as the lines of force of each magnet tend to go back from one pole to the other, rather than across the intervening space to the opposite pole of the opposed magnet. When the space was further increased, then the distribution of the lines was thus*; and when it was still further increased, it was as in the left hand figures†. All these cases are easily referable one to the other and flow as a very natural consequence from the nature and character of the lines of force.

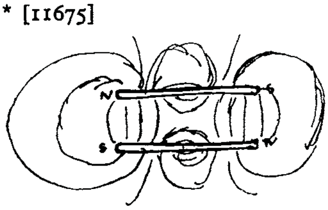


11676. In the last case for instance, the lines through the magnet and outside generally as represented. Some pass through both magnets, while some turn short round and do not; and the maximum internal effect is resident within the equator of either magnet.

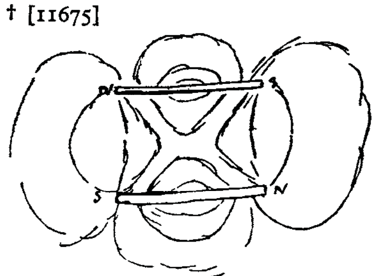


11677. The neutral region (11670, 2) is here very distinct and it is easy to see that a particle of iron placed there will *not be attracted* or *point*. Must trace this to the neutral place in (11670). That

* [11675]



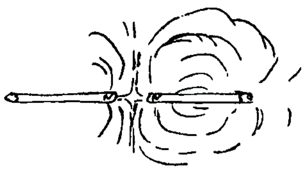
† [11675]



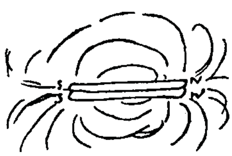
can hardly be at an infinite distance or even at a distance where the forces are insensible.

11678. When two bar magnets each a foot in length were placed in like positions to those described, with contrary pole[s] together or in relation to each other, they gave the same results.

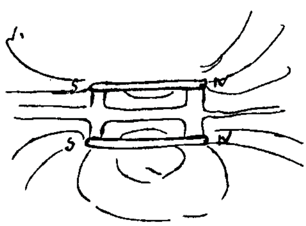
11679*. The two small needle magnets (   ) were now placed in a line with like poles near each other. The systems of curves or lines of force had the expected and well known disposition, being compressed at the place of contact and limited each for itself by a plane there at right angles to the line of the magnets. When opened out a little, the disposition was in principle the same, and it is easy to see how by great removal apart the two magnets acquire their fully developed systems in their ordinary form.



11680†. When these were placed at right angles, then the lines within the angle were compressed together and those from the outer or S poles began to be thrown out; and when the moving part was carried through 90° more, so as to lay the magnets side by side, the inner lines of force disappeared between the magnets by a very natural transition, and the two magnets acted as one. A section through both would of course give twice the number of lines of force as a section through one, and if each magnet were to retain its full power under all these alterations, such a disposition would give double external power as indicated by lines of force than if the two magnet[s] had been placed end to end. Must consider this in the case of magnetization by helices, etc. etc.



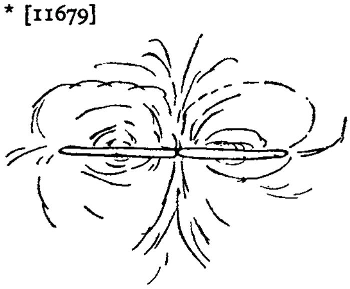
11681. When the two magnets, being together and parallel, were opened a little thus, then the forms of the lines were as shewn, the ærial expansions at the equators appearing between; and it is evident how by further removal the two systems would gradually resume their perfect form.



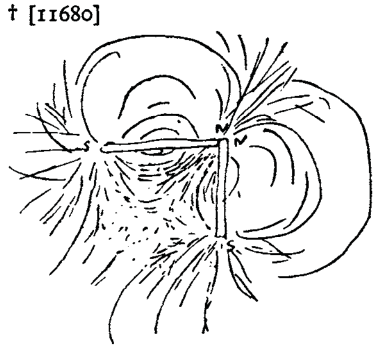
11682. It is exceedingly beautiful to see in all these arrangements how beautifully the lines of force represent the disposition of the magnetic power.

11683. Considering Air, Iron and all things external to the magnet as mere conductors of the magnetic lines of force, I observed the

* [11679]



† [11680]



curves by filings over one of the small needle magnets when sustained on *wood* and also again when lying on a mass of soft unmagnetic *iron*. The form of the curves as shewn by the filings was just as good in the one case as the other, but the quantity of power or force which gave the form was very much diminished in the latter case; and though it was manifest by the disappearance of much power from the air that the iron was a far better conductor than it, still a good deal of power remained to be transferred through the air (11960, 1, 2, 3, 4).

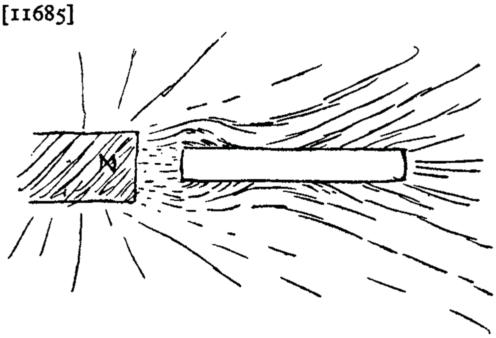
11684. As the power is definite and does not change in its amount though it does in its disposition in the two, it seems to me impossible to make a distinction between it in the iron and in the air, except as to amount. The polarity of the lines of force is the same in both cases and the iron has no more power of retaining them than the air, or water, or wood or other matter.

11685*. Again, placed a piece of soft iron opposite the pole of a bar magnet a foot long, an inch broad and 0·4 thick (the figure is to a scale of about one half[1]); and then placing paper over the bar and magnet, sprinkled fine filings on and observed the lines depicted. It was beautiful to see how they flowed into the iron at the end near the magnet and how they flowed out again at the further part from a comparatively much larger surface—and also to see the concavity of the lines outside the iron near the equatorial part of it, shewing the double curvature—and the beautiful character of the streams of force into the air or space from the further part.

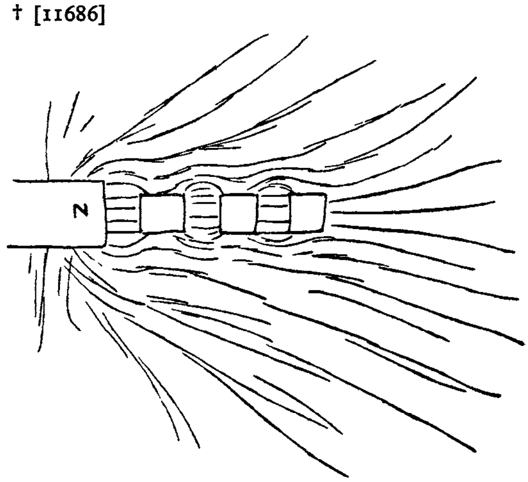
11686†. Again. Several short thick pieces of soft iron, being parallelopipeds, were adjusted before a magnet, air intervening, and here the entry and exit of the lines of force at the different cubes or pieces was very beautiful, giving a fine display of the

[1] Further reduced to 3/4.

* [11685]



† [11686]



disposition of the power. The filings tended to leave the faces over the iron and go to the parts over the air intervals, shewing the greater manifestation of power or stronger intensity of the lines were passing through worse conductors than where they had dipped into the iron and were there carried on. The side undulations too shewed this disappearance and reappearance of the lines of force at the sides of the iron, and this was shewn again by raising the paper supporting the filings *from the iron* faces. For whereas the filings, when close to the iron, did not depict lines of force but just went as the agitation favoured their movement to the parts over the air, i.e. from weaker to stronger places of action; when the paper was raised, then the forms of the curves reappeared over the iron, first at the edges and, as the paper was more raised, at the middle part, and with beautiful delicate indicating curvatures respecting the equatorial part of each piece of soft iron.

11687. The manner in which the iron robs the space around its equatorial part is very striking, and I have no doubt that a little needle *close* to the iron at either the top or bottom or side faces as regards the axial direction, would be indifferent to the lines of force flowing all about it (11960, 1, 2, 3, 4).

11688. So an ordinary magnetic needle placed on a large block of iron is altered in its relation to the earth, and I have no doubt that a dipping needle having a block of iron placed parallel to it—or being inside a thick tube of iron—would be taken out of the earth's action considerably and indifferent to it.

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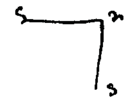
11689. A piece of *hard steel* in the place of the long soft iron (11685) acted very slightly in the same way that it did—but not to be compared in amount of effect. It also attracts either end of a magnetic needle as iron does. One would like to obtain a piece of steel so hard that it should refuse induction and be as air, if not brought in contact. The case occurs almost with a bar of hard steel, a very small magnetic needle and the earth's lines of force.

11690. Worked with the 12 inches bar magnets and the six inch bar magnets for the curves on paper by filings and for the neutral point (11670, 2, 7). The 12 inch bars are too long. The six inch bars will do well for length, but require magnetizing by the electromagnet. At present they are very irregular, and the filings shew it well when the two are opposed thus. The place of the neutral point is very well obtained with them ( ).



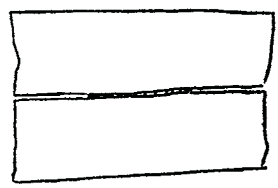
11691. *Filings.* The finer they are the better for the production of neat lines by little use of extraneous matter.

11692. Floating on water will not do—they cling together.

11693. The lines obtained on a sheet of glass over the magnets are very good with large magnets, but hardly do with small needle magnets because of the thickness of the glass. Else the curves are very regular, because the glass does not cockle.

11694. Silvered or Gilt paper does well, the filing[s] slipping into place very easily. Useful in the investigation of neutral places, etc. where the power is small.

11695. Now proceeded to fix the curves as depicted by the fine iron filings, and succeeded in several ways. Thus, the half needle magnet was placed between the edges of two cards so as to make a flat surface, then a piece of cartridge paper, flat, laid over it; sprinkled over with fine iron filings, and the paper tapped with a splinter of wood here and there until the filings were well arranged. Then a similar piece of cartridge paper was brushed over with moderately thick gum water and a camel hair brush—laying the fluid evenly—and wafted through the air a few times, which breaks the air bubbles produced by the brushing. After this it was carefully laid on to the paper sustaining the filings—a cushion of 16 or more folds of filtering paper placed over it—a thick flat plate of glass on that, and then pressure given by the hand or a 56 lb. weight for half a minute or more. On being taken up, all the filings in their proper places were attached to the gummed paper and when that was dried were fast attached to it. No. 1 on the next page [Plate I] is the very specimen and the first made.



11696. Then tried to print off the curves, and therefore in place of gumming the second paper, it was washed over with a solution of yellow ferro prussiate of potassa and pressed on as before. It took up the filings and they were allowed to remain on until dry; but being then brushed off, the traces left behind were scarcely visible and I have destroyed the result as useless.

11697. On brushing over the second paper with a moderately dilute solution of the red ferro prussiate of potassa, and applying it in the same way, the final result was a very excellent delineation in Prussian blue of the position of the filings and the curves. The results will do perfectly well as drawings to go with the paper and to the artist. Nos. 2[1] and 3[1] are the two first specimens so produced.

11698. No. 4[1] is a similar preparation but the filings were sprinkled on a plate of glass laid over the magnet, instead of being sprinkled on paper.

11699. Also employed gum water and the red ferro prussiate mixed, so as to retain the design in filings as long as it would wear and leave a design in Prussian blue afterwards. No. 5 is an illustration [Plate I], and shews how finely the lines may be traced with fine filings.

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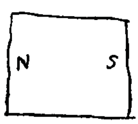
11700. Rubbed the filings in a mortar and sifted out the fine particles. But it being principally oxide dust, though it forms curves, they are not so beautiful as those with clean small filings—nor so well taken up by the gummed paper.

11701. With large displays, the size of this leaf[2] for instance, it is best to lift the paper carefully up from the magnet, to lay it on a flat surface, and when the Gummed or test paper is put over it, to put on 6 or 8 thicknesses of filtering paper and, holding all tight, to rub it down on the filings. With care they may be well taken up this way. But pressure is better for small printings or designs, only the sustaining surface should be flat and equal.

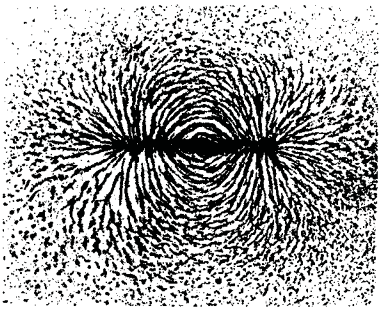


11702. When a thin plate of steel, very hard, is magnetized and then broken down into short length[s], the short wide magnets are very interesting. For instance, a short one such as figured will give curves extending to twice or thrice its own length all the way round.

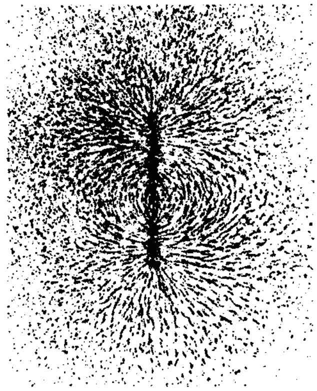
11703. Had a long steel wire 15 inches long and not above 0·05 of an inch in diameter. It is difficult to magnetize it, though soft, as one magnet, consecutive poles starting into existence; but did so at last by our helix (large one). Then took its picture as to

[1] Not reproduced.
[2] Foolscap size.

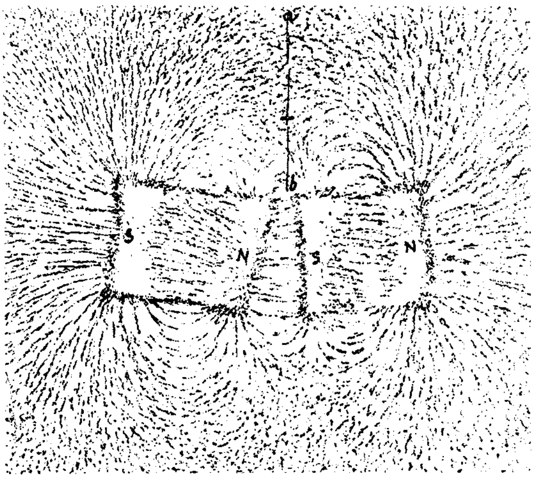
PLATE I



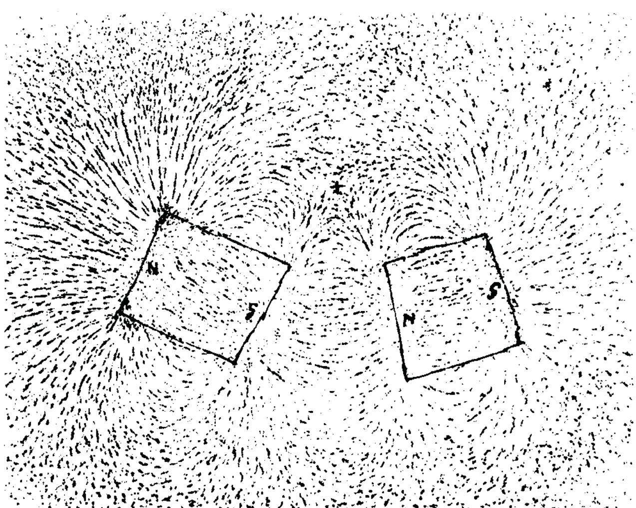
Par. 11695. No. 1. Gum water alone
(*three quarter scale*)



Par. 11699. No. 5. Red ferro-
prussiate of potassa and Gum
water (*three quarter scale*)

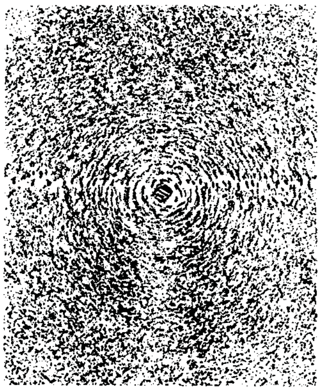


Par. 11704. (Upper figure, *two thirds scale*)

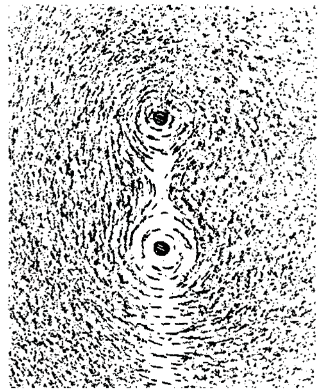


Par. 11704. (Lower figure, *two thirds scale*)

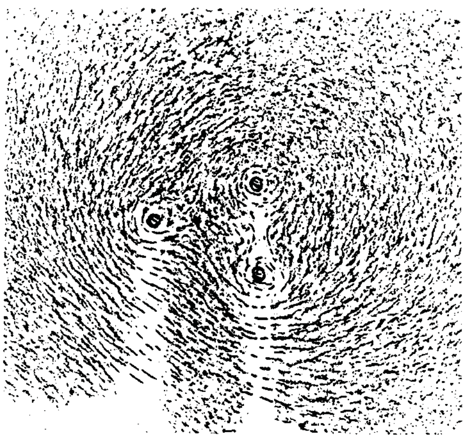
PLATE II



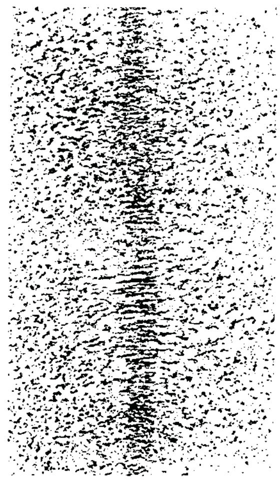
Par. 11891. 1 wire



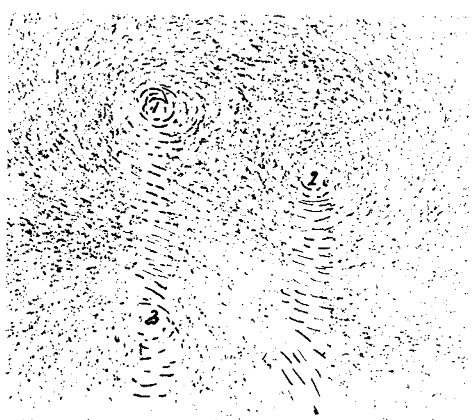
Par. 11893. 2 wires – like currents



Par 11895. 3 wires – like currents



Par. 11896. Lines across a single wire



Par. 11895. 3 wires – 1 and 2 like currents, 3 contrary current

(all three quarter scale)

11891. A wire was arranged perpendicularly and the Current of 10 pair of Grove's plates sent through it. Filings sprinkled on paper assumed the form given in the following attached experimental result [Plate II].

11892. Wires of three different thicknesses were used, but there was no difference in the results. The power or rather the extent of the curves appeared to depend upon the amount of electricity in the current.

11893. When two wires carrying like currents were employed, the form[s] were very beautiful, shewing the coalescence of the lines of force, the inflection between the two, etc. etc. [Plate II].

11894. It appeared as if the two wires had twice the force of one wire, though the forms coalesced. As if the power of one was added on to the other and with no disappearance of the power of either. It was not a mere coalescence as in magnets, but the force was a sum of the two. There must be a neutral place between the two wires.

11895. With three wires carrying like currents the result was similar, as in the illustration [Plate II]. When one of the three currents was in the contrary direction, then the neutral place disappeared and the lines flowed between it and the others [Plate II]. Shewing the element of the helix.

11896. The lines over a current are given in the figure [Plate II] and are of course transverse to it.

11897. It is easy to trace and connect all these lines with those in the helix. Such as are produced over a ring helix are given in the side illustration[1].

11898. I have a helix made of 36 spirals of copper wire 1/6 of an inch in diameter. The wire is not covered and the spirals do not touch; its length is 9 inches and its internal diameter 1·8 inches. It is easy to obtain access to the inside and, by placing a wooden half core within, the disposition of the lines in a plane passing through the axis of the helix could be shewn by filings both within and without. The opposite illustration, A [Plate III], shews the forms taken by filings both within and without about the end of the helix.

11899. Except near the ends, the lines within the helix were

[1] Not reproduced.

parallel or nearly so to each other from end to end, but when a core of soft iron about 2½ inches long and 1/8 of an inch thick was within, it disturbed the forms as in illustration B [Plate III].

11900. A still large[r] core, being a soft iron rod 5 inches long and 0·3 in diameter was introduced. C [Plate III] shews the result as to one of the extremities and the middle of the piece; the other end could not be conveniently recorded but was as the one rendered visible and as the result B. It is evident that the iron does not merely conduct the lines of force from the helix but is a new and abundant source of them. Those of the helix and the core associate beautifully together.

11901. When the same core was out of the axis and against the inside of the helix, then the result was as in D [Plate III].

11902. When the great helix alone was employed, always with the 10 pair of Grove's plates, and the form outside assumed by filings was observed, the lines were feeble, but as in the figure, where the intersection of the coils by the plane passing through the core axis is indicated—E[1].

11903. But when the core of soft iron 5 inches long and 0·3 in diameter was in the inside of the helix, then a great difference resulted; many lines of force issued through the side of the helix and the place of the magnet was easily seen by the place of the magnetic equator—F[1].

11905[2]. When two such cores were employed together, either side by side or end to end, there was much increase of power, and the lines shew where and its direction—in G[1] and H[1].

11906. A similar result is shewn in I [Plate IV], where a larger core was used.

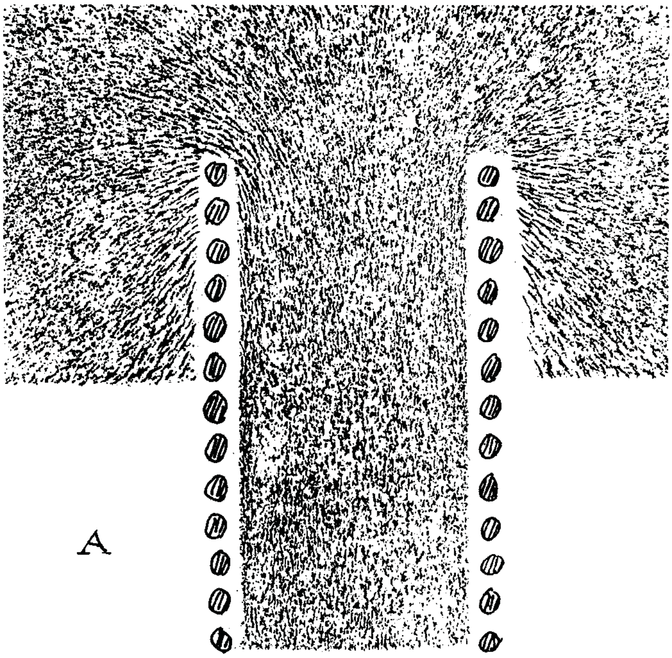
11907. Results with a helix consisting of four superposed and close together shew similar and correspondant results. See [K (not reproduced) and L (Plate IV)].

11908. I placed a bar of iron *outside* the helix—it was not absolutely insensible, for some of the lines of force from the ends of the helix feebly bent down towards it, but there was scarcely a trace of power. This has been stated before. See M[1].

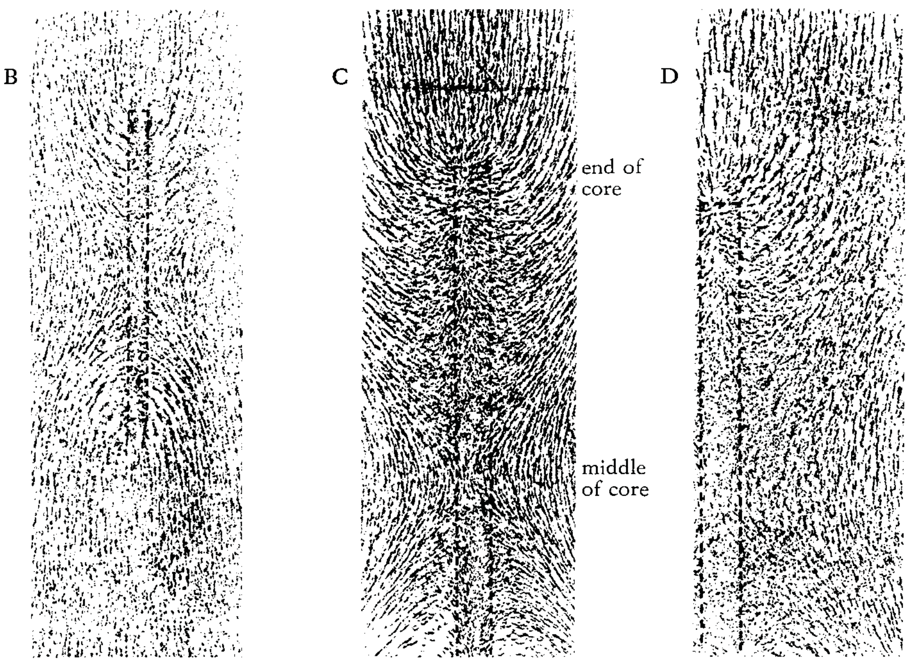
11909. Taking a long piece of soft iron wire 9·3 inches long,

[1] Not reproduced.
[2] There is no 11904 in the MS.

PLATE III



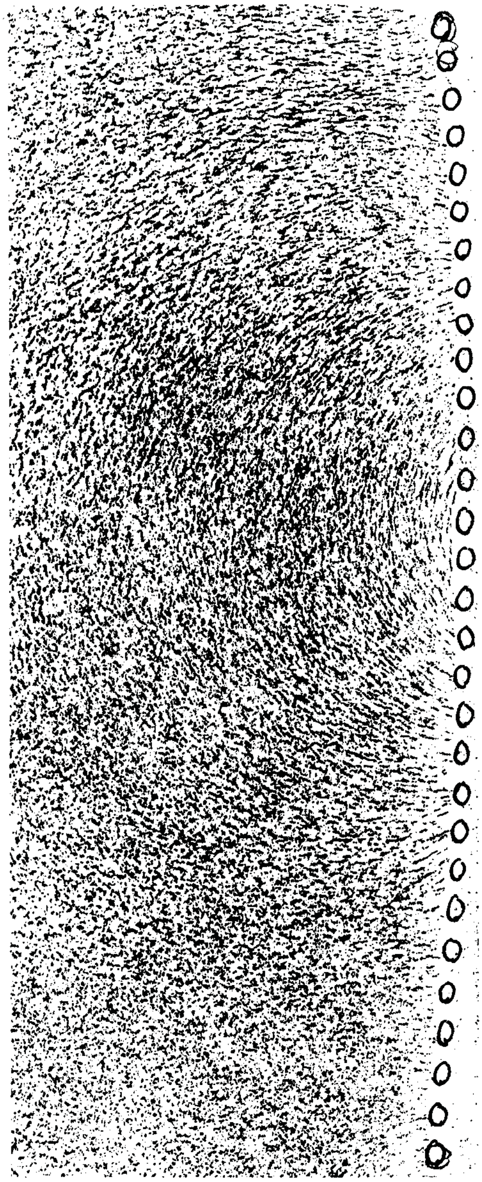
Par. 11898. A. Lines at the end of the large wire helix within and without – in a plane through the axis



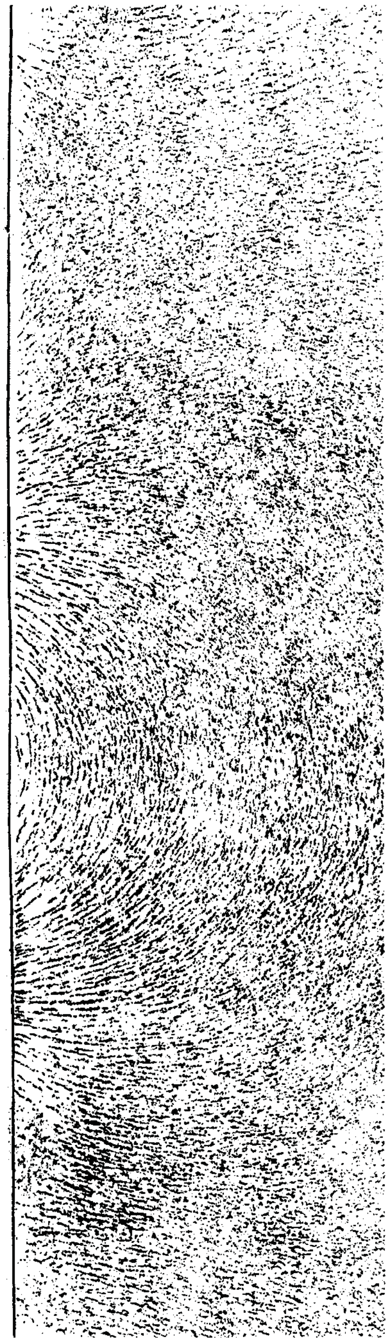
Par. 11899. B. A little core quite inside the helix
Par. 11900. C. Core of soft iron 5 inches long by 0·3 wide in the inside of the large helix
Par. 11901. D. Same core in the helix – at the side

(The outlines of the iron cores, shown dotted here, are drawn in ink on the backs of the specimens. All seven tenths scale)

PLATE IV



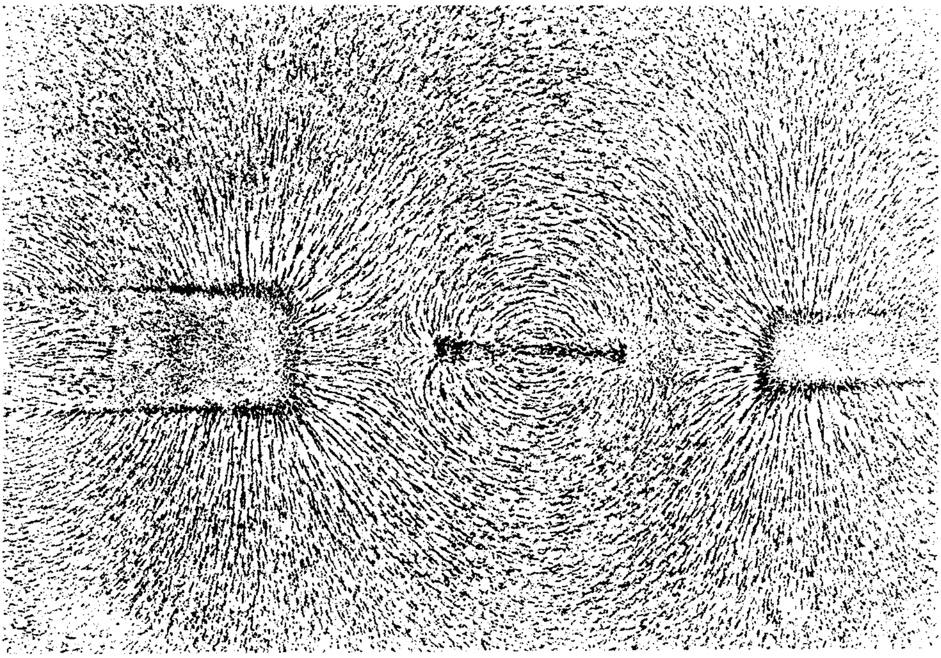
Par. 11906. I. Core – square bar soft iron 4·1 inches long and 0·5 thick



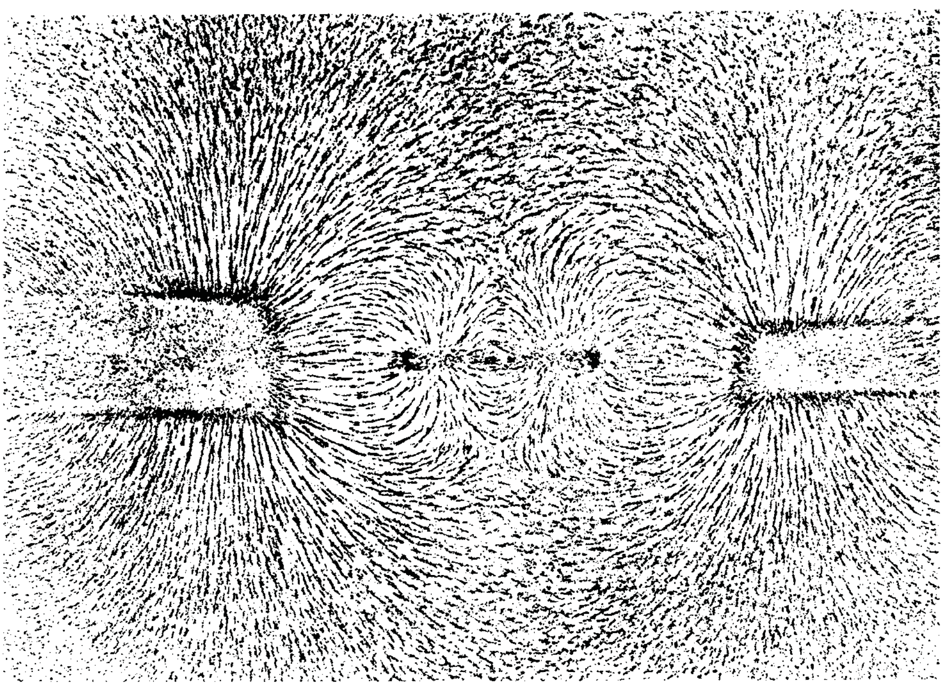
Par. 11907. L. Leather helix – core inside soft iron, 4 inches long and 0·3 diameter

(two thirds scale)

PLATE V

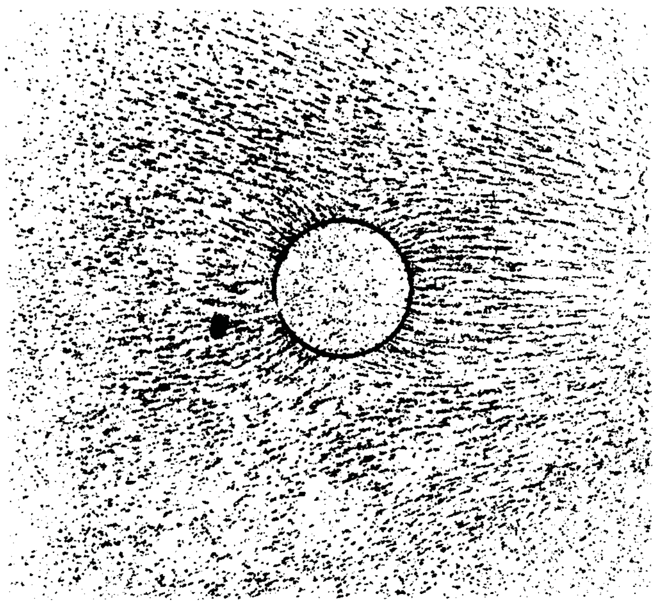


Par. 11911. R. *(two thirds scale)*

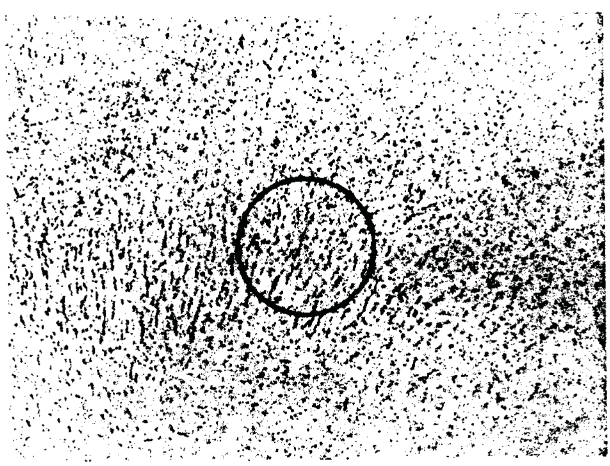


Par. 11911. S. *(two thirds scale)*

PLATE VII



Par. 11928. [C]



Par. 11928. [H]

(The circular outline of the nickel is drawn in ink on the specimens. Full size)

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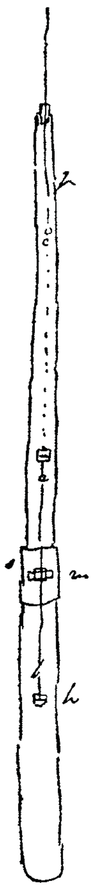

11929. Magnetic effect of Sul. Iron or Oxygen.

With the hope of making an apparatus that might shew the effect of oxygen, constructed the following apparatus. *a b* is a fine straight slender filament of glass, suspended by a cocoon thread to the end of the wire *c*, which going through a cork, is by that held fast in the mouth of a compound tube, for *g* and *h* are two glass tubes connected by a copper socket at *s*. Now the glass filament is 12 inches long and its suspension thr[ea]d perhaps 16 inches in length. At the top and bottom of the glass filament is attached a small magnet, one at each end. A fine needle was well magnetised, then broken up into equal pieces about 1/8 of an inch long, and there are two of them. They are attached in contrary directions to the filament and thus make something like an astatic needle, except that, being not quite in the same plane but thus  , the apparatus does not point in the magnetic meridian but across it, with the marked ends towds. the north. This did not interfere with the expected action. A small mirror consisting of a piece of the silvering stripped from a mirror was fixed on at *m* to the middle of the glass filament, and this was brought opposite a notch cut in the copper socket, so that a ray of light could pass in and after impinging on the mirror, could be reflected out again to a telescope and observer; so that any degree of motion in the whole system could be observed.

11930. Now this apparatus being supported vertically so that the needle system was free to swing, it took up its place under the earth's influence. The needle was very delicate in its indications towd. or with any ordinary magnetic body. For the finest observations a candle was placed five feet off and its image in the reflector observed by a telescope. The image was bad but quite sufficient for the purpose.

11931. A bottle of crystals of proto sul. Iron shewed no effect on this needle by the earth's action. No signs of its conducting power or polarity or conduction polarity.

11932. A Jar of solution of Proto sulphate of iron (saturated), being 3½ inches internal diameter and 13 inches deep—did not affect it.

11933. A Glass bottle 6 x 6 x 7 inches in size, being filled with the solution, did not affect it.

11934. Thes[e] things were tried in various positions, but no power of deflecting the lines of force of the earth appeared; there was not force enough for that, and of course there could not be any hopes of oxygen.

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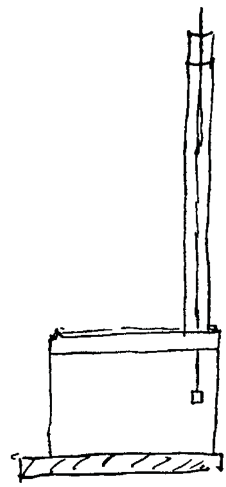

11935. I have prepared another magnetic needle. It is part of a needle, broken out of the middle after magnetization, and is not longer than this ⟹: it is fixed on the back of a small piece of silvered glass about this size  and suspended by a single cocoon fibre in the square glass cell and tube of former times (11194, 249), from the end of a wire passing through the cork. A candle flame is well seen in this reflector by the telescope used on former occasions (11194, 249), and by means of the telescope wire, the position of the mirror (and needle) can be most readily ascertained and any change in position taken note of. The image of the candle flame was very good (11930).

11936. Having a large roundish block of hæmatite weighing 14 lbs., and about 6 inches in thickness every way, it was employed to see whether it would make a sensible deflection of the lines of magnetic force of the earth—a small piece being absolutely insensible in its action. It was therefore placed to the east and to the west, above and below the needle, at various times for this purpose being sustained on a table standing on a stone floor, whilst the needle was sustained on the end of an arm on another stand on the same stone floor. No mechanical disturbance of the needle occurred therefore when moving the heavy hæmatite by its mere weight.

11937. The hæmatite deflected the needle differently in different positions, and this shews a ready sensibility in the needle and goodness in the observing apparatus. The apparent motion of the reflected image to the right or left of the observer is, for the telescope, the *reverse* of that for the naked eye—the telescope being an inverting instrument. When therefore the image in it goes to the right hand, it is really going to the left as seen directly by the naked eye. This correction is needful in determining the way in which the mirror and the needle moves.

11938. But the hæmatite proved to be slightly permanently magnetic, and therefore did not give any clear evidence of its affecting the terrestrial lines of force or becoming a magnet sensibly by position.

11939. A Winchester quart bottle of proto sul. Iron solution shewed no trace of magnetic effect by position. Was indifferent as on the former occasions.

11940. A remarkable proof of its sensibility occurred and the influence of distant things. It was on the basement floor between the Laby. and the street. Carriages passing in the street would be about 8 or 9 feet above the level of the needle and from 20 to 25 feet West of it at the nearest. As the carriages or carts passed, they affected the needle, and that in a constant manner; for whether they came up from the North or from the South, the S end of the needle went a little west and returned to its natural position as they passed away. If it was a quick passing carriage the action was quicker but quite steady, just one to and fro. If it was a slow moving cart or waggon, it was a slower motion but not a steadier one. When the vehicle was on the near side of the way the action was greater than when on the other side farther off. Considering every circumstance, I referred it to the action of the iron tires of the wheels, chiefly, the whole iron of the passing vehicle however combining in the effect. It is a magnet by position, i.e. it deflects the lines of terrestrial magnetic force, and this deflection was shewn by the needle, the motion of which is precisely that which would be produced by such an action.

11941. Try such a bar, and if there be an effect, consider how dilute the iron would be by expansion into a cube of 40 or 50 feet in the side, and whether if so diluted a small portion would be sensible at the needle. Thus a bar 4 feet long and 1·5 inches square occupies 108 cubic inches. A cube 40 feet in the side occupies 110592000 cubic inches. Now 108/110,592,000 = 1/1,024,000. So that the iron

would be above a million times diluted in that space, and a cubic inch of it so diluted would contain only the 1/110,592,000 dth part of the iron. Now a cubic inch of iron weighs 1969 grains, and

108 c.i. = 212,706 grains. The 1/110,592,000 dth part of 212,706 grains is rather less than the 1/2000 dth of a grain. Then query would the 1/2000 dth of a grain of iron, formed into a wire whose thickness should be 1/32 of its length, and placed half an inch off from the needle, affect it as much as the bar 20 feet off (11959).

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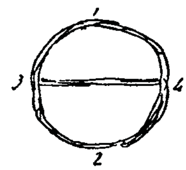
11942. Want to try a mass of Proto sulphate of iron, either as crystals or in solution, to ascertain whether it will sensibly affect the direction of the lines of force of the earth—that I may approach a step to the action of oxygen; for moderate masses of Sul. Iron do not sensibly affect a magnetic needle, though they are affected by a magnet (   ).

11943. 10 oz. avoirdupoise of small crystals proto sul. iron shaken together occupy about 15 cubic inches. The same quantity of crystals would make about 35 cubic inches of cold saturated solution.

11944. Have prepared a cask. It has wooden hoops. I have taken out all the *iron* nails and replaced such as were needful by copper nails. When standing up on end, the height within is 22 inches and the average diameter 16•875 inches, so that its capacity is nearly 5000 c.i. or 18 gallons. A loose division of wood has been made vertically down the middle to confine solid crystals to one side, if needful, and it has been marked 1, 2, 3, 4, on the sides to identify these sides when required.



11945. The needle and its reflector (11935) were first arranged on its separate stand on a stone floor, at the proper height, and also the candle, 73 inches off, to give an image in the reflector and the telescope, eye end 20 inches off, to observe. The needle was of course in the magnetic meridian within the room; the telescope and candle were west of it as in the next Figure.

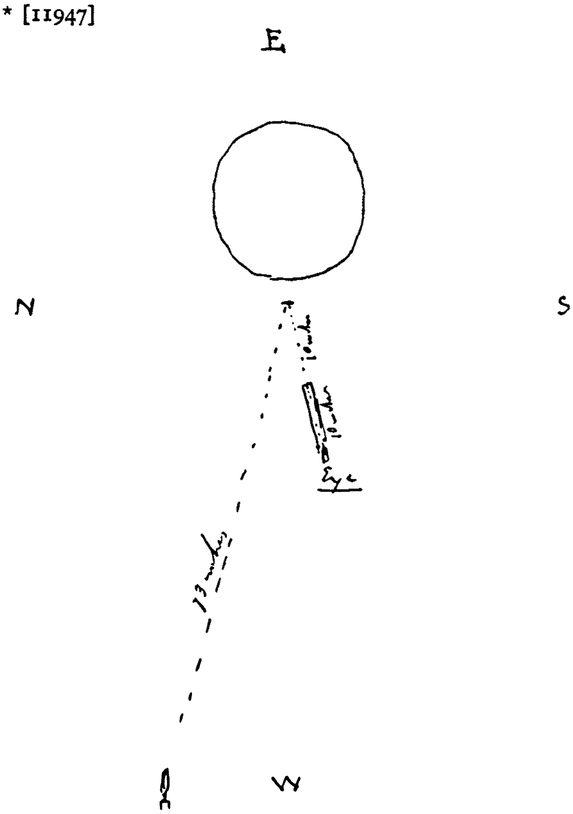
11946. A travelling stool or platform was prepared to carry the cask. This by moving on the floor brought the top of the cask level with the magnetic needle, or being placed on a table where it could move 12 inches or more, then the bottom of the cask was level with the needle. This platform and the cask were to the East of the needle, and moving east and west could be brought up within 1 inch of the needle, or when on the ground taken _____ feet off.

11947*. The Telescope inverts the object seen, but experimentally, whilst it and the candle are stationary, the movement of the image *in the telescope* to the *right* of the observer at Eye occurs when the S end of the little magnetic needle goes *Eastward,* and the motion of the image in the telescope to the *left* coincides with the movement of the S end of the needle to the *West.*

11948. First the Empty cask with its partition and supporting stool or platform was tried, the top being level with the needle. It was brought up to within an inch of the needle, or taken 52 inches off, always on the East of the needle—a careful observation was made in each position, and this was done for the four sides or faces of the cask (11944). With side 1 near or towds. the magnetic needle there was no change in the position of the needle, no deflection when the cask was nearest. With side 4 towards the needle, the same indifferent result. Sides 3 and 2 gave the same indifferent result. So the cask and stool or platform unexceptionable whilst below the level of the needle.

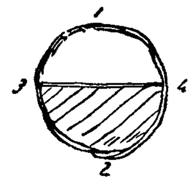
11949. The empty cask was now raised on the table (11946) so that its *bottom* was level with the needle—in this case it could travel 12 inches to and fro—its different distances being 1 and 13

* [11947]



inches. Now the side 3 gave a slight deflection of the image in the telescope to the apparent left = to about half the width of the candle flame. The sides 1, 4 and 2 gave the least possible deviation in the same direction when the cask was at the nearest. This looks like a trace of repulsion on the S end of the needle. It should have been the *contrary,* if the cask had acted in the *manner of iron.* Perhaps it was something in the travelling stool, in which there were iron screws, etc. It was very small.

11950. Crystals of Sul. Iron proto, 112 lb. in weight, were now put into the cask on one side of the division and just filled it to the top on the side 2, thus; and the whole being on the table and so mounted above the level of the magnetic needle, was experimented with, the distance of motion to and fro being 12 inches.



11951. Now when side 2 was near the magnetic needle, the image in the telescope went to the *left* distinctly, about half a flame width. When the cask was removed 12 inches further away, the effect ceased—the effect occurred again and again. With side 3 towards the needle the same thing occurred. With side 1 also the same—but the effect better, though now the sulphate is farther off. With side 4 the same thing. So the general result is that the Sulphate of iron causes the S end of the needle to go *Westwd.,* as if it were repelled. This is the *reverse* of an iron action.

11952. The cask was now placed below, with its top level with the magnetic needle. The extreme distance was 62 inches and the near distance 1 inch. Now with side 2 towds. the needle there was no effect or only a trace to the left. Side 3 to the needle, no effect or a trace to the left. Side 1 nothing. Side 4 nothing.

11953. It would seem by this that the deflection when the cask was raised could not be due to the cask itself but to its contents. Also that it could not be due to the weight of the cask and contents depressing or changing the pavement, for that must have been nearly the same when the cask was below as when raised. But that it is not due to the sulphate of iron as a para magnetic action is evident, because a spike of iron in its place, when raised made the image in the telescope go to the *right,* and when lowered sent it to the left.

11954. Perhaps the flexure on the floor may have altered even the place of the telescope, but I cannot tell. The effect was very small after all, and I do not accept it as evidence that the sulphate of iron had *any sensible action* of its own, either paramagnetic or diamagnetic.

11955. *Cast Iron.* Six bars of cast iron, making together a compound bar 4 feet long, by 2 inches wide and 1·75 inches thick, was held up in the area to the West of the magnetic needle, 13½ feet off, and raised so that the lower end was 2½ feet above the level of the needle. Whether held vertically or horizontally in the plane of the Magnetic meridian, they did not affect the needle. The Window was between and at the window there are 10 or 12 wrought iron vertical bars, which may have interfered.

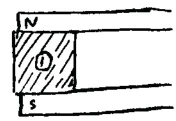
11956. The same bars 6 feet off to the East with no intervening iron affected the needle *properly,* but not much. At the distance of 3 feet the effect was much more. So the Earth's power strong and abundant, and not very easily disturbed by a small thing.

11957. An iron spike or anvil (wrought iron) about 24 inches long, 6 inches diameter at one end and 3 inches thick in the body, having a weight of 70 lb. or 80 lb. or more, when placed 3 feet off, affected the needle considerably and properly, i.e. as it ought to do, whether above or below or horizontal or with either end uppermost. But the effect was nothing like so great as I expected it to be, and considering what it did, the insensible effect of the sulphate of iron in the cask was no longer surprizing.

11958. The carts and carriages that go bye certainly affect the needle, but I doubt whether it is a magnetic effect, and not rather some effect of pressure on the ground and flexure of the whole building, and so change of place of the iron in it and consequently of the magnetic effect of the whole.

11959. As to the suppositious iron bar (11941). An iron wire having the diameter of 0·014 of an inch had 3 grains weight taken, which measured 14 inches. A piece ·014 x 30 = ·42 of an inch and that would weigh 0·090 of a grain—a piece 0·21 in length and 0·007 in diameter would weigh only 0·01125 of a grain—and a piece 0·14 in length and 0·00466 in diameter would weigh only 0·00333 of a grain. Would such a piece as the last affect the needle at ½ an inch distance? Or would a piece the length or 0·093 in length weighing only 0·00222 do so, and as much as the bar (11941) twenty feet off?

11960. *Magnetic needle in iron.* A small magnetic needle about 0·1 of an inch long suspended by cocoon silk was prepared. Also a block of iron (soft) about 2 inches square, with a hole about 1/2 inch in diameter and 1·25 deep in the middle entering in at one side. Also the long bar magnets. The long bar magnets were put edge up on the table as shewn*, being connected at the further end by soft iron; then the small magnetic needle place[d] at $^{S}_{N}$ vibrated under intense power so that its vibrations could not be distinguished by sight. Then when the block of soft iron was placed between the magnet poles, and the same needle gradually introduced into the hole, whilst out of the hole it stood as before, but the force on it was very much less than when the block was away. When *in* the hole, the force was *still less, but* the direction of the force was the same in all cases, i.e. the needle at rest stood parallel to itself whether the iron was there or not (11683, 7).



11961. By changing the magnetic poles outside the iron, the needle within the iron changed its direction. There is just one flood of power, through the iron and the air in and around it.

11962. A piece of soft iron 6 inches long and 2 inches square with a hole in the middle gave a like result but in a weaker degree.

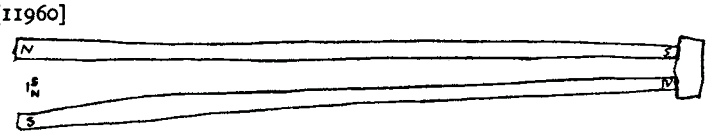
11963. If a bar of soft iron having a depression in it to receive a small needle magnet be demagnetized, and then a needle magnet be prepared which can lay in the notch, the iron filings over such a magnet away from the iron gives a very good set of curves. If the needle be put into the notch and then iron filings be sprinkled on paper over it, there are no signs of curves, or the very least, so thoroughly is the power conducted on by the soft iron. If thin paper be placed between the iron and the needle, then the filings over the needle produce weak curves; if the needle be raised a little from the iron then the curves come out more strongly.

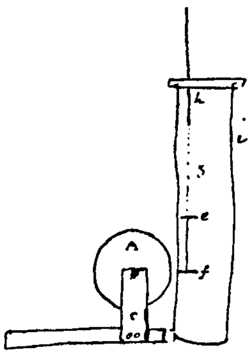
11964. Using a block of hard steel, and placing the needle upon it, the curves by filings come out more strongly than on the soft iron, i.e. hard steel is a worse conductor of the magnetic force than soft iron.

11965. Value of abundant conducting matter in terrestrial magneto electric inductions. A is a solid ball of cop[p]er 3·2 inches

[1] 1851 has been written here: evidently a slip.

* [11960]





in diameter, supported on copper axis by a wooden stand, C, so as to rotate by the hand; *e f,* an astatic needle pointing pretty well because not adjusted; it is supported by cocoon thread *g* to a wire *h* and enclosed in a jar brought close alongside of the ball. When the ball is revolved one way, the magnetic needles, which at first are parallel to its axis, then point vertically, so strong is the action. If the rotation be in the contrary direction, then the oppos. magnetic pole goes towd. the ball. The effect is very excellent.

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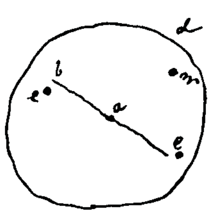


11966. Experimented on possible set of a wire when surrounded by an electrolyte and between two electrodes—*d* is a finger basin filled with water, *a b* a piece of copper wire bent thus* and about this length, suspended by cocoon silk from *a* and quite immersed in the water. Then wire electrodes from a battery of five pair of Grove's plates were put in the water in different places, as at *c* or *e,* or elsewhere. There were very little signs of motion in the wire in any case. When all was still, it kept its place whether the battery connexion was made or not.

11967. Added some Sulphuric acid so as to make the water a conductor, and now the Neg. wire produced a stream of hydrogen gas and the Pos. wire a descending stream of sul. copper and also a little gas occasionally. When the wires were close to the moving wire, as above at *e* and *c,* and on opposite sides of the end of the slung wire, there was approximation, as if attraction; when the N. wire only was near one end, the Pos. wire being away as at *m,* then there was also apparent attraction. When the P. wire was near the end and the N. wire at *m,* then there was much less signs of approximation.

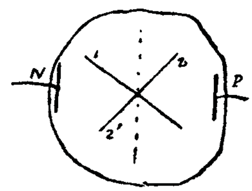
11968. I refer the apparent attraction at present to the mechanical effect of the evolved gas at the N. pole, which, causing a current to set upwards, draws in the neighbouring water and so the end of the horizontal wire with it.

11969. In order to get rid of this current, I removed the dilute Sulc. Acid and placed in the finger basin a saturated solution of Sul. copper. Now there was little or no gas evolved by the electric current—but the apparent attraction was also much less. There

* [11966]

were signs of it, but then there must have been *up* or *down* currents at the place of the electrodes from the electro chemical action, and either of these or both conspiring would tend to shew effects of approximation. When the electrodes were placed at a distance from the ends, I saw no sensible action.

11970. Then I employed two copper plates as the electrodes, arranged on opposite sides of the basin and fixed carefully, and now there was an effect with the copper wire and the solution of sulphate of copper: for if the wire were in the position either of 1 or 2, or any corresponding position, it slowly moved into an *equatorial* position. The effect was small and the power small, but the result clear.



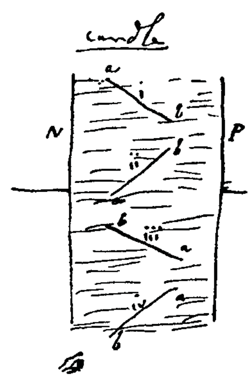
11971. As the wire was bent, this might be some action of a current of fluid formed at the wire which, in passing up or down from it, set the wire round as a vane. Still, it is remarkable that in either position it went into the equatorial position, moving to do this in *contrary* directions. Yet then the same end has contrary electrolytic actions in the contrary positions, and that may do something. To settle that, I must make the same end of the wire stand at 1 for instance and at 2′, for then any vane action which would move it from 1 to the equatorial position, going as the hands of a watch, ought to move it from 2′ into the axial position and so on, also like the hands of a watch.

11972. If the contrary happens, then there must be some peculiar setting force here. Perhaps analogous to that of diamagnetic action. Try nonconducting bodies in the Electric stream.

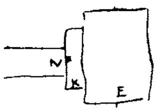
11973. There were present at these experiments to-day M. Dubois Raymond, Dr. Bence Jones and Mr. Barlow and also Mr. Anderson.

7 MAY 1852.

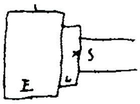
11974. Used the same apparatus, solution, plates, wire, etc. as before, also with 5 pr. of Grove's plates. To represent the positions of the wire and direction of the chief current, etc. the ends of the wire may be called *a* and *b*. So there may be four positions as in the figure. The observations were always made when looking across the liquid (i.e. along the slung wire) from below in the



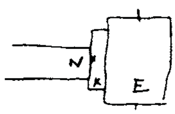
for S, L, are alike in effect by movement or by approximation. Develope clearly this relation of difference.

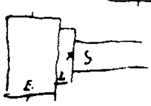


13261. Made the commutator a fixture and placed N and K thus in the anomalous position but without S or L. *Withdrawing* N and K sent the needle to the *left,* and *approaching* them to E sent it to the *right.* The experimental cylinder of iron E was turned into different positions so that any polarity it might have might shew its effect, but there was no difference produced by that change of E.



13262. Then S and L were put up to E alone, as represented. When *withdrawn,* the needle went *right;* when *approached,* it passed *left,* and that in every position of the iron cylinder E. So that E acts as if in no way polar, but as soft iron, and the actions of K and L, which are in contradictory positions, are in their effects contrary. These effects were obtained again and again.





13263. Now with movement only of E. The joint action of N and K sent needle well to the *right*—and with like movement only of E, the joint action of S and L sent the needle also to the *right.* So that still there is the contrast as to the results—movement gives like results when approximation gives opposite results.

8 SEPTR. 1814.

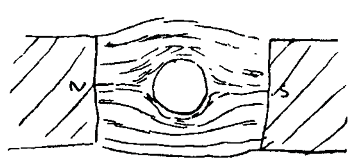
13264. *Magnetic Polarity.* Perhaps have a new paper on this point. In which case take up (Exp. Res., 2822) and all that part and reconsider and enlarge it.

13265. Compare a sphere of bismuth and one of iron when both are stationary in the magnetic field as to their supposed polarity—then rotate them and consider the currents out of them and their indications of polarity (Exp. Res., 3164, etc.).

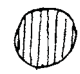
13266. Compare a sphere of soft iron, one of bismuth and one of hard steel, magnetized, when in the magnetic field and the latter reversed in polarity. Will not the hard steel on revolving give the current due to its own lines of force and be *unlike* the spheres of iron and bismuth revolving, thus shewing the difference of its polarity from them, and the *sameness* in character of their polarity?

13267*. A sphere of hard steel unmagnetized. When revolved, will it not be unable to produce currents if quite untouched by

* [13267]



the lines of force of the field around—and would not a little magnetic needle shew the deflection of the lines? The steel should not be polar. Perhaps it ought to be small or be made of discs of very hard steel plate. Is too Paramagnetic (13421, etc.).



13268. A shell of steel as a cylinder would hardly do, because the space within would convey lines of force. Would give an excellent variation of experiment.

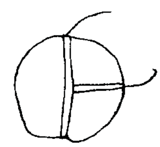
13269*. A sphere magnet in the magnetic field either accordant or discordant. If revolved, will it give its own current and that the same as if revolved with the magnets N and S away?

13270. The reverse magnet should be as bismuth—if bismuth is the reverse of iron; if it gives results contrary to bismuth, then bismuth is polar the same as iron. Examine all these bodies by revolution in the magnetic field.

13271. Polarity references, Exp. Res., 2825. See also 13242—13245, the indications which would be given by a *crystal* of bismuth in such opposed and complicated magnetic fields.

9 SEPTR. 1854.

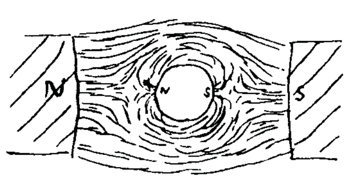
13272. Set a hard sphere of steel revolving in a magnetic field. If *no* lines of magnetic force pass through it, then no current will be produced in it (Exp. Res., 3097). Perhaps even if a copper disc or wire were introduced equatorially there might be no current, because of the quelling power of the steel over the included space, i.e. the *excluding* power of the steel. If so, then contrast the effect with a sphere of soft iron—and from that proceed to a sphere of platinum—of copper—of bismuth, etc.—and so up to hard steel—hard steel and soft iron being the extremes of the series.



13273. Probably hard steel will present that most desired *medium* or *space* of *no magnetic power*. It may claim the right of being a "nonconductor of magnetism". If so, it will present a striking contrast and relation to mere space or a vacuum—and just as striking a relation to iron as its antipodes.

13274. If hard steel a non conductor of magnetism, then a sphere of it in the magnetic field (of moderate force) ought to deflect the lines of force about it (13267), and they ought to pass round

* [13269]



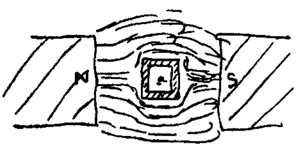
instead of through it. Then a small magnetic needle ought to shew this deflection and contrast with the effect with iron*.

13275†. Again, a plate of hard steel there, as a button, ought to send the lines of force parallel to its sides, and this should be shewn by a small weak magnetic needle and be in full contrast to the effects with iron and half cont[r]ast to those with copper or space.

13276. If a cylinder, prism or plate of steel, hard, were in the magnetic field, then filing on a plane midway ought to shew the same state of things and very well too.

13277. If all this true, we may liken the magnetic field to a *flood of power* which, when an obstruction comes in its way, as a block of hard steel, rushes round it and is conformed to it, just as happens with a flood of air or water. We may perhaps even reach to interference of its lines of force in the places analogous to the eddy places of material floods.

13278. If hard steel a non conductor of magnetism (13421) then it may keep out magnetism, and a flood of other illustrations and proofs will arise. Thus:



13279. A box of steel plates ought to shew *no magnetism* at *a*. A small needle should not point there.

13280. Filings should not arrange there.

13281. A bismuth crystal should not point there.

13282. A rectangle or ring revolved there should give no currents.

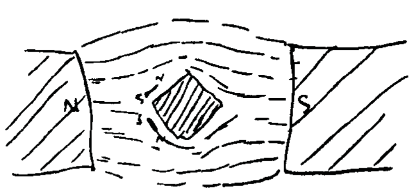
13283. Little balls of iron should not attract or repel or affect each other there.

13284. It should be as if N and S were away. Such a chamber would be free even from the earth's lines of force (13421).

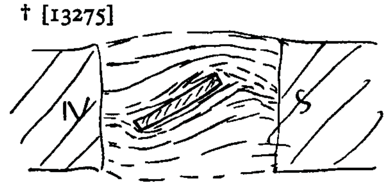
13285. Fine contrast and likeness between Steel and iron here. Needle not point in hard steel because steel excludes the power from passing through itself—not point in soft iron because the iron gathers to and conducts all the power through itself. See former error as to hard steel, Exp. R., 3292.

13286. Contrast of the iron and the steel as of a *conductor* and a *nonconductor*. It coincides with the contrast of Paramagnetic and diamagnetic bodies, and presents the extreme case of such substances.

* [13274]



† [13275]



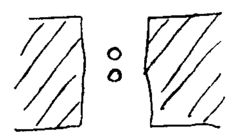
13287. Hard steel may be examined in different ways, as:
by a small magnetic needle, when in the magnetic field.
by its movement or the movement of a wire connected with it or acting about it.
by its action in media—its differential action in relation to them.

13288. Hard steel ought to be expelled out of the magnetic field both by Para and diamagnetic bodies, i.e. if it be a non conductor and so more diamagnetic than any of the rest.

13289. It would be very odd to see it sent out by sol. Sulphate of iron, solutions of nickel, etc.—these being paramagnetic bodies; and just as odd to see it sent out in water, Sulphuret of carbon and diamagnetic bodies.

13290. Hard steel and bismuth on differential balance—bismuth ought to go in as if magnetic.

13291. Hard steel and iron in field of equal force ought to attract each other equatorially, as iron and bismuth do, but far more strongly, though for the same reason (Exp. Res., 2817, 31).

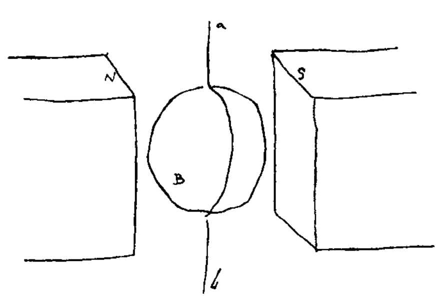


13292. Two hard steels ought to repel each other, as phosphorus does (Exp. Researches, 2814, 5, 6, etc.) and for the same reasons.

13293*. If a block of steel displaces the lines of force (13421) and not merely annihilate or otherwise dispose of them, then the moving wire thus applied will prove many things. Let N and S be two magnetic poles, B a sphere of hard steel and *a b* a covered copper wire partly in a vertical plane which would pass through N and S but partly bent to adapt it to the form of the steel sphere. Suppose the sphere in its place; the removal of the wire *a b* outwards would intersect half the lines of force passing between the magnetic poles, for those displaced by the sphere B on this side would be intersected by it. Then suppose the wire *a b* returned to its place and the *sphere B remained*—after this a like removal of the wire would not intercept the same amount of lines of force but *less,* in fact by so much less as would amount to half the lines of force now passing through the space before occupied by the steel B.

13294. A like effect should be produced if the wire *a b* were left in its place and the steel sphere B put into position or taken away.

* [13293]



Opposite currents ought to be induced in the wire *a b* by this action.

13295*. A like effect ought to be produced thus. Let N, S, be the magnetic poles and B the steel sphere, and let *a b,* instead of a mere wire, be a ring helix. Then if it be a fixture and B be approached towards it, it by throwing the stream of power outwards before it will throw the lines of force across the parts of the helix and produce probably a strong current, to be followed by a contrary one as the steel receeds. Or if the steel be stationary in the midst and the helix be brought towards it, the contrary currents will be produced.

13296†. Or have a large field of equal magnetic force and a smaller ring helix than that above, so small indeed that as it is carried parallel to the magnetic axis from Pole to pole, it shall shew little or no current, because it *intersects* few or no lines of magnetic force. Then let B be a sphere of space or a neutral body (water will do, it is so little diamagnetic) and moving *a* to or from it on either side will shew no effects, because the lines of force still go on as before, parallel to each other. Now let B be a ball of iron; then movement of *a b* will give currents, shewing that the lines of force converge upon and enter into and through it. Then make B a ball of hard steel, of course unmagnetized, and then motion of *a b* will shew that the lines of force diverge away and are driven from it because it cannot conduct the power.

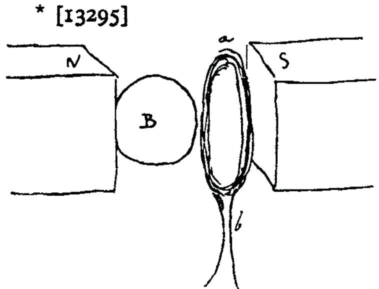
13297. Does not or will not all this shew that the lines of force are there, that they can be curved and bent about, displaced, etc. and have a real physical existence; for the power which the steel drives from one place is found in another by the moving wire, which *cannot* be said to induce it—the *whole amount* of power in the media between the poles being *always the same.*

13298. Will not these experiments prove fine illustrations of the power of the moving wire? Hard steel is paramagnetic and so no such results (13421).

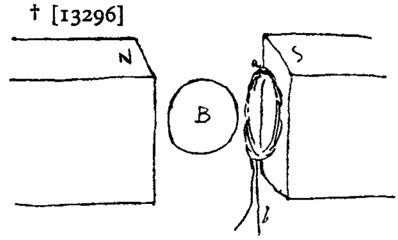
13299. Again, may not hard steel be forcibly employed to prove what I have said of the *nature* and *character* and *use* of the media surrounding a magnet (Exp. Res., 3278, etc.)?

13300. If a magnet, supersaturated, when cast upon its own resources in the free air, falls in power (Exp. Res., 3285) because

* [13295]



† [13296]



the air as a medium cannot sustain it—it would fall still lower in bismuth, it being a worse conductor—and in hard steel it ought to fall lower still, having its power crushed out of it and extinguished. So a fine needle, well magnetized, if placed between hard steel plates, ought to be greatly reduced in power; and if placed in a groove in one plate, just fitting it, and then covered by another, ought to be demagnetized. Good experiment.

13301. Numerous fine proofs of magnetical principles may be obtained by the use of well hardened perfect steel in magnetic fields not too strong in power, i.e. not so strong as to subvert the coercitive force of the steel itself (13421).

13302. Possible titles or heads to consider or determine separately:

Magnetic polarity.
Lines of magnetic force.
Magnetic media.
Magnetic conduction.

13303. Put the proofs together that hard steel is the extreme diamagnetic body (13421).

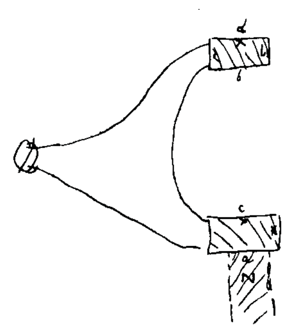
13304. Point out emphatically the value and power of the moving wire as a true natural means of investigating magnetic forces.

13305. Point out emphatically the principles of polarity.

13306. Read, take up and refer to (Exp. Researches, 3155, etc. 3277, 8, 9).

13307. *Pieces of steel.* Must look for the best steel to harden—and the best process of hardening. Must heat them in leather or cyanide of potassium, so as to have no iron or soft part on the surface—or else must remove the outer surface by cold friction on a sandstone. Must carefully avoid any decarbonized surface on the steel.

13308. A magnet reversed is in the same relation to zero as hard steel, but is as far beyond hard steel as hard steel is beyond zero. So the order would be: a hard magnet direct—bismuth or phosphorus—Space or Glass—hard steel—a hard magnet reversed. Iron is not in a fixed or nearly fixed state. As a conductor it is the reverse of hard steel (13421).



13309. Experiments with the apparatus (13223) continued. The N pole *to* either *a* or *b* sent the galvanometer needle to the *left*.

a horse shoe magnet horizontally towards it, to observe whether it would set with the fibres parallel to the lines of force. It did vibrate about a given position, but this was indifferent, for when the cylinder was held for a second in any position and then left to itself, it then set in and vibrated about that position, having taken a magnetic charge. One of the cylinders was carefully ignited, cooled and then subjected to the distant action of the horse shoe magnet (a small one), but the direction of the fibres was perfectly indifferent and there was no sign of magnecrystallic action. The cylinders required great care in their suspension that they should be perfectly vertical.

14178. As to media, cannot tell at present whether even a medium at 0° made up of water and Sul. Iron would change or not:—or even whether space would change its relation. Most interesting questions arise here.

14179. Carbonate of Iron bar (14173, 4): in the curve[1] or line laid down there is the same apparent straight line as in the crystals of bismuth (13925): has no reference to bismuth.

14180. Very cold bismuth might make a powerful temporary magnet. What would it be as a Magnecrystal?

15TH OCTR. 1855.

14181. Red ferro pruss. potassa, heated in oil, at last blackens, then breaks up suddenly with a sort of explosion—being afterwards washed by camphine and water, it gave a dark brown solution and left Prussian blue. Heated in a tube in air, it changed and broke up in like manner, evolving prussic fumes. It is the heat which changes it.

14182. The great horse shoe magnet arranged with 10 pr. Grove's plates and pole pieces with opposed flat faces 3·4 x 2 inches and 1·1 inches apart. A crystal of red ferro pruss. pot., held by copper wire and loop from a single cocoon thread, pointed well between the poles—almost as strong as tourmaline. Then being gradually heated by a spirit lamp, it pointed well Magnecrystallically, though with diminishing power; until it broke up and was dissipated it never lost its Magnecrystallic character.

14183. The *tourmaline* crystal (13691)—pointed well between

[1] See note following par. 14187.

these poles when 1·5 inches apart, i.e. powerfully. Being gradually heated by a spirit lamp, it was raised to a red heat, but never lost its magnecrystallic character, though it diminished in power. As it cooled it regained its power. When cold it was digested in Mur. acid to remove any oxide of iron that the heat and Alcohol vapour might have changed (   ).

14184. *Proto carbonate of iron*—a crystal of this substance pointed with optic axis axially when poles far apart—its powers are very great. A crystal, hung by copper wire and heated by spirit lamp, decrepitated suddenly and powerfully. By heating a crystal first at the sand bath and then by the lamp, it cracked, but held together and seemed changed, being black, etc.: but lost it; must try again.

14185. Tyndall's rhomboid of calc. spar, of which the optic axis points axially, was broken up into smaller rhomboids each of which pointed in like manner. On dissolving some of it in pure M. N. Acid, etc., I found a trace of iron in the crystal, but not much.

14186. Examined some Calcareous spar for further specimens of like Magnecrystallic character and found three pieces. All of these gave traces of iron as the former did. Two of them had faint veins here and there, of a dark green colour, like particles of chlorite or something proto ferruginous (pyrites).

14187. A piece of this ferruginous Carb. lime heated flew to pieces. Must proceed carefully and slowly.

[The results for carbonate of iron bar (14173, 4) were plotted in graphs included in the MS. at this point. They have not been reproduced.]

18 OCTR. 1855.

14188. *Pure Calc Spar* (   ). Rhomboid, suspended by *copper binding wire,* held above by a single cocoon thread—between the flat poles of the great Electro magnet (14182)—it pointed with short axis equatorially. Being gradually raised in temperature over a gas flame and then transferred to the magnetic field, it was gradually heated by a spirit lamp up to a red heat. During the whole time it remained Magnecrystallic, the short axis pointing equatorially—it never changed in character. A part of the exterior of the crystal beneath, where the heat and watery vapour acted most favourabl[y], was converted into caustic lime. Still, at the highest temperature, the crystal pointed and as at common temperatures. As it cooled it continued to possess the same state. I though[t] the directive force might be less than before.

14189. The suspending copper wire is more magnetic than it ought to be. A bundle of it at common temperatures sets axially and is attracted when cold. When hot it is indifferent, or if anything equatorial in air, etc.

14190. Platinum wire, fine, a bundle or ring coil, was not so magnetic as the cold copper when itself cold, and when heated still pointed slightly axially. Better to use it than the copper wire.

14191. A crystal of *Carb. Prot oxide of iron,* slung with copper and hung at the top of a lamp glass with a low flame, bore a temp. up to 200° or about for 1½ hours very well, but then being made a little warmer, it exploded like a Rupert's drop. So also did two other crystals.

14192. A crystal of Carbonate of Iron, put with olive oil into a tube and heated—it bore the first temperatures very well, but at a certain high temperature it suddenly broke up as before. Some pieces were left amongst the smaller powdery part—the largest of these was selected, mounted on platinum wire and suspended in the magnetic field. It proved to be as Magnecrystallic as before. It was then gradually heated by a spirit lamp, and then gradually broke away, but a part remained for some time—this was magnecrystallic, as the cold crystal in kind but not in degree, for the power became less and less—at last the wire and remaining fragment was dull red hot, but still the part which remained was magnecrystallic, though weakly—at last that portion decrepitated and disappeared from the wire. The effect was very good as far as it went.

14193. Another Rhomboid of Carb. iron was slung in platina wire, being wrapped round in two directions, and then suspended in the Magnetic field. It was strongly Magnecrystallic. It was then put for a little while over the Argand lamp—returned to the magnetic field—heated by a spirit lamp applied beneath successfully and tried from time to time by exciting the magnet; it was also Magnecrystallic and in the same manner, but grew weaker in power as the temperature rose. At a dull red heat it was magnecrystallic. It then seemed to lose Carbonic acid rapidly by the appearance and also all magnecrystallic power—it was a little magnetic at the high temperature, pointing with its length axially, but feebly; but on taking away the lamp and letting the temperature fall, it soon became exceedingly magnetic, loose pieces flew from it to the magnetic poles and it found no place of rest in the middle distance. It was in fact very magnetic oxide of iron, opake, and contained no carbonic acid.

14194. As long as the crystal remained a carbonate it was Magnecrystallic.

14195. Ferro carbonate of lime crystallized (14185, 6), i.e. such carbonate of lime as in the Magnetic field sets with the optic axis axially. Hung some pieces slung by copper wire over the gas lamp (14187). After some time, two flew to pieces, though the heat not enough to scorch paper. Put a thermometer in the place: temp. 220°. Hung up other crystals from other pieces; at 260° began to fly—at 300° the rest broke up like a Rupert's drop. With another crystal, about one half remained as a piece fractured throughout—hung it up in the Magnetic field—it then bore the heat of the spirit lamp pretty well and with the following result. When below a certain temperature it pointed with the optic axis axially, but on raising the temperature considerably, it pointed with the optic axis equatorially. On lowering the temperature, this was changed; and again when raised in temperature, the change was reversed. The crystal pointed Magnecrystallically at all times, but at the lower temperatures it pointed as a rhomboid of Carb. of iron and at the upper temperatures as a rhomboid of Carbonate of lime. When cold it was attracted as a whole, being paramagnetic.

14196. Another rhomboid of the Ferro carbonate of lime was put into a tube with oil and heated—at about 300°. Much of it burst into dust, but even after raising the oil to a decomposing temperature some fragments of the Rhomboid remained. One of these was selected and slung by the platina wire—hung in the magnetic field and tried: it set magnecrystallically, as Carb. Iron. When the spirit lamp was applied, the fragment remained after the oil was dissipated and burnt away, and now it always pointed Magnecrystallically; but when above a certain temperature it pointed as pure calc. spar, when below as carb. iron—confirming and enlarging the former result (14195).

14197. *Compressed carbonate of iron.* Tyndall gave me a rhomboid of Carbonate of iron powder pressed together in one direction—the rhomboid pointed with the short axis axially—being suspended in the magnetic field, a very short application of the spirit lamp took away this power, which did not return by cooling—and very soon parts were decomposed and the magnetic oxide of iron set free. Might make the experiment better in heated oil.

14198. The ferro carbonate of lime spoken of contains very little iron (14185) but some of its properties seem much affected, especially its cohesion. The manner in which it flies to pieces by a certain elevation of temperature, as if it were then really another body, seems to shew that there must be contending actions going on within side; and its change from the paramagnecrystallic to the diamagnecrystallic state accords with this view. Probably the optic qualities would alter in some manner and they seem deserving of examination. The substance is I think harder than the regular calcareous spar and always more fissured. As far as I have traced it, the fine veins of minute crystals of pyrites accompanies the presence or production of this kind of Calcareous spar.

5 NOVR. 1855.

14199. A, a calcareous spar from Mr Tennant. A little rhomboid of it set between flat faced poles (   ) with the optic axis equatorially.

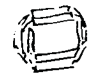
B, another calcareous spar from Mr Tennant—a rhomboid of it set with the optic axis equatorially.

C, another calcareous spar Do.—rhomboid sets with optic axis equatorially.

D, another Calcareous spar Do.—rhomboid sets with the optic axis equatorially.

E, another calcareous spar—labled from Wicklow in Ireland, very lustrous and having something of the topaz look about it—also with many conchoidal or cross fractures—a rhomboid of it sets with the optic axis equatorial, like the rest.

14200. Four crystals of Tantalite from Greenland (from Mr Tennant). All were strongly magnecrystallic and set with the length (when they had length) equatorially. Certain planes faced to the flat poles, and these were I think the planes M in the figure in Thomson's Mineralogy[1], Vol. I, p. 485; but the crystals I have do not well identify with that figure. The planes I speak of join on to other planes which surround the crystal, and the linear angles are parallel to each other and to an axis round which the crystal may be revolved—but this happens in two directions which are at right angles to each other, so that probably the planes are not difficult to recognise in other crystals.



[1] *Outlines of Mineralogy, Geology and Mineral Analysis.* By Thomas Thomson, M.D. London, 1836.

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FARADAY'S DIARY

VOL. VII

MICHAEL FARADAY

FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

made by

MICHAEL FARADAY
D.C.L., F.R.S.

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Director of the Laboratory of the
Royal Institution

VOL. VII

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This second edition of Faraday's Diary (7 vols. with integrated index) is an unabridged republication of the 1936 first edition with the index volume (v.8) integrated into the seven main volumes of the manuscript; comprises the "Experimental Notes" of Michael Faraday during the years 1820-1862; includes first edition photographs and thousands of illustrations by Faraday; published for the first time since the original printing in 1936 under exclusive arrangement with The Royal Institution of Great Britain.

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PREFACE TO VOLUME VII

IT was mentioned, in the preface to Volume I, that folio volumes I to VI of the manuscript Diary were bound by Faraday, and that folio volumes VII and VIII were made up afterwards from the loose scientific papers left at his death. Of this accumulation of papers, everything seems to have been treasured, and bound into the last two volumes of the Diary; with the result that folio volume VII contains a small amount, and folio volume VIII a great deal, of miscellaneous material having little or no relation to the contents of the earlier volumes.

It is very unlikely that Faraday regarded all these miscellaneous papers as being included in the "philosophical notes of experimental investigation" he bequeathed to the Royal Institution; and a considerable part of them is obviously unsuitable for publication, as for example, a quantity of index slips which have been pasted on to foolscap sheets. It would seem that Faraday made some progress with an index to the Diary, but did not complete it. The slips are not in order, and the words: "Slips left loose and unarranged" have been written on some of the sheets, apparently by Mrs Faraday. Notes and jottings of all kinds on scraps of paper have been preserved in much the same way, and there are some longer manuscripts; but comparatively little of this material relates to actual experimental work.

The long sequence of numbered paragraphs of the Diary continues in folio volume VII up to 16041, on March 6th, 1860, when it ends. The last sixteen or so pages of this volume consist of the index slips, and have not been printed. Of the contents of folio volume VIII, a selection has been made, on the principle that the material to be printed must be of the same general character as that in the earlier parts of the Diary, that is to say, notes in Faraday's own hand of experimental researches upon which he was personally engaged. The items thus chosen, and printed in this volume, are as follows:

(i) Experiments on telegraph wires, made in 1853–4 with the co-operation of Mr Latimer Clark (pp. 393–411).

(ii) Experiments on the electric discharge in vacuum tubes, made in 1858 in association with Mr J.P. Gassiot, F.R.S. This work was principally Gassiot's, but the extent of Faraday's collaboration was considerable. Gassiot published two papers[1] describing the experiments, but Faraday kept his own notes of them, in a separate numbered series (pars. 1–292, pp. 412—461).

(iii) Experiments in 1861–2 with Steinheil's apparatus. That on March 12th, 1862, is probably the last experiment Faraday ever made (pp. 462—5).

Of the material in folio volume VIII which has not been printed, a considerable part, occupying about two-thirds of the whole volume, is evidently the contents of a loose file of odd papers and notes. Prefixed to it is a table of contents, in Faraday's hand-writing, headed "Subjects to work and think out". The most considerable item in this section is a long "Description of a magnetic torsion balance", apparently the rough draft of a paper which was never published. The instrument described is that referred to in the Diary entry of July, 1852 (Vol. VI, p. 73, par. 12020) and used in the magnetic measurements during the autumn of that year. The other papers not printed include:

(i) Note on regelation. This is the rough draft of Faraday's last communication to the Royal Society.[2] The experimental work referred to is described in the Diary (pp. 382–390).

(ii) Note on the possible relation of gravity with electricity or heat. This is the manuscript of a short paper which, according

1 *Phil. Trans. R.S.,* 1858, pp. 1—16 and 1859, pp. 137–160.
2 *Proc. Roy. Soc.* x, 1859–60, pp. 440–450.

to Bence Jones, who gives an extract from it in his *Life and Letters of Faraday,* was sent to the Royal Society in June, 1860, and subsequently withdrawn on the advice of Professor (afterwards Sir George) Stokes, as containing only negative results. The experiments upon which it is based are those made in 1859, and recorded in the Diary (pp. 334—381). It has been thought best that the paper itself should remain unpublished.

The index to the complete Diary, volumes I to VII, will be found in a separate Index Volume.

I wish now to thank all those who have been associated with me during the past five years in the work of producing these volumes. Mr A.J.V. Gale has assisted me from the beginning. His interest and enthusiasm have lightened the labour we have shared, and I am greatly indebted to him. Mr Gale has that faculty for detecting the errors in a page of print which is at once the envy and the despair of less skilful readers, and has contributed largely to whatever qualities of accuracy and integrity the text may possess. To Mr R. Cory, who has supervised the preparation of the photostat prints from which the blocks have been made, I am most grateful. Mr W.J. Green's knowledge of Faraday's experiments, which he has allowed me to draw on from time to time, has often helped in the elucidation of doubtful passages; and other members of the Staffs of the Royal Institution and the Davy Faraday Research Laboratory have given their assistance in various ways.

The interest and advice of Mr G.H. Bickers on every point in connection with the publication have been invaluable, and my thanks are specially due to him and to the staff at Messrs G. Bell and Sons, who have borne with me with exemplary patience. Of the printing of Mr Walter Lewis and the Cambridge University Press the book, I venture to think, speaks for itself; and the demands

on the printer have been exacting, in a work in which many of the ordinary rules of composing have had to go by the board. Messrs Emery Walker Ltd. have made the excellent line blocks and collotype plates.

To Sir William Bragg and the Managers of the Royal Institution I am deeply indebted for the opportunity of carrying out a task which, if it has seemed at times a feat of endurance rather than a literary work, has been a most valued privilege.

T. M.

ROYAL INSTITUTION

September 1935

Cross references to published papers are, as before, to the collected editions of the *Experimental Researches in Electricity* and the *Experimental Researches in Chemistry and Physics;* except papers published after these volumes had appeared. References are given for these to journals in which they may be consulted.

To avoid repetition, only a single reference has been given below to Faraday's paper on the Experimental Relations of Gold (and other Metals) to Light; but it is to be understood that the paper is based on the whole of the work carried out from February to December 1856, and recorded in pars. 14243 to 15403.

FOLIO VOLUME VII OF MANUSCRIPT *(continued)*

1855

1856

CONTENTS

PLATES

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FOLIO VOLUME VII
OF MANUSCRIPT
(CONTINUED)

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14201. Static induction experiments in relation to Riess' letter—Phil. Mag., 1855, IX, p. 410, etc.

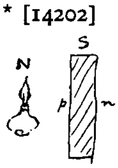
14202*. Let P represent the inductric body—S, the insulating plate whose state under induction is to be investigated an[d] N the flame or other inducteous body, which is to be uninsulated and to act as a discharger—as in Riess' experiments.

14203†. Made P the end of the E. Machine prime inductor, N a spirit lamp according to Riess, and S a plate of shell lac 4½ inches square and 0·9 of an inch thick. A thick silk thread was fa[s]tened round it and two silk thread loops proceeded from the four corners, by which it could be held in any position and yet remain insulated. The side of this plate facing towards the inductric body shall always be called the anterior side—the other, the posterior side.

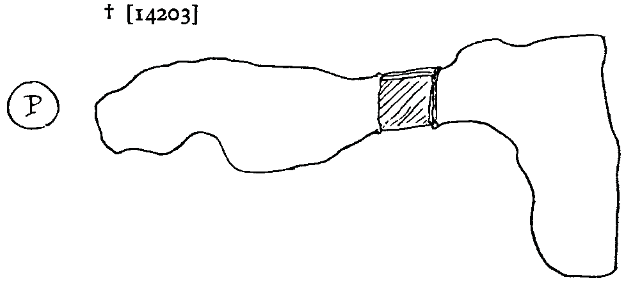
14204. The shell lac S was first discharged by the near approach of the flame N to both sides of it, P being away, and then examined by being laid on to the plate of a gold leaf electrometer—it was found to possess *no* charge and could at any time be brought into this state by a like process. Then the flame N was removed and the plate S brought into inductive position as regarded P, left there 5 or 10 seconds, taken away and again examined, and was found without charge. It was replaced in inductive position and the hand applied at N, so as to perfect the induction arrangement but so that the hand should not communicate electricity; after which the plate S was removed and examined by the electrometer, but had received no charge, though it must have suffered induction.

14205. Then the shell lac plate S was put into position in respect of P, and afterward the flame N brought up to within an inch and a half of it, and moved about for a second and removed; and then the shell lac plate, being taken away from P, was examined at the Electrometer. Whichever side was laid on the electrometer cap plate, still the shell lac was found to be charged negative, P being Positive, but the charge was much stronger in its indications when the posterior surface was against the Electrometer cap, as according to my theory it ought to be.

* [14202]



† [14203]



14206. The next point was to ascertain whether the anterior surface was charged Neg. as Riess says—or the posterior surface negative as I believed, and the result was soon obtained. For on bringing up the flame towards the posterior surface (14202, etc.) to discharge it (if charged) and then examining the shell lac at the Electrometer, it was found uncharged, and that whichever side of the plate was laid on the cap. If the anterior surface had been really charged Negative as Riess supposes, the approach of the flame to the posterior surface would have given the latter a positive charge, and these two charges would have acted on each other inductively across the plate; and then on putting the plate on the electrometer cap, the charge which might appear strong before the second action of the flame would have appeared weak afterwards; but then the approach of the hand or a metal plate uninsltd., by taking of[f] the induction of the upper charge, would allow the lower to be exerted on the electrometer and shew its full power: also by this process opposite kinds of electricity would appear on opposite sides of the shell lac plate. But the approach of the hand did not produce any effect of this kind, whichever side of the plate was downward, for the plate was wholly discharged. Hence therefore it was the posterior side of the shell lac that had been charged Negative by the flame under the action of induction, and it was the same surface which was discharged by the second approach of the flame when the induction was removed.

14207. To complete this proof, the experiment was repeated up to the second discharging action of the flame—and at that time the flame was brought up on the *anterior* side of the shell lac plate. The latter was then examined at the electrometer with its posterior surface on the cap; the charge was Negative and therefore the same in kind as before, but the effect much lower. On approaching the hand or an uninsulated conducting plate towards the top or anterior surface, the negative indication rose up almost as high as in the first instance, and on removing the hand, etc. it fell again. On turning the shell lac plate round so that the anterior surface was on the cap, all the effects were repeated, the charge indicated by the electrometer being now of positive electricity.

14208. Hence it is very clear that the first action of the flame (under induction) was to charge the posterior face of the shell lac negative:—that when the induction was removed, that posterior face remained charged and could then act by induction on things around, either through the air on one side the face or through the shell lac and air on the other side. That when the flame was approached on the air side, the induction across the air was discharged by the flame, and so the charged face itself discharged; but that when the flame was approached on the other side, i.e. the anterior side, the induction was through both air and shell lac; that the flame could discharge that across the air, but because of the immovability of the particles of the lac, not that across its surface; and so the discharge ended on the surface of the shell lac, and it acquired the positive state, i.e. the state the hand or any undischging. body would have received put in the place of the flame, and the state the flame itself has before it can exert any power of discharge.

14209. Instead of employing a flame for N, I used a fine needle point, with precisely the same results, except that the needle point is not so good a discharger by convection as flame is; the different states assumed by the shell lac plate are not so readily assumed or discharged by it as by the flame.

14210. It is evident that if in the second step of the experiment the spirit lamp as a discharge[r] of the posterior surface were dispensed with, and a conductor applied to that surface, it might be made to take all the electricity at once. This was done by laying a gold leaf on the cap of the electrometer and putting the plate of Shell lac from position S with the posterior face down upon it. The electrometer was then well diverged negatively, and by bringing an uninsulated wire up to the gold leaf or the cap in contact with it, that negative electricity was drawn off as a spark and the plate and electrometer was left entirely discharged.

14211. It is just as evident that the metallic coating may be applied to the posterior surface before the experiment begins. So a plate of tin foil was attached to this surface and then the experiment made, using the spirit lamp at N first to charge that surface under induction, then to discharge it when the induction was over; the effects were precisely of the same kind as before. But then varying the experiment by using an insulated metallic knob for N, the negative electricity going from N to the posterior surface whilst under induction, and that going back again when the induction was taken off, occurred as two visible sparks.

14212. It was but another variation of the experiment with precisely the same results in kind, when both surfaces of the shell lac were coated; that only made the extreme limits of the shell lac conductors, whilst all the intervening parts acted as insulating matter in the induction polarized condition. The consequences were the same. The plate being put into position and the flame applied at N and then removed and the plate then examined, it was found charged negative on the posterior side; a touch of the wire there took away this negative electricity and the whole system was discharged, no electricity remaining in the anterior surface. But when the flame was applied on the anterior side, that lining became positively charged, whilst the posterior lining remained negatively charged, the shell lac being in the inductive and polarized state between.

14213. When P was made a negatively electrified body, then precisely the same effects occurred, but the electric states conferred or removed had the contrary signs. Still, it was always the posterior surface which became first charged under the induction.

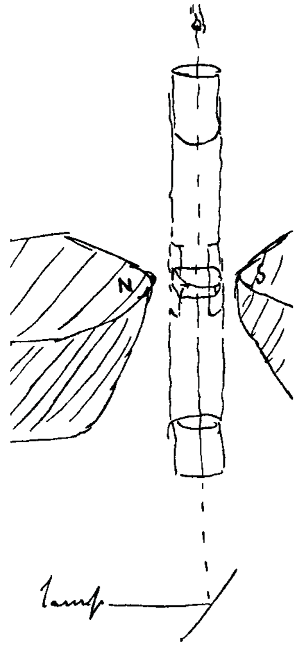
14214. Then S was changed from Shell lac to Sulphur, the plate being 5 inches square by 0·8 of an inch thick. All the experiments were repeated with precisely the same results; there was not the slightest difference in the kind of action.

14215. P may be any electrified body. A gutta percha sole excited may be used and was used in all these experiments. It may be brought within an inch of the anterior face of the shell lac or sulphur—it may even touch it (without friction)—and not charge it in the least. Still, the point or flame or knob on the posterior side, i.e. the inducteous body, will do its full duty in charging or discharging the posterior side, so that there is no difficulty in making all the experiments with ordinary care.

14216. P may be an excited rod of glass or any like charged body and is easily brought up at the moment the induction is required. So that the experiments become very easy and simple, and they are fundamental as regards the true action in and theory of induction.

17 DECR. 1855.

14217. Polarized ray of light through Magnecrystals in the magnetic field. The object was to place a magnecrystal in the strong magnetic field in different positions and then pass a polarized ray through it at right angles to the magnetic axis. For this purpose the Great horseshoe electromagnet was used, excited when needful by 20 pr. of Grove's plates. The Pole pieces N and S were employed (   ). A brass tube sustained between them carried two Nicholl's prisms, one below to polarize a reflected ray of lamp light, the one above to serve as the optical analyzer. An opening in the middle of this tube and a little stage there sufficed to permit the introduction and arrangement of the crystal to be examined between N and S.



14218. Various crystals, of *Sulphate of iron, red ferro prussiate* of potassa, etc. had the required parts rubbed down on sand paper and then by means of a piece of linen stretched over a flat plate and damped in one part, the surfaces could be finished and polished so that an excellent image of the lamp flame could be seen through the crystals as through coloured glass.

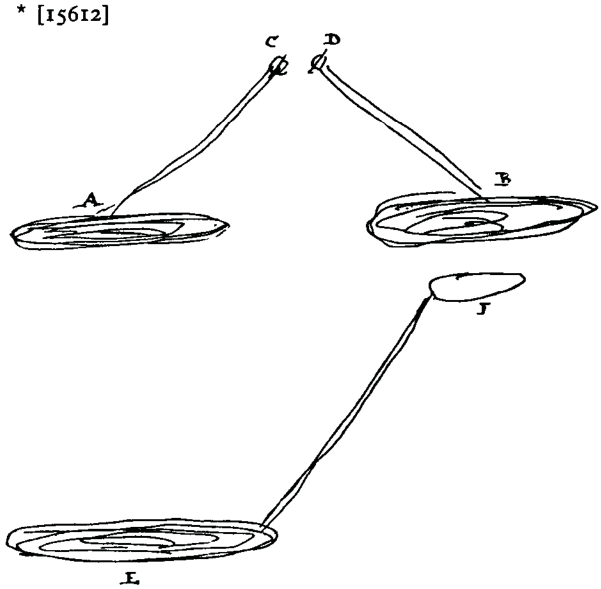
14219. *Sulphate of iron crystals* were first dealt with—some were in flat plates, with the polished surfaces parallel to the Magnecrystallic axis of the crystal. Others were formed into square prisms, of which two of the faces were parallel to the Magnecrystallic axis and other two faces perpendicular to this axis.

14220. First the Analyzing Nichol was turned so as to be at right angles with the Polarizing Nichol, i.e. until all light was extinguished; the tube with the two Nichols could then be turned altogether and of course no ray passed during the whole time. Then a crystal could be placed on the stage between the Magnetic poles, and this depolarizing the ray from the lower Nichol, suffered light to pass through the upper. By turning the experimental crystal round on a vertical axis, it could be brought into one of two diametral positions in which it did not affect the polarized light and then all remained dark, and by turning the whole system this arrangement could be preserved and yet the partition of the lower apparatus room is 35 feet, and from that to the North end of the distilled water cellar is 11½ feet, making altogether about 112 feet in length. The glass house in the yard is 8 feet from N. to S. between the shelves and the heating furnace and 10 feet from E. to W. between the wall and the door—7 feet from the door reaches to about the middle of the open yard.

15611. Have ordered an additional lime light from Mr Becker. Have also ordered an eye reflector axis from Mr Newman. Have also ordered three plano cylindrical lenses from _____ (New road), an inch square each and silvered on the convex sides, to serve as concave eye reflectors.

15612. In respect of the arrangement of the two inducing systems that have to be compared, probably the two inducteous helices ought to be alike in length, character, etc., that retardation from length of wire, induction amongst the spirals, etc. may be proportionately alike in both. But the inductrics acting on the two must be of different strengths so as to produce something like equal currents in the two. Still, the inductrics must both be excited by *one* current, that the identity of time at the beginning of the induction may be CERTAIN. Theoretically this arrangement* would do. A, B, two equal inducteous coils with their galvanometers C, D. Then E, a large inductric coil and F, a small one, connected together in one circuit with the battery, F being of such power as to produce an induced current in B of the force corresponding to that in A. In order to render the systems AE and BF independant of each other, they may be arranged in planes vertical to each other; thus A and E may stand in vertical

* [15612]

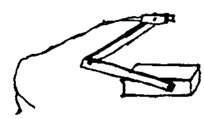


planes at each end of 100 feet, inducing horizontally*, and BF in horizontal planes, or in vertical planes at right angles to the former, adjusting the positions by trials so that the inductric E should have no action on B, and the inductric F no action on A; also that the inductrics E and F should have no direct action on the Galvanometers. Perhaps that may require the common axis of E and A to be horizontal and also magnetic north and south; in which case the joint axis of F and B could hardly be horizontal but might be made vertical, and so as to render the needles independant of F.

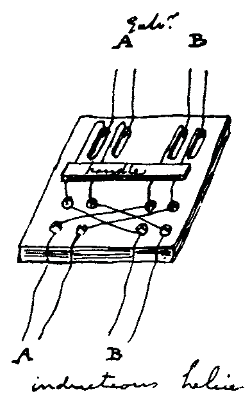
19 AUG. 1857.

15613. *Observing Telescope.* Gives good image of the needle reflector at 8 feet distance. Shews it long, horizontal ▭ and full of light when a candle is 6 feet distance. As the reflected light always appears in one place, it is evident the eye reflector may revolve either in the vertical or horizontal or any other plane when looking for the disappearance of the light. Only when the two reflected lights are to be observed at once, the axis of revolution must be parallel to the line which *apparently* joins the two lights (15647).

15614. Have prepared *two platina wire lights,* the platinum wires being 1/100 of inch in diameter an[d] an inch long each. Have blackened all the wood surface within reach of the light from the wire so as to prevent extraneous light passing to the eye reflector.



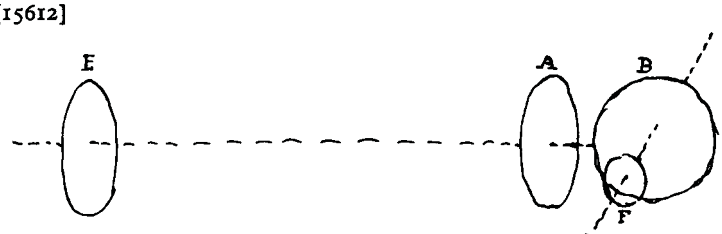
15615. *Commutator* for inducteous helices and the two galvanometers. A board with 12 mercury cups as figured, the long channels being connected unchangeably with the galvanometers and the small cups also unchangeably, as represented, with the inducteous helices. The only moveable connexions are the four wires fixed to the cross bar handle. As these are brought forward or put backward they connect the galvanometers either with helices A and B or with them as B and A.



31 AUGUST 1857.

15616. *Regulator* for Electric current of Platinum lights. A cylinder of wood 2 inches in diameter and about _____ inches in length. A copper wire 0·035 of an inch in diameter is wound round it from end to end in a spiral of 63 convolutions, each of which

* [15612]



contains 6·35 inches, so that the whole length is 400 inches. There are screws for communication in the circuit at each end and one end has a sliding spring which, passing over the coils, presses on any one of them on which it rests and so can bring in successive lengths of the wire, and so increase the resistance sufficiently at pleasure.

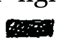
2 SEPTR. 1857.

15617. Have made a new *flat helix,* the *third* one, of wire 0·1 of an inch in diameter. *Call it* L. It contains 50 convolutions; the outer contains 19 f. 6 inches and the inner 6 f. 6 inches—the medium is 13 feet, which multiplied by 50 gives 650 feet of copper wire of 0·1 diameter. Convolutions 1/2 an inch apart.

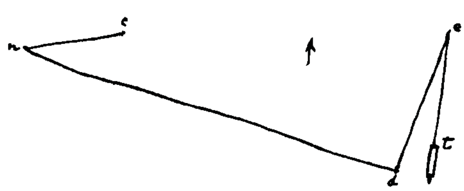
4 SEPTR. 1857.

15618. The two galvanometers must stand in the same horizontal plane to have their connexions clear and bear approaching. Then by bringing one a little forwarder than the other, the needles may be brought to within 2 inches of each other apparently if necessary, and then if needful to shelter them, one shade would cover both. If a shade be used it must probably have a flat glass face to avoid reflexions.

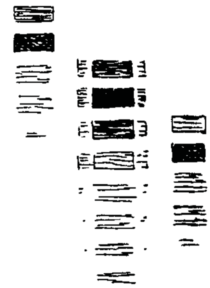
15619*. Associated a candle flame at *c* with the needle reflector at *n* (15605) and 20½ inches about from it. The directing reflector *d* (15627), which is silvered glass, was placed 7 feet from the Galvr. needle *n,* and threw the ray on to *e,* being the eye-reflector (15598) of badly silvered glass and 32 inches from *d;* and the eye reflector returned the ray to the telescope *t* 25 inches from it (15613). All these reflections were in the horizontal plane or nearly so; the adjustment of the parts was easy enough.

15620. The telescope (15613) was focussed to give a good image of the Galvanometer needle *n,* for this gave the most compact and brightest object as light—the needle reflector then seemed full of light and was bright enough; it as an object having this form , but the chief light was accompanied by many reflected lights. It had one above—then it had a series below, the nearest being as bright as the one above and the other[s] gradually diminishing in brightness. Also on the right and left of these were other lights

* [15619]



as figured, and which as regarding their place and degree of brilliancy, etc. deserve analyzing. Besides these on the right and left were faint repetitions of the chief column of lights, but those on the left raised and those on the right depressed—those on the left being the brightest. All these are no doubt the effect of the double surfaces of the reflectors *e* and *d*. They make the whole light as seen in the telescope a very diffuse object instead of an intense concentrated one.



15621. Using the opera glass (15647) in place of the telescope, the whole image was much smaller and more compact; it looked more like a spark and was of good brightness but had a burr form and appearance. On close examination the disposition of the parts of the light was seen to be as before. Must use reflectors with single faces and then probably the telescope will be better than the opera glass.

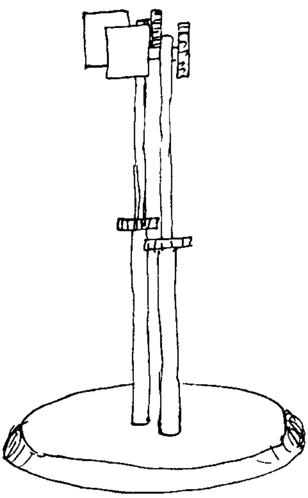
15622. Displaced the eye reflector at *e* and placed the telescope there, so as to look directly into *d,* thus removing the effect of the former and taking that of the latter only. Now the right and left reflexions disappeared and also those above and below the chief light, the image being bright and reduced to this form  ; so we see what the double surface does (15634).

15623. I restored the eye reflector *e* (15622) but brought into action the plain polished silver surface (15598), so as to have a single surface there, but the form of the surface was so bad that the image could not be concentrated and was quite useless. Still, I think the extra reflexions of *e* were all gone. Put a steel button at *e,* but that gave as bad a result as the silver (15598).

15624. Restored the silvered glass of the eye reflector to its position at *e* (15619). Displaced the candle flame at *c* (15619) and placed a platinum wire ignited by a voltaic battery as the light there. When the effect was examined by the telescope at *t* (15619, 20), the object was found to have the same *form* and *size* and *reflexions* as the candle flame, but was not so bright. It could be made brighter by more current and was then better in that respect—but the thinness of the source in a horizontal direction did not give a better image in the telescope, with the present reflectors and needle. Theoretically however it ought to disappear quicker (15529).

15625. The current regulator (15616) performed its duty very well. Its wire, so much as was in the circuit, became hot, though the platinum wire not very bright. Lessened the resistance, then the heat of the platinum rose much but quickly fuzed, and this would be very liable with platinum lamps.

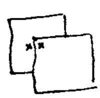
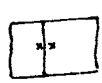
15626. I think I must use oil lamps at first and afterwards lime light if I have to give great velocity to the eye reflector.

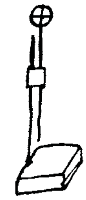


15627. *Directing reflectors.* The two rays from the two galvanometer needles have to be so directed as to place the two images close together as one object, so that further observation may shew which is displaced first—it is easy by adjustment of the two lights to make the reflected rays from the galvanometer needles converge to one point, but after that they will separate again. I have had a pair of directing reflectors constructed, to receive the rays at this point, and reflecting them on in lines so nearly parallel that the two images shall appear close together when viewed by the eye or a telescope from any given point. A small plane silvered glass mirror is supported on a horizontal arm at the top of a little vertical column, and by a thumb screw can be turned on a horizontal axis; the vertical column is supported in a lower sliding vertical tube in which it can also turn on a vertical axis; the whole is set on a stand, and within an inch of it is a second like mirror supported exactly in the same manner. Now it is easy to place the stand so that the mirrors shall stand side by side or one overlap the other entirely or in any degree, as for instance thus*: and it is just as easy, when these directing reflectors are placed at the spot where the rays reflected from the galvanometer needles meet, to reflect these two rays in any direction by adjustment of the reflectors, and therefore to make them appear to the eye reflector or in the telescope as close together, giving the two lights as one object. After that, the point is to see which of these two lights disappears first. I have had such a pair of reflectors made and find I can place the two images of two separate candle[s] as close together as I like.

12 SEPTR. 1857.

15628. *Guide rings* to determine the place of the ray. A ring 1½ inch in diameter fixed on top of a straight wire passing through

* [15627]  or thus 



a cork—another wire fixed to a leaden foot passes through the same cork—by this means the ring is adjustible; cross wires are fixed to it and when a reflected ray is traced, the ring is set so as to encompass it and of course the direction of the ray is recorded for the time.

15629. *Light.* Microscope lamps, two. The flame of one of these lamps is 5/8 of an inch diameter. Being placed 20 inches about from the galvr. needle reflector and the latter looked at from a distance of 6 feet, the effect was good, i.e. the needle was very bright; but the diffusion of the light to the right and left of the axis of the reflected beam was very great because of the size of the flame and the angle it subtended at the reflector. When the lamp was removed 6 feet from the needle, then the diffusion laterally was very much less—not a fourth of the former; but the light was weaker.

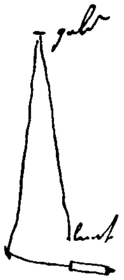
15630. The lamp flame 6 feet from the Galvr. needle and the telescope 7 or 8 feet from it, adjusted to give image of the reflector needle, gave a pretty good image, but not quite good, for needle reflector is not correct in its action. When a screen with a slit 2½/8 of an inch wide was placed before the lamp, the light in the telescope was apparently as good as with the whole flame open. When the slit was 1/8 of an inch wide the light in the telescope was much less, and with a slit of 1/16 of an inch wide the light was still less—the diminution now being very considerable. Must place the lamp in the focus of 20½ inches and then compare the effect of different sized slits. They ought not to *impare* the *maximum* brightness, but ou[gh]t to diminish the lateral diffusion.

15631. I focussed the telescope for objects 12 feet distance—as that is the distance of the light—the light was poor, diffuse and bad, and I had to reduce the telescope to the focus distance of the needle reflector to make it good again. This ought to be so, as the needle reflector is not a plane mirror but concave.

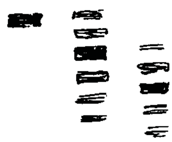
15632. I think the proper light will be a strong one, as the lamp or a lime light, at 20½ inches and with a fine slit screen before it.

15633. As to the available angle at the needle reflector, I found I could easily make that of the incident and reflected ray 90° or even 120° and have very good light.

15634. Examined the image of the illuminated galvanometer



needle given by different reflectors employed as eye reflector and observed by the telescope adjusted so as to give most distinct image of the galvr. needle, the lamp being always 6 feet from the galvr. and the latter 6 feet from the reflector examined. The two directing reflectors (15627), of glass silvered, both gave this form of light  (15622). This was due to the inclination of the reflectors to the incident ray, for when one reflector was turned 90°, being retained in the same plane, the light still had the same position (not in relation to the turned reflector but) in relation to the plane of the incident and reflected rays. It is the double image of the two surfaced glass*. As the incident ray fall[s] more nearly perpendicularly on the reflector, the image becomes better and the two outlying portions gather up. If the glass surfaces were parallel and the incident ray perpendicular to them, then there would be no outlying portions and only a simple image. So the more direct the reflexion the better.



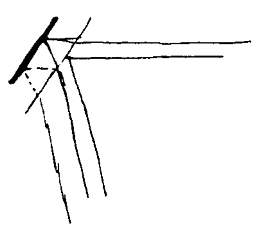
15635. The former glass of the eye reflector (15620) gave a very complicated image but without the lateral wings to the center portion. The silver surface (15623) gave as before a very bad diffused image. The steel surface (15623) gave as before a bad, diffused image—but these were single images. Other steel surfaces (buttons) all with bad results (15598).

15636. Tried the four silvered glasses from the New Road workman (   ). The one professing to be flat gave three images far apart, and the chief or middle one very much confused by reduplication. The three concaves are of no use; first because of their bad surfaces and distorted action and image; next because, if good, they could not be used with the telescope but with the naked eye only. In fact a reflector there, if concave and perfect, would form its own instrument to an eye in the focus and would require no observing instrument, as the telescope—only a tube to guide and keep the eye.

15637. These reflectors, being parts of spheres, would confound two lights seen at once in them. If ever usable, the reflector here must be part of a cylinder. But probably they will not be wanted.

15638. So the reflectors, if of glass silvered, must be true and good in surface and the ray incident perpendicularly or nearly so. But

* [15634]



may probably obtain pieces of flat speculum metal—in perfect condition as to surface—that will serve far better. Try.

15639. Have cleared out the distilled water cellar, which has a stone floor: placed a table on it and the Galvr. A (15605) on the table:—then by a lamp and reflected ray observed the steadiness of the Galvr. needle and of the ray reflected from it, and found it perfect. Will do perfectly well in that respect.

15640*. I think the arrangement had better be made keeping the reflected ray in the same horizontal plane until it comes to the eye reflector—which revolving on a horizontal axis, will cause the images to move apparently in lines nearly vertical; and that the reflexions had better be as nearly vertical to the reflecting planes as may be.

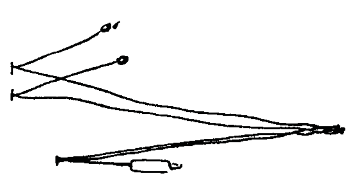
15 SEPTR. 1857.

15641. Microscope lamp for light (15629). Copper chimneys pierced with slits are not good, as the apertures disturb the flame. Must have copper screens with slits outside of the glass chimneys.

15642. The whole lamp flame of 5/8 of inch (15629) at 20½ inches (focal length) from the needle reflector and the telescope 56 inches off. Great diffusion of the light; it appears on all sides of the telescope. The image of the reflector in the telescope is good and full of light, but when the needle is set vibrating in a manner strongly evident to an eye at the edge of the diffused ray, the appearance in the telescope or in the axis of the ray is as if the needle were immovable.

15643. The lamp only 12 inches from the needle reflector—gave a strong light but the ray still more diffuse and worse than before, though of the same kind (15642). Lamp at 24 inches distance—the effect better than in former case, but still when motion was seen at the edge of the diffused ray, it was not visible by any variation in the telescope unless the vibration of the needle were considerable. Lamp at 36 inches distance: as before, but in a less degree—the telescope would give an apparently fixed needle when it was really moving. When the lamp was 56 inches off, a portion of the same effect remained. As regards the perception of needle motion, the effect was better to the naked eye than with the telescope—the changes of the light in the moving reflector were more

* [15640]



distinctly seen. This diffusion is I believe chiefly due to the great size of the lamp flame. It may be partly to the imperfect polish and face of the needle reflector.

15644. The motion of the needle is no doubt vastly better seen by the reflected light than by looking simply at the needle as a whole: the motion was very manifest in the former way when the eye could not see it in the latter way.

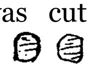
17 SEPTR. 1857.

15645. Have ordered some *metallic reflectors* from Mr Varley (15682). Having written to Mr De la Rue, have received 7 or 8 specimens of plane polished reflecting metals from him: lumps about the size of nuts or wallnuts; but they may be cut.

15646. As edges of ground reflectors (needles or other) must be more or less rounded in the grinding, they must of course reflect light when the mirror is a little out of position. The remedy will be to break them off or else to stop edges by blackening them.

15647. The *telescope* (15613) magnifies about 11 times linear. The *opera glass* (15621) magnifies about three times linear.

15648. The lamps have been supplied with *copper screens,* having slits 1/8, 3/16, and 2/8 of inch wide. They are blackened and allow no light to pass except through the particular slit pointed towards the Galvanometer.

15649. Lamp (15629, 48) with 2/8 aperture, 20½ (or focal distance) from the Galvr. needle: the telescope 54½ inches between its object end and the galvr. needle: so the whole length of ray 75 inches. Arranged the telescope length for this distance: the needle image was of course obscure: but the lamp image was also confused in appearance  . Shortening the telescope (as if for more distant objects) made this light larger and more indefinite at the edges: it was the confused image of the flame, and as the needle moved, it was cut off by its edge on the opposite sides alternately thus  .

15650. Lengthened the telescope gradually so as to fit it for distinct view of objects more and more near. The light less and less and at the same time more and more bright until it was thus  , being the distinct view of the needle. This was a good object and the best of those obtained: but as the image of the mirror was far smaller than the image of the flame which would have appeared in a larger mirror, so the eye in the axis of the beam saw the light steady or fixed, whereas if applied at the side of the beam the reflector was seen to be in motion. There is no doubt that the needle is the object to be looked at distinctly, but the light ought not to subtend a large angle at it.

15651. Keeping all other things the same, the lamp screen was turned until the slit of 1/8 was opposite the needle; this would subtend only half the angle of the former. The light reflected was very bright and good both to the naked eye and in the telescope. Moving either to the right or left, the reflected beam was found to be less diffused than the former; still, not much less; also it is not so bright—but have to remember that the surface of the steel reflector is not perfect and that probably much of the diffusion results from that.

15652. A ray may seem fixed in the telescope and opera glass which, when viewed by the naked eye, is seen to move. But in these cases both the telescope and opera glass shew that it is moving when they are moved to one side into the edge of the diffused ray.

15653. The small slit gives on the wall an illuminated surface of this character, being the camera image of the flame, in which the brighter parts *a, a,* result from the foreshortened projections of the sides of the flame. It is well to turn the shade on the lamp so that the brightest part falls upon the reflector.

15654. The lamp being at 20½ inches and the 2/8 slit towards the needle, a card screen 5/8 wide across the middle of the object glass (which is 9/8 clear) shut off nearly all the regularly reflected light, so that little more than half the width of the object glass transmits the ray of light. When the opera glass was used, a vertical screen of 6/8 was almost enough to obliterate the image of the light seen through any part of the glass when adjusted to the place of the visible object. The opera glass is so large that it is easy to look aside of the screen, but then adjusting the screen, the effect recurs.

15655. Now moved the lamp to considerable distance, i.e. 54 inches, with 2/8 slit—telescope as before, 54½ inches—very good illumination. Reduced the light slit to 1/8, using the brightest light (15653); still there was a very good light in the axis of the beam, but there was much less diffusion than before, so that the motion of the needle tells quickly in the telescope. The image of the needle seems fixed but the image of the reflected light travels across it.

15656. The opera glass gives with this arrangement an excellent little luminous image of the mirror, which by motion of the opera glass describes good curved lines of light.

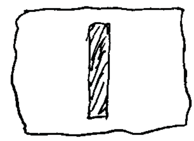
15657. As to a *flat needle reflector*. Took one of De la Rue's flat reflectors (15645)—blackened it over—cleared off four places of about the size marked, by a point of wood—fixed it before the galvanometer needle as a stationary object of four differently sized flat reflectors—placed the lamp with 1/8 slit aperture at 8 inches from it and the telescope at the distance of 54 inches as before. To the naked eye the reflexion was very bright:—the light in the different reflecting parts built up one image of the flame and slit, the eye adjusting itself to the flame distance. There was much diffusion of the reflected ray and of course more in a vertical than in a horizontal direction.

15658. The *telescope* was adjusted to the reflector distance—all the four light images were good but the largest of course the most prominent and effectual. Shortened the telescope so as to adjust it to the lamp distance: this was a disadvantage, for the image of each reflecting place was enlarged and became blurred and the light became diffuse with it. Using the *opera glass* adjusted to the reflecting surfaces' distance, the images were very good—small, compressed but very bright—the largest not too large, quite starlike to the eye. When the glass was moved, the lines described were very fine and good: the largest spot perhaps the best line but the smallest was good. I could not make the lines invisible by the quickest motion the hands could give. The effects were very good.

15659. Now the lamp with 2/8 aperture the full distance of 54 inches. To the *eye,* the effect in the axis of the beam was good, but the diffusion, especially in the horizontal direction, far less than before—the effect star like. In the *telescope* adjusted to the reflector, the effect was very good. The largest reflecting surface makes most impression on the eye. On shortening the telescope, the images became worse, ran together and at last obtained the image of the slit and flame, losing that of the reflectors: but all these effects were worse than those with adjustment to the reflectors. The distance thus *experimentally* obtained was found to be right for an object 108 inches off. Now the distance from the lamp to the reflector and then to the telescope was 104 inches, so the actions and results coincide. But it is best to adjust to the reflector, which is the thing whose motions are to be observed. The *opera glass* gave an excellent star like effect—with very little diffusion. When it was moved, the light lines were very good.

15660. Employed the 1/8 lamp slit—to the eye and to the opera glass the effect was very star like and good.

15661. Now employed a large De la Rue reflector (15645), the angle subtended by it being much larger than that of the flame of the 1/8 slit—the lamp was at the full distance of 54 inches. To the eye the whole flame is in the reflector and of course travels across it as the eye moves right or left. *Telescope* up, adjusted to the reflector, gave it clear and a round confused light in it. Adjusted to the lamp light, it gave it smaller and more brilliant and distinct, and the reflector obscure. This is perhaps the best when the reflector includes the whole of the light, but the worst when the reflector subtends much smaller angle than the light. When the *opera glass* was used, the adjustment to the light was also the best. Whatever becomes the luminous object, either the whole light or the whole reflector, should be made distinct in the instrument.

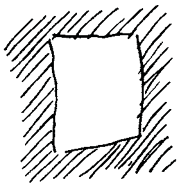


15662. Have a concave silvered glass reflector, radius about 27 inches, i.e. when a candle flame and its image are equidistant, 27 inches is their distance from the reflector. Of course a lamp being in the distance and the mirror moved, the image traverses in one direction if the eye is near the mirror and in the other if the eye be outside the secondary focus. The image of the flame is largest when the eye is in the secondary focus and diminishes as the eye advances or receeds from that position, but at any distance within those I should use, it is *always* larger than the image would be in a plane mirror. When the eye is close up to the concave mirror, the nearest approach to equality is obtained. Such is the case for all distances of the flame or object.

15663. Theoretically, a concave reflector with a small light in one would supply such a spark. The jar might be retained charged to the proper degree by the use of a feeble discharge train acting constantly to the earth, whilst the machine was constantly acting. The lever on the mirror axis might either *depress* the earth connexion E, or might *fall away* from it, E being then a spring.

23 AUGUST 1858.

15772. Worked at the arrangement of Galvanometers, etc. Have cut holes in the sides of the shades, so that the entering and reflected ray may not be deranged and the image deformed by the action of the glass. The sheltering of the needle by the shade seems to be perfect notwithstanding these holes, which are small.



15773. Used a fixed star light at S (15768), it being the image of a small argand lamp reflected from a silvered sphere about 1¼ inch in diameter. The reflected image seen by the eye at I is small and feeble, and I doubt whether the Galvr. needles are good plane reflectors.

15774. The least displacement of steel spectacles, or motion of scizzars or other steel articles in the place, shewed a disturbance of the Galvanometer needle as proved by the disturbed reflected ray.

15775. I think I must be content to work with a continuous light in the first instance, and see what I can obtain with that, and afterwds. proceed to the use of a momentary light. For the fixed light, each Galvanometer may have its own light object and that had better in the first instance be near. Probably an Argand lamp with a pierced opaque chimney will do. One galvanometer image may disappear before the other if the *time* be in any case sensible.

24 AUG. 1858.

15776. Resumed the Expts. began ten years ago (9970, 9971), 24 Novr. 1848, on possible deposition of carbon slowly. The four bottles were examined and apparently had undergone no change—they were not darker in colour—no gas had been generated—no sulphurous acid—the odour was etherial as at first—the addition of a little water evolved heat—the acid was a mixture of sulphuric and sulphovinic—and the diamonds in each of the two bottles amounted to 0·7 of a grain in weight and were unchanged in character.

30 AUG. 1858.

15777. Have been working with a constant light, and the following arrangement of parts*.

15778. A lamp or candle placed at *a* sent a ray to G′, then to 1, then to D and then to E, which was very easily adjusted for E and seen there as a bright star, that being the image of the illuminated needle of G′.

15779. When the candle or lamp was placed far back, as at S, then the ray required much more care to direct it to E, the adjustment from D to E being very nice—the image was of course feebler, and a much smaller motion of the Galvanometer needle or of the eye at E threw it out of sight.

15780. It is well at first to keep the light at S separate from the contact breaker and eye reflector; it allows of a little adjustment of the one or the other separately.

15781. It is well to raise the light at S, so that the ray going towards G should pass *over* the head and hands in the adjustment.

15782. A lime light might be used there, if desirable to increase the distance for delicacy's sake—or to use a smaller intenser light.

15783. Using a small argand lamp at S, I could get the two reflected lights from D into the eye mirror E as one object.

15784. It will be well to fix the reflectors 1, 2 and the director D together on one board or basis—allowing motion to adjust and the action of screws to clamp. That is being done.

* [15777]

15785. Surely the force of gravitation and its probable relation to other forms of force may be attacked by experiment. Let us try to think of some possibilities.

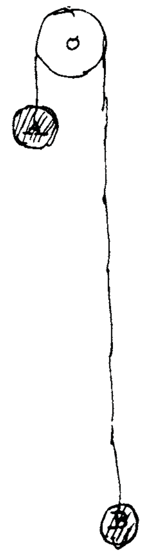
15786. Suppose a relation to exist between gravitation and electricity, and that as gravitation diminishes or increases by variation of distance, electricity either position or negative were to appear—is not likely, nevertheless try, for less likely things apparently have happened in nature.

15787. There is more chance of any observable effect in a body acted on by the earth than in the same body acted on by a like body. There is more chance of a variation being observed in a ton of water or lead when lifted a hundred yards upwards from the earth, than in the same ton when removed a hundred yards in a horizontal direction from the side of another ton by which it at first stood.

15788. Must not be deterred by the old experiments (10018, etc.). If there be any true effect of gravity, it may take much gravitating matter to make the effect sensible, and I had but very little. Moreover, the motion of a body with or against gravity ought not to form a current in a closed circuit, as tried in the former case, but perhaps give opposite states in lifted or depressed bodies, and though a current might be formed in a wire connecting two such, it would not be a current in a circuit. So may consider the imaginable effects under two views, *static* or *dynamic*. Take the former first and imagine as follows:

15789. If an insulated body, being lifted from the earth, does evolve electricity in proportion to its loss of gravitating force—then it may become charged to a very minute degree either *positive* or negative. When thus charged it may be discharged, and then if allowed to descend insulated, it would become charged in the opposite manner, and so on. If three or more bodies of the same size, but in weights as 1, 2 and 3, then the intensity of the charge ought to be as the densities.

15790. Might not two globes (or masses, as pigs) of lead be attached to the end of a long rope passing over a large pulley at



the top of the Clock tower, or in the whispering gallery of St. Paul's serve an experimental purpose. Starting with both balls insulated, discharged and balanced, then it would be easy to raise B and lower A, and examination by a very delicate static electrometer might shew A charged pos. and B negative; then discharging both and reversing the motion, B would come down Positive and A become negative and so on. The static electrometer might be applied either above or below or at both places. If the effect were real but insensible, several journeys up and down might be effected—the discharge above being made by a bell wire and touching lever. Or the discharge above and below might be made *automatically* to two electroscopes, one above and one below, so as to accumulate many results into one—these electrometers being very delicate and of the condensing kind. One man, having only to turn a windlass, might work the apparatus for half a day—or it might be kept in motion by a small engine or other mechanical power.

15791*. Perhaps a much less height would do—and then perhaps a balanced lever would do, carrying the insulated balls A and B. Place could easily be found for such, 15 or 20 feet high and about 40 feet long. A and B would then be easily manageable and weights made heavy or varied at pleasure.

15792. As to the character of the gravitating matter, it may be conducting or nonconducting—and probably an important difference might here arise. Non-conducting matter invested in conducting matter would be peculiar in its discharging action, etc.

15793. Iron—lead—platinum—copper pigs—Tin blocks—Water in vessels—Stone—Marble—Crystal.

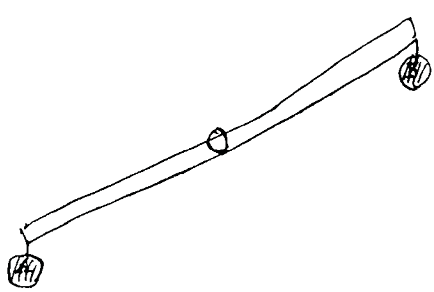
15794. It would be easy I think to distinguish between an atmospheric and a gravitation effect.

15795. The evolution of *one* electricity would be a new and very remarkable thing. The idea throws a doubt on the whole, but still try, for who knows what is possible in dealing with gravity?

15796. The first thought _____ would give a new relation, a relation of a dual power to a single power, which would probably give a modification to the character of singleness supposed to belong to gravitation—for it would then be as dual as electricity.

15797. If the insulated masses assumed the state supposed, they

* [15791]



would when connected give a current between them and thus dynamic effects; but it is possible that if connected whilst they were changing their position, more distinct dynamic effects might be produced. A bar of metal turned end for end upwards might have a current flowing along it and especially at the middle. If in two pieces and these connected by a fine galvanometer, might have results.

15798. Perhaps a thick wire galvanometer might be better than a thin wire instrument. Insulation should be good. The oscillating bar (   ) might answer. Might construct a revolving system, connecting the ascending and descending parts by a commutator with an Electroscope or a galvanometer.

15799. Perhaps a jet of drops of water from a height would tell below—only water is a bad substance because of the discharging facility of moist air.

15800. Probably a jet of lead would do better; the fall of shot in the shot tower. Might insulate the tub of water into which it falls below and so get traces of any evolution.

15801. One of their pigs of lead, insulated and discharged below, then raised to the top and examined by the Electroscope.

15802. Two pigs of lead at opposite ends of a cord as before (15790).

15803. Might make some trial experiments at home from the Lecture room window to the floor, or even from the top of the house to the bottom of the yard. Shot might be sent from above in a stream from a metallic uninsulated vessel, a fine hopper—into an insulated vessel below. Or as lead may be liable to oxidizing action, could also use Mercury. Probably 100 lb. or 150 lb. would shew some effect and could then compare the state of one with the state of the other.

15804. Let us encourage ourselves by a little more imagination prior to experiment. Atmospheric phenomena favour the idea of the convertibility of gravitating force into Electricity, and back again probably (or perhaps then into heat). Matter is continually falling and rising in the air. The difference and the change of place of the bodies subject to Gravity would perhaps give a predominant electric state above, as the Negative; but also an occasional *charge* of the other state, the Positive. If there be this supposed relation of gravity and Electricity, and the above space be chiefly or generally Negative—then we might expect that, as matter rises from the earth or moves against Gravity, it becomes Negative.

15805. Then we might expect a wonderful opening out of the electrical phenomena.

15806. So to say, even the changed force of Gravity as Electricity might travel above the earth's surface, changing its place and then becoming the equivalent of Gravity.

15807. Perhaps heat is the related condition of the force when change in Gravity occurs. Atmospheric phenomena are not at first sight opposed to this view. Might associate a thermo electric pile or couple, to see if change of elevation from the earth causes any sensible change of temperature.

15808. Perhaps almost all the varying phenomena of atmospheric heat, electricity, etc. may be referrible to effects of gravitation—and in that respect the latter may prove to be one of the most changeable powers instead of one of the most unchanged.

15809. Let the imagination go, guiding it by judgment and principle, but holding it in and directing it by *experiment*.

15810. If any effect, either electric or calorific—then consider the infinity of action in nature—a *planet* or a *comet* when nearer to or farther from the Sun. Dr. Winslow's observations on earthquakes—a Falling river or cascade—the falls of Niagara. Evaporation—vapour rising—rain falling—hail. Negative state of the upper regions. Condition of the inner and deeper parts of the earth—their heat. A falling stone or ærolite heated. A Volcano and the Volcanic lightning. Smoke in a chimney perhaps goes out electrified.

15811. What a multitude of events and changes in the atmosphere would be elucidated by such actions. I think we have been dull or blind not to have suspected some such results.

15812. If, etc.—it may be possible in the clock tower, by having a large heavy conducting mass, to obtain, after its elevation, a spark and so realise a thunder storm, in the lightning, etc. and to explode things by it.

15813. What would be the relation of the balloon experiment with the wire hanging down?

15814. If any thing results, then we should have

15815. An entirely new mode of the excitement of either heat or electy.

15816. An entirely new relation of natural forces.

15817. An analysis of Gravitation force.

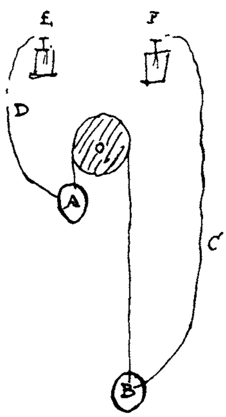
15818. A justification of the conservation of force.

15819. If either heat or electricity evolved, would probably refer both them and gravitation to actions of the ether or medium in space.

15820. What relation will the Electric Zero given by the inside of a metallic vessel bear to these effects and phenomena? Will it be zero every where?

15821. The drops at a water fall may not give the state of the air at the upper part—or an effect of induction from that air upon the brim of the fall but the state related to Gravity acquired by descending.

4 MARCH 1859.



15822. A, B, heavy weights insulated. C, D, connecting wires. E, F, two electrometers—all discharged; then A down and B up—touch electrometers—B down and A up—touch the reverse electrometers, and so on for some time, using a commutator for E and F contacts. Ought to collect opposite charges.

15823. Make A a real weight and B a hollow metal vessel of the same size—then pull up and down—A should give signs—B little or none.

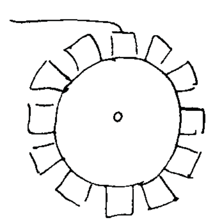
15824*. A fly wheel with insulated parts revolving—touch at top and bottom—should give contrary states; or wheel with insulated weights—might give a machine?

15825. What would be the electrical equivalent of a certain weight moved through a certain vertical space?

15826. That equivalent would be the equivalent of the *mechanical force* required to lift the weight through that space—also, when of the contrary sign, the equivalent of the mechanical force acquired in falling through that space.

15827. How would this relate to an equal mechanical force exerted by other means than gravity, as a push, or gunpowder? Or to exertion of force in planes across the lines of gravity force?

* [15824]

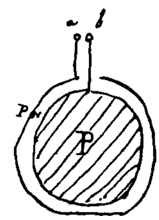


15828. Perhaps whilst the weight is falling it may not shew the electric state—only when stopped. If so, what would be the equivalent for rising—the antagonism of gravity?

15829. Motion of weights towds. or from each other in horizontal planes ought to do nothing sensible, i.e. mere motion should do nothing if not related to gravity.

15830. As to the infinitessimal difference in distance between the centre of the earth and the center of a heavy body rising or falling through 100 feet, and therefore the insensible change in the degree of the force of attraction and the impossibility of measuring it:—the gravitating force may and must vary—and varies by the force required to raise the body or that produced by its fall—that force becomes the expression for the change of gravity; small as it may appear when estimated by distance effect. It is a true and probably useful expression of the change of gravity. The Electricity may detect and even measure it.

15831. What may be the Gravelectro equivalent for one ton of matter raised 100 feet high—or for any other distance or weight? Will it be the same for all kinds of matter, or is there any specific capacity or capability concerned? Will there be any difference between paramagnetic and diamagnetic bodies?



15832. Suppose a weight P surrounded by a metallic coating N P insulated from it. The weight P would evolve electricity, but the coating, because of its little weight, none. Then if P became positive, it would induce upon the inside of N P negative electricity and an equal amount of pos. on the outside. Then the ball *a* connected with an electrometer would shew a charge exactly of the nature of P. After that and when N P had been discharged external[ly], a spark ought to pass between *a* and *b* when brought together.

15833. In this way all external influence might be separated, for if P were examined for itself alone and was found to have a state, it would be independant of such influence.

15834. In shot making—the descending shower of lead ought to charge the receiving tub below, it being insulated. Another temporary tub, insulated, might be put for an experiment on the top of the usual one.

15835. Falls of Niagara—great quantity of matter falling. There ought to be a current of discharge somewhere—probably directly into the body of the earth.

15836. *Our stairs*—free space for fall—39 feet from the stones below to the coal box platform above—and about 4 feet 10 inches more to the bottom of the hanging hook. The wheel used is a foot in diameter.

15837. Is it possible that heat, as well as or instead of electricity, may be evolved? Mr Welsh's anomaly in the temp. of the atmosphere at a given height. Also Piazzi Smyth's Teneriffe Report, Phil. Trans., 1858, page 526.

7 MARCH 1859.

15838. A Sling or loop made of thick white silk thread. Four thickness of this thread sustained half a hundred weight well when quiet, but if jerked much so as to give impetus, I could succeed in breaking the silk sometimes at the knot and sometimes else where. A sling of this silk thread consisting of 14 circles or convolutions and therefore forming a sling of 28 threads and about 9 inches long was used to suspend the horseshoe magnet. A Zamboni was employed to charge the suspended magnet by a touch, and a delicate Gold leaf electrometer (   ) was employed to test its state. The Zamboni charged the magnet and electrometer well, but in three minutes all the charge was lost by conduction through the silk. Again, the charge was nearly all lost in *one minute*. The sling is good as to strength, but as to insulation will not do.

15839. *Gutta Percha sling*. A round Gutta Percha band or rope, 0·17 of an inch thick, was made into a loop by a bad sort of tie and employed to suspend the magnet. There was an insulating space of 14 inches in length clear. The magnet being charged by the Zamboni, eighteen minutes after it was found charged, apparent[ly] as strongly as at first. Again charged and left for an hour and sixteen minutes—at the end of that time it was found still well charged. So this suspension will do as to *insulation*.

15840. Another loop made of Gutta percha, nearly 0·25 of an inch thick and having 9 inches of clear insulating length, was used with the magnet. Being electrified by the Zamboni, it kept its charge for 1½ hours. On being recharged by the Zamboni, it kept its charge for 4 hours and then it was very good. This good as regards insulation.

10 MARCH 1859.

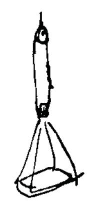
15841*. Suspended a 56 lb. weight on the thinnest Gutta Percha Sling (15839). The Electrometer was at hand and also one of the coiled wire connecters with its end support of Gutta percha in a foot, to make contact with the suspended weight. The Electrometer wire and weight being connected and the Zamboni applied to the iron suspended weight—it seemed to charge *slowly* only and not like the magnet (15843). This may be due to the increased induction on the electrometer, extended wire and on the books near to and below the weight—or perhaps to the bad conducting power of the oxide of Iron covering the weight and touched by the Zamboni.

15842. Left all charged, i.e. weight, galvanometer and the spiral connecting wire, at 10^{h} 50′—25 minutes after, all were found fully charged—left them so and 75 minutes after that again, there was still a strong charge. So the insulation good.

15843. Filed a clean place on the iron to allow of good contact—then found the charge was quick enough (15841).

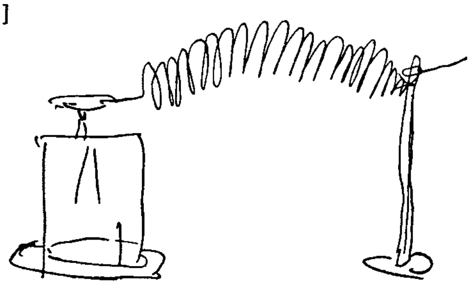
15844. Placed a piece of wet bibulous paper about 3 inches by 2 on the side of the weight, to create a damp atmosphere against it at the place. Being charged by a touch of the Zamboni, it kept a full charge for 25 minutes. Being charged again and left for an hour, found it well charged, notwithstanding the damp paper.

15845. Strength of Gutta Percha slings. A ring sling of the 0·17 gutta percha was made, with twisted and warmed joining bound round with red tape. A scale pan was hung up by it and then the length of the sling was 13·6 inches—on adding 56 lb. it stretched to 14·75 inches. 56 lb. more added after a time. Soon after, it gave way at the joint, which was very bad. Temperature 54° F.

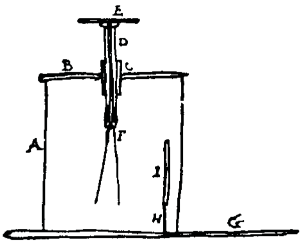


15846. Made a sling of the 0·25 gutta percha (15840): it was better in the joint than the former. With the Pan only, its length was 13 inches—put in 56 lb.—became 13¼ inches long—added a second 56 lb., length became 14·2 inches—left at 9^{h} 45′, the parts at the joint slipping and stretching a little—was holding at 12^{h} 20′, with signs of stretching at the joint and the length 14·75 inches—added

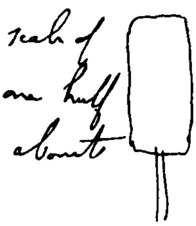
* [15841]



a third 56 lb.—at $2^h\ 45'$ the length about 15 inches and I think a little further stretching at the joint—put on the fourth 56 lb. At 5 o'clk. put on the fifth 56 lb. At 7 o'clk. it was 18 inches long and had slipped or stretched or both much at the joint—the pan was touching the papers below. On taking off the 280 lb. the sling contracted in length to 16 inches—not at the joint but generally. The joint was of this form*.

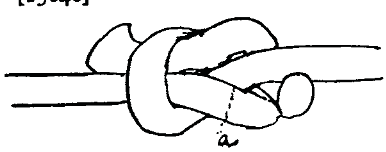


15847. Have made two electroscopes. No. 1 is of this character: A, a cylindrical glass jar—B, a brass cap to the top, having a brass tube C in the middle of it. Into this is cemented a glass tube D, which has within it a brass rod connected at the top to the brass plate E and below with the gold leaves F. This brass rod is insulated from the glass tube D by two plugs of shell lac, one under the brass cap E and the other at the bottom of the tube, just above the setting on of the gold leaves F. G is a flat brass plate carrying an upright rod H, and at the top of that a brass plate I. By slipping the glass jar along the brass plate G, the gold leaves can be brought into any degree of vicinity to I. All the brass of this instrument is kept free from varnish. The insulation and retention is so good that when the leaves are opened by a Zamboni pile to the extent of 2/3 of an inch, it will keep the charge for many hours.

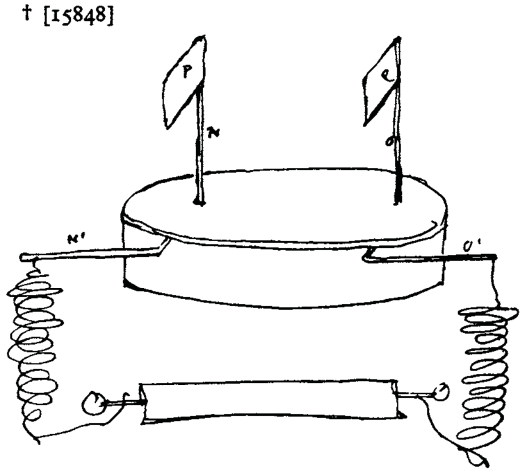


15848†. No. 2 Electroscope is, as to the jar and cap, mounted in the same way, except that the Jar is taller and there is but one gold leaf, longer than the others, about 3 inches long. The bottom is a thick cake of shell lac. Two bent stout wires N, O, are lodged in two grooves in the shell lac and are held in place by a thick sheet of Gutta Percha placed over them adhering to the shell lac. P and Q are two copper plates soldered to the tops of the wires N and O. By the motion of N′ or O′, one or both of these can be inclined inwds. or outwards so as to be nearer to or farther from the single gold leaf hanging between them. A small Zamboni column, also insulated if necessary, is connected with these two

* [15846]



† [15848]



plates so that any desired electric condition can be given to them. The gold leaf, if charged, is affected accordingly. Is not this Bohnenberger's Electrometer? Every part insulates well and holds a charge. All the metal is free from lacquer or varnish.

15849. When the leaf of gold is excited between the two charged plates or even between the uncharged plates, it often turns a little on one side. Might extend this lever principle and make a torsion instrument that, carrying a small mirror, might shew a minute deflection to a large audience, as Dubois Reymond did.

11 MARCH 1859.

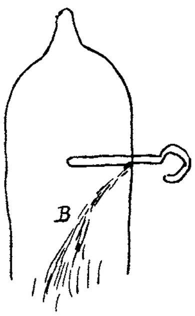
15850. Arranged the Sling of thick Gutta Percha (15846) as before—put three 56 lb. weights into the pan—the length of the sling was then 17½ inches—added two 56 lbs. more: the length became 18½ inches—added a sixth 56 lb. weight which brought the pan on to the papers below—removed them and added a seventh 56 lb. weight. Now the pan by a little time settled on the floor and the loop was 21½ inches long, every part having extended, but that near the tie joint most. After a few hours, shortened the rope suspenders—re-arranged the loop and pan—added the weights by degrees, and after having put in the eighth weight, the loop in about 10 minutes gave way, having supported 4 cwt. or 8 times 56 lb. The fracture was sudden and just at the knot, in a part thinned by the moulding; the place is marked in (15846) *a*. The sound unchanged part of the Gutta percha was very good.

I think such a loop of gutta percha safe for two hundred weight.

15851. The silk sling (15838) has been dipped in white hard varnish, stretched and hung up with a weight for a night—then put into a warm air cupboard for 48 hours; it is now perfectly dry. Being employed, as before, to suspend a 56 lb. weight and tested for insulation, it was found to be much better than before—it held a charge for 9 minutes—but it would not hold a zamboni charge for 47 minutes, for all sensible signs were then gone. It will not compete with gutta percha.

15852. Tried the Electrometer No. 2 (15848) with Zamboni associated, but it did not appear to me to be so delicate or advantageous in its use as No. 1 (15847).

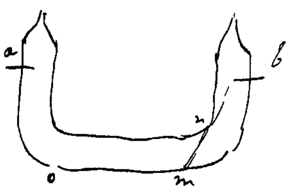
15853. *Delicacy of indication.* Connected the 56 lb. weight, the



in one position of the poles the shoot rose up, made a curve round the end of the wire and descended into the space beneath, carrying on the electric discharge. But when the poles were reversed, the shoot proceeded from the same spot but went downwards at once. These directions accorded with the expected power of the magnet in deflecting that part of the current which set off from the spot in the platinum next the glass. There were no signs of stratification—and very little fluorescence (   ); the latter fact seems to shew that the fluorescence depends on the light on the wire—for now little or no light was there. The temperature was —97° F.

[A page in Gassiot's handwriting, included in the MS. here, records further experiments made by Gassiot with tubes 32 and 18, at the Royal Institution on March 19.]

285. Took the tube out of the bath—placed it on flannel—continued the Ruhmkorf action and watched the changes as the temperature rose. There was soon a change—more light appeared. By applying the magnet, found that the shoot of light A identified itself with the former tongue of light (273), and that when B was produced by the right position of the magnet, the tongue appeared at the same time.



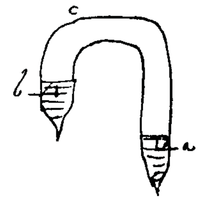
286. Also the rosy luminous covering appeared on the negative wire, and the fluorescence appeared at the same time. By the proper application of the magnet, the fluorescence could be thrown on to *m*, and I believe would have appeared further if the corner at *n* had not cut off the radiation. Looks wonderfully as if all depended upon the reflexion of the shoulder above *b* and the motion of the light on the wire *b* by the action of the magnet.

287. On examining the tube, it was found that a dew of mercury globules had gathered on the bottom from *o* to *m*. By the lamp it sublimed from place to place. The tube is not merely full of the vapour of mercury at common temperatures but there is a little excess of liquid mercury at that temperature.

288. Then placed this tube 31 in the same position in the air and applied a spirit lamp to the horizontal part of it. As the heat rose, the light became more brilliant and there were sparks or globules of light frequently appearing on the glass here and there, perhaps from globules of mercury. The fluorescence at the Negative wire was very fine—and travelled well by the magnet.

Sometimes puzzled to guess whether (or no) a plane of electric condition may exist there.

289. No. 18 is the short syphon Mercury tube (282). Hung it up in air thus—both the electrodes are mercury now. At common temperatures made *a* Positive—there was fine Pos. strata continued over to the Neg. obscurity and then the Negative glow upon the mercury at *b*. Applied the spirit lamp to the bend and the end *a*—this made the strata approach each other. As the quantity of mercury vapour increased, the light increased, and when the mercury at *a* was very hot and distilling and condensing all the way to *c*—the colour of the light was of a fine green and the dart of flame upwards from the surface of *a* very fine and luminous, but unstratified. This dart seemed bright white and the green more around it nearer to the glass. The cessation of stratification, beginning at the Pos. mercury, gradually extended to the end of the Pos. column—but the Negative obscurity remained.



290. Making *b* Positive and the hotter end *a* Negative, then a fine river of light ran through which continued until, by taking away the lamp, cooling was allowed; and then gradually close striæ appeared. When the tube was entirely cooled, it returned to its first state. The neg. terminal then resumed its rose coloured layer of light and the strata were about 1/10 or 1/12 of an inch apart in the Pos. column.

291. As to the Positive and Negative mercury surfaces. It appears that Pos. metal and Neg. insulating medium *exchange with facility*. But that Pos. medium and negative metal *exchange with much difficulty*. Exp.[1] 1600, 1480, 1525.

292. 22 *March*. Wrote to Gassiot proposing to combine the Voltaic battery current and the Ruhmkorf current through the vacuum tubes—connecting both continuously with the tube—or connecting V. battery with tube and making tapping contact with the Ruhmkorf— or else making continuous contact of the Ruhmkorf and connecting the V. Battery by tapping contacts.

[Here follow nine pages of MS. in Gassiot's handwriting, the pages (but not the paragraphs) numbered by Faraday in continuation of his own notes. The experiments recorded were made by Gassiot during the period March 22 to April 23, 1858, and on the latter date he worked at the Royal Institution.]

[1] Experimental Researches.

Steinheill's[1] apparatus up in my room and in order.

For sources of light used two platina terminations placed thus before the object end of the telescope, at distances from half an inch to two inches from the slit—the slit also being vertical.

Connected these wires with the Holland Ruhmkorff coil—and excited that by three plates of its own battery.

All acted very well and the different parts of the spectrum shewed at different heights, the rays from the upper or the middle or the lower part of the discharge inverted. These parts have different character through the ignition of the wire ends, etc. Reversion of the current was very convenient and useful in this examination.

The discharge between the platinum wires gave its own rays—well separated by the apparatus.

By moistening the ends of these wires with saturated solutions of Salts of Soda, Baryta, strontia, lithia, etc. the peculiar rays due to these substances came out well, but did not last long—rewetting the ends reproduced them.

Baryta by its green and blue rays—Strontia by its blue ray—and lithia by its fine red and other rays—will be useful in further experiments.

A Nichol's prism was placed between the platinum wires and the object end slit and another as analyser on the eyepiece of the observing telescope. They answered their purpose very well with a lithium light—polarizing and analysing the ray in due order. In one position of the analyser, lines, many, were observed crossing the line of colour light—but these moved round with the analyzer and not with the polarising prism—they were due I think to a set of reflections in the former.

I put up the Steatite burner with a strong yellow soda flame, then between it and the slit the platina wires with soda salt on them. There was no apparent object, but the yellow light of the wires was added to the yellow light of the gas burner.

[1] Presumably C.A. von Steinheil.

Steinhill's app. up—associated with E.M.: good—urged by 10 pr. plates—good action.

Also the Ruhmkorf—and the platina points-2 pr. plates. Discharge is mixed, being part bright and part haze.

When course of current in the Ruhmkorf is in one direction and discharge at *x* occurs between platina points—then when the interrupter is most open the sparks are bluest and most distinct as spark and least hazy. As the interrupter is screwed up, its interval lessens, the sparks become more powerful as discharges—more electy. passes—and there is much haze and much discharge of E. in the hazy parts around the sparks—the latter seem then even to carry less Electy.

There is a difference between the two ends of the discharge—red or yellow and violet concentration. If the Ruhmkorff current be reversed, these terminations are reversed (the spark being upright). Otherwise the general character of the spark discharge is the same whichever way the current passes.

When the interrupter is screwed out and open, and the sparks are blue with little haze, then the spectrum in the Steinheill apparatus is pale and bluish—the yellow and red rays being almost wanting; and that is the case whether the current is sent in one direction or the other, except that in one direction the red and yellow are almost entirely wanting and *a blue* is very fine, whereas in the other direction that blue and the general blue is not so fine and red and yellow faintly appears.

On screwing up the interruptor—more electricity passes and the hazy sparks appear. Then the spectrum in the Steinheill exhibits yellow very bright and red, in addition to the blues, etc., and it does this whichever way the current is going through the Ruhmkorf, only with the difference before spoken of. The difference seems to depend upon the position of the spark discharge to the slit, for it is probable that each extremity is not equally placed in relation to the slit, and if so, changing the direction of the spark will make a difference.

With interruptor still more screwed up and the sparks still more hazy—the appearance in the Steinheill was more marked by the appearance of yellow and red rays. The same whichever way the current went, but with the same difference as before.

It seems as if the warm rays come from the luminous haze, which to the naked eye has a warm coloured light.

When the Rhumkorf discharge is first sent one way, the yellow and red appear with much brightness and power, but manifestly sinks as one counts 1, 2, 3, etc. and very shortly sinks in power; then reversing the Ruhmkorf current, the red and yellow shine out with much increased brilliancy, to sink in turn; reversing the current again, they appear brilliant and quickly sink, and so on continually. The continued discharge one way or the other lessens the ability to pour out the red and yellow rays. I think the same may be said of the blue and other rays but in a lesser degree. They are indeed altogether of lesser brightness.

Arranged the Ruhmkorf terminals so that the discharge was between the poles of the Electro magnet and adjusted the interruptor so that the discharge was all pale spark and with no haziness or flame about it. Now put on the Ruhmkorf and then made magnet—there was *no apparent* deviation of the course of the sparks—they appeared to be undisturbed in direction. Then screw up the interruptor so as to give a little haze round the sparks; on putting on the E. Magnet, this haze was increased in some degree. It was thrown in between the poles or out from between them according as the Ruhmkorf current was in one direction or the other, and it was the old phenomenon of the deflection of the current by the magnetic poles. The haze, i.e. the flaming luminous part of the discharge, was collected into a flat plate of a circular or crescent form, thin and perpendicular to the magnetic axis—thrown in or out so as to be quite distinct from the place of the sparks, which remained undisturbed in their course and place.

When the interrupter was screwed up close so as to give a still more hazy spark and abundant current, then the haze and its deflection was still greater and finer, and even the bright sparks were shewn to be deflected a little in the same direction as the luminous arc. I think the magnet increased the bulk of the luminous cloud, but it might be only its disposition into a compact flat discharge—its collection from all sides to *one* side.

I do not find by a slip of paper that there is any draught either *in* or *out* in the direction that the arc seems to blow. It does not seem to depend upon any motion of the air, but to be a place or line of discharge determined by the forces, not the transference of the charged and discharging air to a distance.

28 JANY. 1862.

Apparatus up as before, same E. Magnet but with 20 pair of plates. But the Ruhmkorf was dismissed and in place of it the Gas light used and the platinum wires with salts on it introduced. The poles were as before—pointed ends about 3/4 of an inch apart and the flame between them.

When the flame was between the poles—the making of the Magnet did not change its form or direction—and there was no appearance of change in the lines when magnet made or broken.

Used Chlo. sodium on the platinum wire—still there was no appearance of change in the luminous lines in any way by the magnetism.

Poles 1/3 of inch apart. No magnetic effect either with or without the Sodium.

Used Chloride *barium*—no effect.

Chloride Strontium—no effect.

Lithia—nothing.

12 MARCH 1862.

Apparatus as on last day (28 Jany.) but only 10 pr. of Voltaic battery for the Electromagnet.

The colourless Gas flame ascended between the poles of the Magnet and the salts of Sodium, Lithium, etc. were used to give colour. A Nicol's polarizer was placed just before the intense magnetic field and an analizer at the other extreme of the apparatus. Then the E. Magnet was made and unmade, but not the slightest trace of effect on or change of the lines in the spectrum were observed in any position of the Polarizer or analyzer.

Two other pierced poles were adjusted at the magnet—the coloured flame established between them, and only that ray taken up by the optic apparatus which came to it along the axis of the poles, i.e. in the magnetic axis or line of magnetic force. Then the Electro magnet was excited and rendered neutral; but not the slightest effect on the polarized or unpolarized ray was observed.

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FARADAY'S DIARY

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FARADAY'S DIARY

Being the Various Philosophical Notes of Experimental Investigation

made by

MICHAEL FARADAY
D.C.L., F.R.S.

during the years 1820 – 1862
and bequeathed by him to the
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Director of the Laboratory of the
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AFTER several trials, the only practicable way of indexing certain sections of the Diary was found to be to group the entries relating to these sections under headings. Thus entries which would otherwise be scattered will be found brought together in the Index by prefixing to them a word or words, briefly descriptive of the subject referred to, as for example, *Electrostatics; Gravity and electricity; Induction, electro-magnetic; Light, magnetic action on*. These group headings or prefixes have been chosen to correspond, as far as possible, with the terms used in the tables of Contents in the separate volumes. To avoid repetition and economise space the prefixes, and other words repeated in a succession of entries, are printed in italics at their first occurrence and represented subsequently by dashes.

It may be of interest to say that the method of indexing arrived at resembles very closely that used by Faraday himself in the volumes of his *Experimental Researches*.

T. M.

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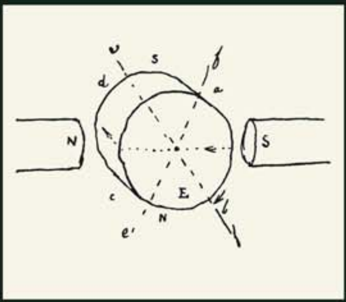

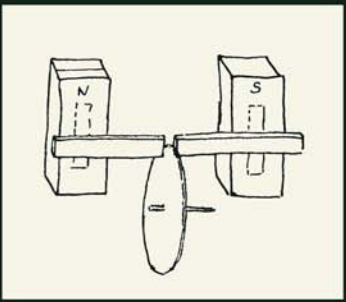

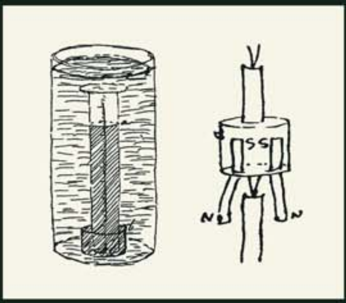

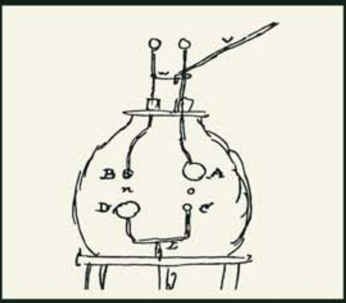

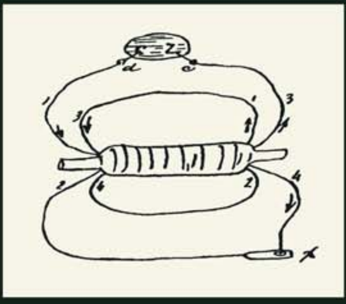

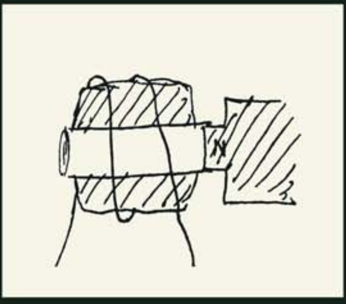

"Faraday is generally held to be one of the greatest of all experimental philosophers. Nearly every science is in his debt: and some sciences owe their existence mainly to his work. The liquefaction of gases, benzene, electro-magnetic induction, specific inductive capacity, lines of force, 'magnetic conduction' or permeability, the dark discharge, anode, cathode, magneto-optics, electrochemical equivalent; all these terms suggest fundamental researches which he made, and many of them were called into existence in order to describe his discoveries. It is extraordinary that a man who did such excellent work, should also have excelled in his description of it." (1932)

SIR WILLIAM H. BRAGG
Director of the Laboratory
of the Royal Institution

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