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 experiments 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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Director of the Laboratory of the
Royal Institution

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FOLIO VOLUME V OF MANUSCRIPT

PLATES

FOLIO VOLUME IV
OF MANUSCRIPT
(CONTINUED)

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?

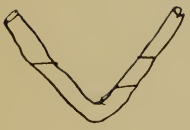
5768. Or if it is said that the force of contact of the acid, or rather its difference of force, becomes gradually greater as the acid is more and more dilute, the metal in the stronger acid always being Negative to the metal in the weaker; i.e. in the cases, as of nitric acid, where the weak acid is the one making the metal Positive (the reverse of course being supposed to be the case where the 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.

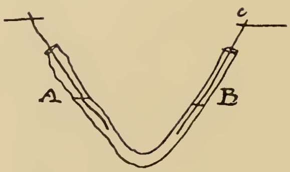
5774. *Lead Lead* in these acids. Lead in A was Pos. to that in B or in C (5712). But lead in B was Neg. to that in C. So that B acid rendered the lead Negative either to lead in acid stronger or weaker (5828).

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 a power—not mere indifference, but of a peculiar kind; here it has been much more effective in producing changes than Palladium.

5793. On taking out the Palladium wires, a little powder as dirt wiped off from the A Palladium and a very little also from B. These probably indicate some chemical action.

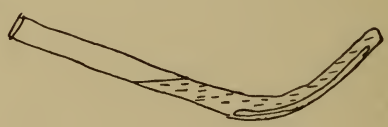
5794. Now *Iron Iron* in *Sulphuret of Potash* strong solution. Wire B was Pos. to A at the immersion—soon came to 0°. Heated A: the needle scarcely moved—cooled it: still quiet. Heated B: it was the smallest degree Pos. and then returned—then left quiet, was Neg. 1°. On cooling it retd. to 0°. The effect was very small indeed and is as nothing. On taking out the wires, the parts in the solution were bright and wiped clean on cloth, but a part of one which was in the air in the tube above the solution was tarnished and wiped black on the cloth.

5795. Diluted the solution 1 vol. to 12 of water and replaced the Irons, to see if action on them when dilute solution used (5647). At the first B was a little Pos. Heated B: there was very little change of the needle, but on cooling it, it became Pos. 8° or 9° and there was evident action, for the yellow colour of the solution was rendered rather green and sulphuret was there. Still, the lower part of the iron was bright and the upper part in the air was black; and the sulphuret may have formed at the upper part and washed down with the boiling.

5796*. Put some of the same solution into a tube bent and closed at one end. Put in a piece of wire so as to be entirely immersed in the solution and quite clean. Heated the end of the tube, where no air was, to boiling for several minutes and then left it still. At first there was no discoloration and the iron apprd. quite bright, but in a short time action appeared at one spot on the iron. Black sulphuret formed there in a little spot like rust, i.e. compact and limited, and from that place a little green cloud rose into the yellow solution. As the piece of wire had been touched by the Laboratory pincers, it is possible some irregularity there caused this action. At all events, the great difference between the action of the air in the tube before used, and the non action now air was excluded, was very evident.

5797. So Iron shews no effects in favour of a sensible thermo electric current.

* [5796]



5798. Made experiments with red nitric acid in reference to possible *thermo electric* currents. But the results were here altogether new and of a different nature to those expected, and in reference to the thermo effects, I think their existence is very doubtful in any of the experiments made.

5799. *Platina; Platina; Very Orange Nitric acid* (5607, 5411). At the outset A was Pos. 10° and continued so when at quite uniform temperature, as if there were some vigorous chemical action going on here. On heating either side, the Platina there became Negative 60° or 70° or more. The effect very striking but surely cannot be one of thermo electricity.

5800. The moment the spirit lamp was applied the needle moved, and I think almost before the Platina could have been sensibly warmed.

5801. Brought the whole to common temperatures and the needle was at 0°. Then dropped a little ether on the one side to cool it. That side quickly became Positive 15° or more: cooling the other side produced the corresponding effect. Must remember in relation to these effects that the acid is an excellent conductor and so the effects are, as it were, magnified.

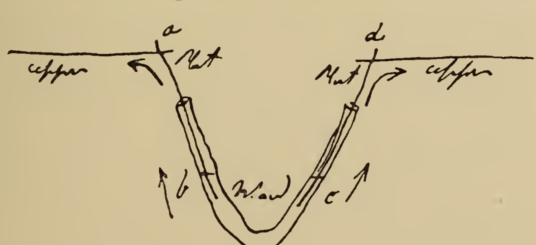
5802*. When with the same arrangement a joint of the copper and platina was heated, the deflection was not more than 5°, the heat being then far higher than before for the acid in the tube. The order of the current produced when any *one* of the spots *a*, *b*, *c*, *d* were heated is shewn by the arrows.

5803. When all was at a uniform temperature, I took out one wire, washed, dried and warmed it (not to 212°), and then put it into the acid; it immediately apprd. to be Neg., producing such a current. I then took it out, washed, dried and cooled it by a little ether, then quickly wiping and immersing it in the acid, it was Positive.

5804. *Gold; Gold; Red Nitric acid.* Repeated the same experiment, using gold wires. The effects were just the same and I think to as high a degree with this metal as the former. Either heating or cooling the liquid or heating or cooling the wires, the proper effects were produced.

5805. *Palladium; Palladium; Red Nc. Acid.* Palladium and the acid acts at common temperatures and a brown solution is formed

* [5802]



at the wires. A was Pos. Warmed A side and whilst the chemical action was going on, it became Neg. This might be partly due to power like that of the Gold or Platina, and in part to the investing solution formed by chemical action. But it was striking. It at all events shews that the chemical power of Palladium (which was much increased by the heat) is not a strong current force.

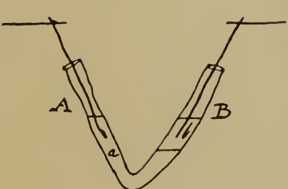
5806. Would be well to make out what is that state of Platina, Gold and perhaps Palladium which makes their chemical actions differ so much as sources of voltaic power from the metals at the other end of the series, as Cadmium, tin, zinc, etc. etc. Remember the Gold on Nitro-muriatic acid (5595).

5807. *Iron; Iron; Red Nitric acid.* Violent momentary action when first put in and corresponding momentary effect on the needle. Then became inactive and quiescent to the usual degree.

5808. Heated B side; did not become Neg., but *more Positive*; cooled it and the effect went down. Heated A and it in turn became very much Pos. So Iron is quite unlike Platina or Gold or the inactive metals in this acid.

5809. It would also appear that Iron and Platina, when in Nitric acid, are not quiescent because they are alike to each other, for they are evidently very different. Hence they should shew the effect of contact, if there is any such effect, where they touch each other.

5810. These effects of Platina, Gold, etc. seem almost to be effects produced by a power in these metals of altering the state of the acid around them (the Ns. acid, etc.), like that they have in mixed oxygen and hydrogen, and makes them sources of a current in the contrary direction to that produced by oxidizable metals —it is very wonderful. It is most probably related to the former effects, in which, though heating one of two platinas did nothing, yet heating the Platina of Silver and Platina ( ) made it Negative. Must examine these effects all together and bring them into proper relation.



5811. Conceiving that the effect might be due to some peculiar action of the Nitrous acid in the Nitric acid, which though increased by heat, did not depend for its existence on it, but was in the foregoing cases balanced at the two extremities, I arranged the tube thus, making *a* colourless Nitric acid; *b*, red nitric acid, and using

Platina wires. Immediately B side became Pos. to A side 70°. Changed the wires side for side, but still the B side was Pos. 70°. This therefore must involve the cause of the effect in the former cases, and now it appears to be independant of temperature, to be in the Nitrous acid in some way and to be an action making the Platina Pos. instead of Negative. So that in the former cases, the heat was perhaps only shewing an effect by diminishing the pre-existing action, not by causing a new one. But this, and the true locality of the action, I must search out carefully hereafter.

5812. Warmed A, or the colourless acid side, and it became More *Negative*; cooled A and its former degree of Negative state was reproduced. Warmed B or Red Nitric acid side and its state became less Positive, but the change was very little in degree compared to what it had been when both the Platinas were in Red acid (5799). In fact the difference of warm and cold in the same red acid is but little, compared to the difference of coloured and colourless acid to each other at common temperatures.

5813. Placed *Gold Gold* in the two acids of the tube (5804). A was Pos. at first for a moment, then B became spontaneously Pos. to about 25°. Took out, washed, wiped and changed the wires: still that in A was a trace Pos. at the first moment and then B became Pos. about 25°. (Can A at that first moment be taking up a state analogous to that taken by Iron at the first moment in Nitric acid?) So the permanent deflection, though in the same direction as with Platina, is nothing like so strong. Heating the Gold in the A or colourless acid side made it much more Negative, as before–cooling that side lowered it again. Heating B or the red acid side made it a little less Pos., but as with the Platina (5812), so here the quantity of effect was nothing like so much as that of the heat on A.

5814. The gold deepens in colour in the Nitric acid as if there had been some action upon it.

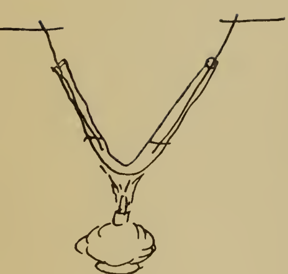
5815. Then put *Gold Gold* into Nitric acid, colourless on both sides, to compare the effect of heat on it now, when the metal being in the same acid was *not Negative.* At first the needle was quiet and at 0°; then heat either at A or B produced scarcely a sensible effect. So that in the above action the effect of heat on the metal in the colourless acid is still related to the metal in the red acid, i.e. to the Positive metal (   ), as in former cases.

5816. When all was at the same temperature and quiet, I dropped a little red Nitric acid in on B, and that instant the gold wire in that end became Pos. and strongly so for the moment.

5817. I examined the colourless and coloured Nitric acids for Muriatic acid, supposing a little of that might be present, and active as chlorine on the Gold or Platina. I found none in the colourless acid, but there was chlorine in the red acid. I therefore added some liquid Nitrous acid (from distilled nitrate of lead) to a portion of the pure colourless Nitric acid, so as to make a red acid which, when tested, was quite free from chlorine or muriatic acid.

5818. When this acid was used in place of the former coloured acid, either by itself with the platina wires (5799) or in conjunction with the colourless nitric acid (5811), it gave exactly the same effects and as strongly as before. They do not depend therefore upon any chlorine or muriatic acid on the Platina or Gold.

5819. Tried an experiment or two with fuzed electrolytes to ascertain if correct results might be obtained with certainty from them.



5820. *Platina; Platina; fuzed Nitrate of Silver.* This experiment was made in a bent tube. The actions at first rather strong and irregular. After a while, A was rather steadily more Pos. than B when the temperature was as nearly as might be alike. Then whichever side was made distinctly hottest became a little Negative, but the effect went down much in degree whilst the heat was continued and sometimes even to the cooler side becoming the Negative. If A had been made Neg. by continued heat and had gone back in degree, then heating B made it, B, more Neg. by far than it would otherwise have been or than it could continue. This appears to be that common effect (   ) due to the forces accumulated by a certain current and coming into play in the contrary direction when that current is stopped.

5821. On the whole, the apparent effect of heat is to make the wire heated Negative; but that this is a pure thermo-electric effect is very doubtful—for it falls much, though the heat be continued, and on changing the heat to the other side the current produced is over strong at the first. These effects are more accordant with chemical than thermo-actions.

5822. Is it possible that Platina can determine the separation of silver at the heated side and so become an active negative surface ( )? On taking out the platinas, they were well washed and wiped, and as they looked a little different from their first appearance, bng. rather more rough, I examined them by a drop of nitric acid to find metallic silver upon them. There certainly was a trace of silver on them, especially on B side wire. But it would require polished wires and great care to decide the notion of *reduction by the Platina.* It is however worth deciding.

5823. *Platina; Silver; Chloride of lead.* Silver is Pos. to Platina in fuzed chloride of lead. On taking out the metals, there was no evident action, but quite enough to produce the effects would have been inappreciable. Such experiments with fuzed electrolytes are not easy to make without good glass, free from lead, and an especial heating apparatus.

5824. It appears to me that the experiments on Nitro muriatic acid (5593), Nitric acid (5410, 5606), nitrous acid, etc. etc., as well as the others on strong and dilute acids, hot and cold acids, etc., are well fitted to illustrate many points: as for instance, the locality of the generating force as at this or that surface; the manner in which heat affects chemical affinity in local actions; the constancy of chemical force (in certain of the cases of dilution) at a given temperature; the nature of compound and complicated electrolytes, as Nitric acid, hydrated sulphuric acid, etc. etc.

5825. All those effects, so numerous, which relate to the state the metals take in consequence of a previous current; or of investment; and perhaps some of those where Platina, being Neg., becomes more Neg. by heat, should be collated and compared with Schonbein's Polarity of liquids;

Schonbein's indifference of Iron in N. Acid, etc.;

De la Rive's peculiar state of Platina electrodes by current;

Marianini's single pair producing same state;

and putting Platina plates into oxygen and hydrogen; as well perhaps as my results with clean and dirty Platina in the sixth series.

5826. When going to work on Henry's late dynamic induction experiments, remember that the following parties have Henry's inductive coils—Gassiot; Leeson; Edwd. Solly; The Adelaide Gallery; Daniell.

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5827. Repeated the expts. with Lead and copper (5774) but made an acid B weaker, thus

A was Oil of vitriol.
B 1 vol. Oil Vit. + 2 vols. Water
C 1 „ „ + 20 „ „

5828. *Lead* in A was well Pos. to that in B, and it was before Pos. to that in C (5712). But lead in B was well Neg. to that in C, and also to that in A. So that this B acid rendered the lead in it Negative and well so to lead in acid of the same kind, either stronger or weaker (5774).

5829. Copper in A was a trace Neg. to copper in B, and it was in the former case (5710) a little Neg. to copper in C. Also copper in B is a little Pos. to copper in C, and a trace Pos. to copper in A. So copper in B is Positive to copper in stronger or weaker acid of the same kind (5775).

5830. Whilst comparing the coppers in A and B, I thought the small effect produce[d] might be an effect of dilution (5675). I therefore used Platina wires instead of coppers in the same acids, but did not obtain the same effects. So that I believe the result is not due to the mere dilution, but to the chemical relation. The result with lead is far too powerful to admit any thought of this kind.

5831. Made a comparison of different metals in three different nitric acids, to see how their relations ran and changed about. The table on the next page[1] gives the tabulated results. The acids were

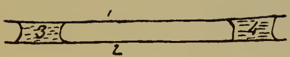
A—strong Nitric acid.
B—1 vol. strong acid + 1 vol. water.
C—1 „ „ + 20 vols. water.

[1] i.e. par. 5832.

5832. See (5846).

	in acids A and C	in acids B and A	in acids B and C
Copper; Iron	cop. in A very Neg. to iron in C	cop. in B very Pos. to . . iron in A neg. at first moment	copper in B very Neg. to . . iron in C
	iron in A Neg. to . . cop. in C	iron in B very Pos. to . . cop. in A	iron in B least Pos. to . . . . copper in C
Copper; Silver	cop. in A little Pos. to sil. in C	copper in B Pos. to . . . silver in A	copp. in B well Neg. to . . silver in C
	silver in A Neg. to . . cop. in C	silver in B Neg. to . . . copper in A	silver in B well Neg. to . . . copper in C
Copper; Lead	cop. in A Neg. to . . . lead in C	copper in B Pos. to . . . lead in A	copp. in B very Neg. to . . . lead in C
	lead in A Neg. to . . . copper in C	lead in B Pos. to . . . . copper in A	lead in B a little Pos. to . . . copp. in C
Lead; Silver	lead in A Neg. to . . . silver in C	lead in B strong Pos. to silver in A	lead in B little Pos. to . . . . silver in C
	silver in A Neg. to . . lead in C	silver in B slight Neg. to lead in A	silver in B very Neg. to . . . lead in C
Lead; Iron	lead in A Neg. to . . . Iron in C	lead in B strong Pos. to iron in A	lead in B Pos. to . . . . iron in C then iron very Pos.
	iron in A Neg. to . . . lead in C	iron in B well Pos. to . . lead in A	iron in B constantly Neg. to lead in C
Iron; Silver	iron in A Neg. to . . . silver in C	iron in B Pos. to . . . . silver in A	iron in B well Pos. to . . . . silve in C
	silver in A Neg. to . . iron in C	silver in B Pos. to . . . . iron in A at first instant Neg.	silver in B extremely Neg. to iron in C

Plenty of differences, etc. Perhaps the most striking is that copper and silver being in acid C, the copper is Positive. Keeping the silver in acid C and putting the copper in the medium acid B, it, the copper, is Negative to the silver. With silver in acid C still and copper in acid A, the copper becomes Positive again. So as the concentration proceeds with the acid at the copper, it becomes from Pos.—Negative, and then returns to Pos. again. But must repeat this to be sure that copper in B is Neg. to silver in C (5845).



5833. Arrangements producing voltaic currents without difference of metallic contact. Two metals were taken, and connected in two places at their extremities by solutions intended either to conjoin in producing a current, or that the current of one should be strong enough to overcome the resistance at the other place, and so the current pass (decomposing in fact if conduction cannot happen in an electrolyte without decomposition). Thus the lines 1 and 2 may represent two metals, as zinc and platina, and 3, 4 the two solutions, as nitric acid and iodide of Potassium, which are to serve as the electrolytes. The metal 1 or zinc was generally an arrangement in which the terminal parts only were zinc, the rest being copper wire; and the metal 2, or platina, was similarly arranged, for then a galvanometer could be introduced into the circuit to indicate and generally measure the current produced. In the following table the order of contact is that of going round the circle, and the red arrow heads[1] indicate both the direction of the current produced and the *place* where it is determined. Opposing actions are indicated by a small black arrow.

Iron →	dilute N. A.	Platina	Strong sol. Sulrt. Potash	full current
„ →	„	„	Red nitric acid	full current
„ →	„	„	Strong sol. Potash	feeble current
„ →	dilute Sc. A.	„	Sulrt. Potash	full current
„ →	„	„	Red Nc. acid	good current
„ →	dilute M. A.	„	Do.	good current
„ →	„	„	Sulrt. Potash	good current
„ →	„	„	Sol. Potash	feeble current
„ →	Sol. oxalic A.	„	Do.	feeble current, very
„ →	„	„	Sulrt. Potash	small or weak current
„ →	„	„	Red Nc. acid	a current
Platina	dil. N. A.←	Silver**→**	strong N. A.	feeble current
Copper→	Sulrt. Potash	Iron →	dil. N. Acid	powerful current
„ →	„	„ →	sol. iodide Pm.	current
„ →	„	„	sol. Acetate Lead	feeble current
„ →	„	„	sol. Potash	doubtful
„ →	Strong N. A.	„ →	dilute N. A.	very powerful
„ →	„	„ →	iodide potash	fair current
„ →	„	„	acetate lead sol.	feeble current
„ →	„	„	sol. potash	doubtful

[1] In the manuscript, all the arrow heads are red, except that printed in heavy type in the twelfth line of the table.

Silver →	Sulrt. Potash	Iron →	dil. N. A.	Strong current
„ →	„	„ →	Iodide Pm.	fair current
„ →	„	„	Potash	Do.
„ →	Strong N. A.	„ →	dil. N. Acid	strong current
„ →	„	„ →	Iodide Pm.	good current
Copper	Sulrt. Potash	Platina	„	not sensible
Silver	„	„	„	not sensible
Lead	„	„	„	not sensible
Zinc →	Potash	„	„	feeble, but decomposes
Tin →	„	„	„	very feeble, not decompose
Cadmium	„	„	„	not sensible
Lead →	„	„	„	very feeble, not decompose
←Copper	diluted N. A.	Platina	Strong Pale N. A.←	feeble current
Lead →	„	„	„	good from first moment
Iron →	„	„	„	good current
Tin →	„	„	„	moderate
←Cadmium	„	„	„ ←	good current
Zinc →	„	„	„	moderate

In these six experiments, the ends in the dilute acid were first immersed, and then those in the strong acid; hence probably the reverse currents with copper and cadmium.

Tin	Strong S. A.	Iron →	diluted S. A.	strong current
„	„	copper →	„	moderate current
Lead	„	Iron →	„	strong current
„	„	copper →	„	feeble current

5834. It appears to me probable that any one of the very numerous combinations which can be made out of the following table would produce a current, and that some of them would be very powerful.

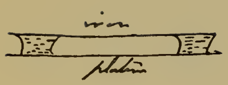
Platina Gold Palladium Rhodium Silver Nickel Copper Lead Tin Zinc Cadmium	With either *strong Nitrous Acid* or a *strong solution Sulrt. Potash*	arranged with *Iron* and any of the substances in the next column, namely	dilute Nitric acid „ sulphuric acid Muriatic acid Solutions of vegetable acids Iodide of Potassium Iodide of Zinc solution of salt many metallic solutions

Besides these cases there are also

5835. All the cases of currents with *one metal* and one acid heated at one of the junctions (   ).

5836. All those of *one metal* with the same acid, but strong at one end and diluted at the other.

5837. Perforce all those where, different metals being used, the effect of dilution or heat is to cause a current in the reverse direction to that which it is supposed the metallic contact ought to produce (5695).



5838. Arrange two plates of iron and Platina with two drops of strong nitric acid between them. There is no current. Then heat one end until action is caused, and there is a current. How can this depend upon contact?

5839. Or all being quiescent, dilute one drop a little and there is action. How can this be contact?

Batteries of no metallic contact

5840.	Iron dilute acid Copper Sulrt. Potash Iron, etc.	or	Iron dilute acid Silver Sulrt. Potash Iron, etc.

or

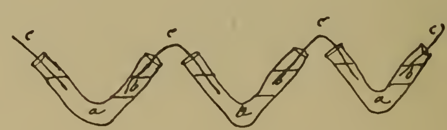
5841*. *a* is strong nitric or sulphuric acid, and *b* is dilute acid of the same kind; *c* are wires or plates of one metal only, being copper, iron, silver, tin, lead or any of those metals which become *Pos.* and *Negative* by difference of dilution of the acids.

5842. *or* Strong and weak solution of potash with zinc, lead, cadmium, etc. will do the same thing.

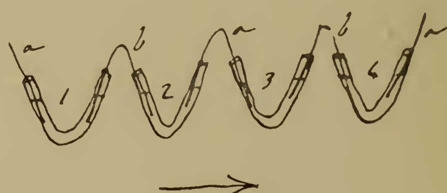
5843. If the fluid above be sulphuric acid and Iron the metal used, the arrangement will give a current in one direction—but if tin be the metal used the current will be the other way.

5844†. *or* the following arrangement with two metals not touching. 1, 3, 5 contains strong sulc. acid; 2, 4, 6 dilute sulc. acid; the metals *a*, *a* are tin, and *b*, *b* iron. The current will be in the direction of the arrow.

* [5841]



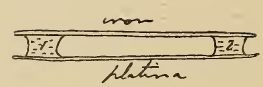
† [5844]

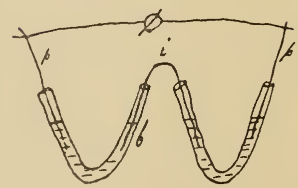


5845. Repeated the experiments as to copper and Silver of (5832). Silver in N. Acid C was compared with copper in the acids C, B and A. When the copper was in C, there was scarcely a sensible current. When it was in B, the metals were as nearly alike as well could be: the silver was in the least degree positive and the copper in B acid was acted upon. When the copper was in acid A, or the strong, there was first of all a hesitation in the needle action and then the silver was positive. So that the results are but feeble and in the last point are not like the former ones: the feebleness of the differential action involves these variations.

5846. In reference to the same table of results (5832), ascertained the relative state of silver, copper, iron and lead to each other in the N. A. B, i.e. 1 vol. acid + 1 vol. water. The *lead* was Positive to Iron, copper and silver. The *iron* was Pos. to copper and silver, and the *copper* was pos. to Silver.

5847. Made this arrangement, in which two metals, iron and platina, are connected at two different places by the same strong nitric acid, but with the power of heating or diluting one place to observe the effect. The following is the practical arrangement. The two tubes contain the same strong pale nitric acid; the wires *p*, *p* are platina; the wire *i* is iron, and a galvanometer is in the circuit. All was neutral at first. Heated the iron end, etc. at *b*. It is surprising what a little difference this made; the deflection was perhaps not more than 20° and yet the heat caused action to go on on the warmed iron and the metal dissolved there, though with difficulty. This is a very striking result in connection with the peculiar condition of iron in nitric acid.





5848. In this case it was not so much that the cold iron could not transmit the current produced (though it is known from the former expts. with thermo currents that iron is far worse than platina in this respect), as that the hot iron could not produce a strong current, its action being almost entirely local; and the following result shews that.

5849. The heated part *b* was cooled down—then a drop of water was put in there on to the nitric acid—at first there was no chemical action and no deflection of the needle. Moved the water and acid about with the end of the wire. In a few moments proper chemical action came on, the iron not evolving Nitrous acid, but nitrous gas apparently, or a lower grade of nitrogen compound with oxygen; and *now* the acting iron was powerfully Pos., and the current ran round the circuit properly and in a manner very different to that of the former case by heat.

5850. As the heat rose from the action there were redder fumes evolved at the place of action in the tube, and I *thought* the deflection of the needle was then not so strong. The action was apparently more local.

5851. How are all these effects referred to the theory of contact?

5852. Some more special results on voltaic currents without metallic contact—see (5833).

Iron	dil. N.A.	Platina	hydr. Nitrous acid	most powerful current
,,	Mur. acid	,,	,,	Do.
,,	sol. common salt	,,	,,	Do.
,,	common water	,,	,,	Powerful current

In all these the current was excellent. The nitrous acid was that green hydrated acid before described ( ), and it seemed to make no difference to the Galvanometer needle whether the iron and platina terminations in it were in contact with each other, or connected only by the fluid. The results were most striking in relation to metallic contact.

Iron	dil. N.A.	Platina	sol. iodide Pm.	small current }
,,	Mur. acid	,,	,,	small current }
,,	dil. S.A.	,,	,,	Do. }
Copper	dil. N.A.	,,	,,	Do. }
,,	Mur. A.	,,	,,	Do. }
,,	dil. S.A.	,,	,,	not sensible ✓
Tin	dil. N.A.	,,	,,	small current
,,	Mur. A.	,,	,,	Do.
,,	dil. S.A.	,,	,,	Do.
Lead	dil. N.A.	,,	,,	Good current
,,	Mur. A.	,,	,,	Good current
,,	dil. S.A.	,,	,,	small current
Cadmium	dil. N.A.	,,	,,	good current
,,	Mur. A.	,,	,,	good current
,,	dil. S.A.	,,	,,	not sensible ✓
Zinc	dil. N.A.	,,	,,	good current
,,	Mur. A.	,,	,,	good current
,,	dil. S.A.	,,	,,	very feeble ✓

All caused *decomposition* of the iodide evolving iodine against the Platina *except* Copper, Cadmium and Zinc in dil. S.A.

5853. *Plumbago* is often Neg. to Platina in solutions which would seem to have no power of acting on the platina to induce a current, and yet the plumbago would not seem capable by any action of its own, like that of deoxidizement (as with per oxides), to be an active source of what may be called a Negative current. Made some experiments to elucidate this point.

5854. *Plumbago; Platina; Mur. Acid.* The plumbago was one of Morden's slices and was fresh: the Muriatic acid was pure. On first immersion the *Plumbago* was *strongly Neg.*, but quickly settling to plumbago a trace Negative and that constantly.

5855. Took the Plumbago which has been soaking in Muriatic acid (strong) for 7 weeks (5241)[1]; it had communicated iron to the acid and coloured it yellow; it had also become rotten and broke easily. This plumbago was also clearly Neg. to the platina, and when steady, to the amount of about 10°.

5856. Took another fresh piece of Plumbago, and it in Muriatic acid was clearly Neg. to platina. The acid quickly acted upon it and in a few minutes was yellow from iron dissolved. I boiled this plumbago in the acid for 5 minutes, then cooled it a little, but trying it whilst still warm, found it was powerfully *Pos.* to Platina —its state having been thus reversed. Cooled it quite and it was still Pos. a little to Platina. So there is no permanent effect to produce a neg. current here.

5857. Put the same piece of plumbago back into the acid and boiled: removed the acid and put fresh so as to exhaust all chemical action. Did this again and at last left the Plumbago in the acid on the sand bath for a day or two, to exhaust it of chemical power (p. 1326[1]).

5858. *Plumbago; Platina; Nitric acid*, i.e. 1 vol. strong N.A. + 4 vols. water. Fresh plumbago was *Neg.* to the platina—warmed it in the acid and it quickly became very Pos. to Platina—cooled it and it then was either 0° or the smallest possible trace Neg. to the Platina. Warmed it again and it was well Pos. again.

5859. The Plumbago and dilute N. Acid of (5241)[2] the 23rd Septr. was put into a glass. The acid was colourless and the Plumbago was *Neg.* to Platina—warmed it a little and it was still Neg.—warmed it pretty well it was still a trace Neg., but not as before in degree—left it on the sand bath for 1 hour—now much iron

[1] Par. 5893. [2] This should be 5214.

dissolved out and the acid, which was colourless, became yellow from iron. The plumbago was now well Pos. to the platina—being cooled, the plumbago was again well Neg. to the Platina. (This acid was 1 vol. strg. N. A. + 5 vols. water).

5860. Put this and the other plumbagos which had been used in the N. A. into a flask with fresh N. A., to heat on the sand bath and become exhausted of any chemical reagents (p. 1325[1]).

5861. *Plumbago; Platina; Potassa.* Plumbago has been soaking in solution of caustic potassa (5241[2]) since 23 Septr., or 54 days. It is clearly a little Neg. to Platina—about 3°. The plumbago does not seem altered and the solution is not coloured. I boiled the plumbago in the alkaline solution for 5′ and now it was clearly Pos.; more than it was before Neg.

5862. Fresh plumbago in Potassa is clearly Neg. to Platina and then falls to about 5° Neg. Warmed it a little and the Neg. state of the plumbago *increased*—heated it more and now it became clearly Pos. to Platina, and when cold was still a slight degree Pos. This increased effect of Neg. on first heating is very interesting, but the whole result shews that Plumbago has no permanent power of producing a current by contact.

5863. *Plumbago; Iron; Potassa.* Fresh Plumbago is negative to Iron in Potassa, as it is to Platina, and perhaps rather more so than to Platina. This is natural enough, but the explication of its relation to Platina will probably shew its relation to iron.

5864. *Plumbago; Platina; Sulphuret of Potassa.* A fresh piece of plumbago in strong solution of sulphuret of potassa (5263, 5268) was *very Neg.* to Platina—being heated for a minute or two in the solution, it was more Neg. to Platina than before—heated still longer and then the Plumbago became fairly *Pos.* to the platina.

5865. The piece of plumbago left in sol. sulrt. potassa (5241[2]) since 23 Septr., or 54 days, was found in the least degree *Pos.* to platina, at once and at common temperatures.

5866. It is clear therefore the power that fresh plumbago has is not inherent but exhaustible, as a chemical power would be.

5867. Left the fresh piece, etc. in sulrt. potassa on the sand bath to take up a final state (p. 1333[3]).

5868. Per oxide of Manganese is Neg. to Platina in certain solu-

[1] Par. 5888. [2] This should be 5214. [3] Par. 5931.

tions. Is this an effect of contact or of an exhaustible chemical action? Per oxide of lead has the same power, as Schonbein and others have shewn. Made a few experiments with these oxides to illustrate this point, and first with per oxide of lead, being that prepared by well boiling in distilled water and found to be a conductor of a thermo-current (   ).

5869. *Per ox. lead; Platina; Mur. acid.* Per oxide of lead very Neg. indeed; much chlorine evolved and chloride of lead formed. Is quite in accordance with theory of chemical action.

5870. *Per oxide lead; Sol. Salt; Platina.* Per oxide when cold is a little Neg. to Platina—by heat is more and well so. Boiled them together for several minutes—was still so. Continued the boiling for some time and found that plenty of chloride of lead was formed. Hence sufficient reason for the action and state of the per oxide.

5871. *Per oxide lead; Platina; strong pure N.A.* When cold the per oxide is Neg. to the Platina. Being heated in the acid, there seemed to be an action and the continued evolution of a little gas —then it was the least trace Neg.—heated it again and after some time still it was Negative, as if the generating action were still going on.

5872. A little per oxide of lead, being boiled in nitric acid, soon changed in colour and disappeared, leaving a grey insoluble substance in its place: this substance dissolves in water, crystallizes from its Nc. acid solution, and is precipitated by Sul. Soda. It is doubtless Nitrate of Lead, and from its formation from Per oxide and Nitric acid arises the Neg. state of the Per oxide and the current produced.

5873. *Per oxide lead; Platina; dilute S.A.* The per oxide is Neg. fairly to the Platina—heated the acid and the per oxide was more Neg. than before.

5874. Imagng. that the addition of a little organic or reducing matter added to the electrolyte ought to increase the action on the per oxide of Lead, I added a little alcohol to the acid and thought that the per oxide was more Neg. than before.

5875. *Per oxide lead; Platina; Sol. oxalic acid.* Per oxide is most powerfully Neg. Strong action goes on, carbonic acid is formed and evolved and oxalate of prot oxide of lead is produced. If enough per oxide be triturated with the acid the latter becomes entirely neutral. This and muriatic acid furnish I think good illustration of all the actions of per oxide of lead in producing currents and becoming Neg. to Platina.

5876. *Per oxide of lead; Platina; Sol. Tartaric acid.* The per oxide was Negative. There was action and the evolution of a little gas (carbonic acid) at the per oxide, but the effect was not to be compared in strength with [that] of oxalic acid. A little nitric acid was added: the action at the per oxide was increased, but the effect was local, for the needle was not more deflected.

5877. *Per oxide of lead; Platina; sol. Sulrt. Potash.* The per oxide was very Neg. to the Platina. The needle stood at 70° steady. Per oxide of lead in sol. sulrt. Potassa becomes Sulphuret of lead directly. So this deflection depends upon chemical action.

5878. *Per oxide lead; Platina; sol. Potassa.* The per oxide is Negative to the Platina—when made hot, very Neg., i.e. to 20° nearly. After boiling some time it was still Negative.

5879. A little of the Alkali, being neutralized after this boiling, gave full odour of chlorine, so that some chlorine compound is here present—but it should not alter the relation of the per oxide and potash. Per oxide of lead boiled in a solution of potash gives a solution of the protoxide in a short space of time. It is easily shewn by Hydro sulrt. ammonia.

5880. Now worked with *per oxide of Manganese*, using such as is employed for yielding oxygen. Employed fragments of it.

5881. *Per ox. Mang.; Platina; Mur. Acid.* Chlorine produced and the oxide fairly Negative, as was to be expected from theory of chemical action.

5882. *Per ox. Mang.; Platina; Strong pure N.A.* Per oxide Negative moderately. Added some water and alcohol (5874): violent action came on between the Alcohol and acid, but on adding more water, that went off except at the piece of oxide, which appeared to effervesce and was more Neg. than before. Per oxide boiled in Strong N. A. gives a solution of protoxide and of Iron. Dilute N. A. with a little alcohol and per oxide of Manganese, being put on the sand bath, in 5′ all the per oxide was dissolved and proto nitrate formed.

5883. *Per ox. Mang.; Platina; dil. S.A.* The per oxide is in the least degree Neg. to Platina. In much stronger acid it is the same.

A little heat very much increases the effect. The reduction of Per oxide to prot oxide in Sulc. acid is one of the processes for obtaining oxygen.

5884. *Per oxide Mang.; Platina; sol. oxalic acid.* The per oxide is Negative but not strongly—only in small degree—heat increases the effect a little. The oxide is very compact and hard compared to the per oxide of lead (5875).

5885. *Per oxide Mang.; Platina; Sulrt. Potash.* The per oxide is slightly Negative to the Platina.

5886. *Per oxide Mang.; Platina; Potash.* Clean crystd. native per oxide Manganese was put away in pure sol. of caustic potassa (5214) on the 23 Septr. The solution was now coloured a bright yellow, as if it contained chromic acid or some such thing. The oxide was still and well Neg. to Platina—being heated in the Potash a few minutes it became more Neg.—boiled it well, still more Neg. Put in on the sand bath to boil some time. The solution became deeper and deeper in colour and the per oxide was still Neg. 20° perhaps. It looks as if it were really an active chemical agent continuing to give up something (oxygen) under the influence of the potash and not easily exhausted of its power.

5887. Put it to boil again. On the 21 Novr. was still Neg., but in a very small degree, to the Platina (5984).

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5888. *Plumbago; Platina and N. Acid.* The plumbago in N. A. of the 16th (5860) is still a little Neg. to Platina—most at first and soon falls to very little. Warming the Plumbago in the N. A. made it more *Neg.*—hotter was more Neg.—hotter still, still was more *Neg.*

5889. The N. A. had taken a little iron from the plumbago; but now put the plumbago into a flask and heated it in N. M. Acid for 15′. Turned the two pieces into water; took one out, washed, wiped and put it into strong M. A. It was powerfully Neg. to Platina. The other piece in the water was a very little Neg. to Platina—put a little acid in and it shewed more Negative—diluted the acid in which the first piece was and that became less Neg. than before. Put both pieces into the same acid and water. Each was then Neg. to the Platina wire, but one of them was powerfully Pos. to the other, the large being positive to the smaller.

5890. Broke a small piece off from each. The piece from the Positive was pos. to the piece from the Neg. Broke two fragments off from one piece and found one well Pos. to the other. So soon is this difference of Pos. and Neg. established and so easily is it shewn by plumbago in such an acid. Can the difference depend upon the solution in the pores of the plumbago?

5891. The two pieces of plumbago in the dilute Mur. acid, when lying one on the other and communicated through the galvanometer by the platina wires, gave a current, though one was pressed against the other with considerable force. So that such contact does not communicate the acting parts with each other so well as the wire of the Galvanometer.

5892. I put both pieces into pure strong M.A. in a flask on the sand bath, that the fluid within might be displaced by the simple Mur. acid, and so left them (5906).

5893. *Plumbago; Platina; Mur. acid.* The plumbago in Mur. acid of 16 Novr. (5857) was found at first contact (all being cold) a little Pos. to Platina, but the effect quickly sank to 0°, and then Pos. or Neg. state was not sensible either way. Washed, wiped and put the plumbago into fresh Mur. acid, but there was no action to platina. The needle was at 0°.

5894. This neutral plumbago, which has been 7 weeks or more in M.A., was put into distilled water and boiled. The water was removed and fresh put on, and this was repeated two or three times at intervals. The next day, the 19th Nov., this plumbago was the slightest degree Pos. in distilled water to Platina—in cold common water it was scarcely sensibly Pos. to the Platina plate—but in cold strong solution of pure Mur. acid it was freely and permanently pos. to the platina plate about 20°. In this case the Pos. Plumbago would have a weaker solution within it than that against the Neg. Platina.

5895. Put the plumbago and Mur. acid into a flask on the sand bath to see if it would become neutral in Muriatic acid. After an hour it was cooled and was still Pos. to the platina plate—much at the first introduction of the plate and less afterwards—scarcely at all after a while. On taking the platina out, washing, wiping and reintroducing it, it was again more Neg., and fell by degrees almost directly as before; but this effect on the platina was by no means so great in this case, where the plumbago is Pos., as it was found to be in another case where the plumbago was Neg., to be described hereafter (5909).

5896. Took some fresh slices of plumbago, placed them in a crucible and gradually raised their temperature *to full redness.* Kept them so for a time and then allowed them to cool.

5897. A piece of the *heated plumbago* (5896) in strong *pure Mur. acid* was neutral to Platina. Another piece of the same heated batch was also neutral in the same acid to the Platina. But when the two pieces were compared together, one was very strongly Positive to the other.

5898. I put the two pieces into a dilute solution of Mur. acid—there was no sensible difference between either and platina, but the one which was Pos. before was also well Pos. again when compared to the other. The effect is very striking.

5899. Took a piece of the heated plumbago (5896) and broke it in two. These were put at once into the same Muriatic acid in the same dish. Both were a little Negative to Platina wires, but fearing wire had hardly surface enough, I compared them to a platina plate of surface equal in the immersed part to their own. One was trace Pos. to the Platina plate. One was very *Pos.* to the other very *Neg.* Removed the Neg. Piece—then the Pos. piece was scarcely sensibly Pos. to the Platina plate. Put in a little piece of the Neg. one not nearly equal in surface to the platina plate and then the Pos. plumbago was well Pos. to it.

5900. So that with equal surfaces, or still more unfavourable proportions, the Plumbago could discharge the current of the Pos. Piece when the platina plate could not.

5901. Then on a sudden the little piece (just now neg.) became Pos. to the larger and formerly Pos. piece.

5902. Put another little piece of the first Neg. one in and it was neg. to the former little piece. So that even small pieces of the same portion of plumbago differ *from each other* more than plumbago does from Platina, and also change in their state in a very complicated way.

5903. All these effects were in pretty strong pure sol. of Muriatic acid, which both conducted well and acted quickly on the iron in the plumbago.

5904. After an hour, looked at these pieces in the M. A. They were a little and variously related to the Platina wire—but were strongly contrasted one with another, and more than before. They seem by rest to store up power, as if the platina wire by pressure broke through external films, and related internal films to each other, which could not otherwise be in communication—but then the current, which is strong at first, always goes down in strength, as if platina took up an opposing state.

5905. Still the plumbago present no constant permanent effect of *contact*.

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5906. Worked a while this morng. with the pieces of *plumbago* which were left to digest in Mur. acid (5892) on the 18th instant. The acid now shewed scarcely a trace of iron, but it contained fine acicular crystals of chloride of lead derived from the glass of the flask.

5907. They were transferred into fresh strong M. A., and being cold, were very nearly indifferent to each other; but both were well Neg. to a clean Platina plate at the first moment, and then the effect went off until the plate was removed, rewashed and restored.

5908. When Plumbago and platina were connected in the acid and 0°, if the platina plate were *more depressed*, then plumbago again was Neg. to it, which effect went off as before.

5909. If the neutral platina plate were taken out of the acid, washed, wiped on clean cloth and restored again, the plumbago was neg. though it remained unaltered in every circumstance in the mean time.

5910. If the neutral platina plate were washed and cleaned by *sand paper*, still it became Pos. when first restored to the acid in connection with the plumbago.

5911. It seems as if the platina had a power of producing a current by some change it suffered in the M. A., but that change (on its surface effected)[1], its power ceased, for the change could go no further.

[1] The sense seems to require the closing bracket after "surface."

5912. If the clean platina were put into the mur. acid, not touching or connected with the plumbago, it did not lose this power, for at the moment of touching the plumbago the momentary current was produced.

5913. If the clean platina in the Mur. acid were touched by other platina (as the wire of the galvanometer by which connection with the plumbago was made), still no current was produced, nor could its state of tension in the M. A. be discharged, for the moment the plumbago was brought into the circuit the current was produced and the Platina was Pos.

5914. So that the exciting force appears certainly to be in the platina, and the current is produced as the platina acquires a certain state, analogous perhaps to that of Iron in Nitric acid. The plumbago appears to be first a body having no chemical tendency for the chlorine of the M. A., and next a most excellent discharge; and so shews the production of this state and the current produced during its development.

5915. That Platina cannot discharge Platina of its first tense state, though Plumbago can, is quite consistent; their opposite tensions are equal.

5916. The current only can bring on the inactive state of the Platina.

5917. A platina wire which had been discharged by the Plumbago in the Mur. acid, and so made to assume its inactive state, was very slightly Neg. to fresh undischarged Platina, as if the latter had now its state of tension in excess; but the effect was not to be compared to that of plumbago in facilitating the current.

5918. Plumbago seems to *exert* no action and to have no opposed final state. It appears to offer an open door to the electrolyte for the transfer of any current, determined elsewhere, which tends to pass through it.

5920[1]. If the cause of the current is in the mutual action of the Mur. A. and Platina, which is enough to bring on a certain state in the Platina (the first step as it were of chemical action), then Gold in M. A. ought to be more powerful in relation to Plumbago than Platina has been; and yet should shew but little Pos. Power to Platina, because of its tension when undischarged and its

[1] 5919 is omitted in the MS.

imperfect power of communicating the current force to the electrolyte.

5921. So Gold and Platina in the M. A. in which the plumbago was. The Gold was just sensibly Pos. to the platina. But the Gold and Plumbago: now there was a strong current, and the Gold was far more Pos. to the Plumbago than the Platina had been; also its excess of power over the Platina was far greater than its effect when compared directly with Platina itself.

5922. If Plumbago is quite indifferent as a motor and only powerful as a discharger, then it ought to surpass also in discharging current in the reverse direction, i.e. where it would be in the relation of Positive. Put a little piece of per oxide of lead in dil. S. A. and made platina and plumbago in turn the opposite termination in the acid. There was good current in both cases, the platina or plumbago being Pos.—but I am persuaded the plumbago was best. It requires a more delicate current than the per oxide of lead gave.

5923. Plumbago will probably be an excellent developer of the state of Electro motive bodies.

5924. *Plumbago and Water.* Two pieces (different) of the heated plumbago (5896) were put into a basin with common water. The small piece was Neg. to Platina plate and the large piece was still more Neg. to the Platina plate. But when the pieces were compared together, there was far more effect than between the platina and the *most Negative piece*, the small piece being now Pos. and the large piece Neg. to each other. So common water shews the effect of plumbago.

5925. Two pieces of plumbago heated (5896), broken from the same piece, were put into distilled water, and being compared, there was very little difference or current sensible. Added a few drops of nitrate of soda and stirred all up. Now when they were compared, one was strongly Neg. and the other Pos., the needle standing at 30° or more. Both were the least trace Neg. to the Platina Plate. The effect was very striking and shewed clearly that the effect was not one of mere extent of external surface, for the platina plate was more than equal to them in its immersed part.

5926. Warmed the water and pieces and then they were in the least degree *Pos.* to the Platina plate, and differed very little from each other.

5927. When warmth is applied, must be careful about the difference of temperature in different parts; for Plumbago may be able to shew thermo electric effects though the metals hardly can.

5928. Into a basin containing warm water and platina, put a cold platina, and the latter appeared to be slightly Neg. to the former (5927); but must work this out carefully with Plumbago.

5929. Plumbago appears to act as a most ready discharger of currents to an electrolyte. But in relation to the generation of the current, Carbon might take oxygen and hydrogen easily, and if it can, then plumbago may help to generate either a Pos. or a Neg. state according to circumstances.

5930. Make a Ritter's secondary battery with Plumbago and see if it has any reaction.

5931. *Plumbago* and *Sulrt. Potash*. The Plumbago of 16 Novr. (5867) was examined in its solution and found to be still a little Pos. to the Platina on the morng. of the 18th. It was again examd. this morng., having remained on the sand bath all the while since in its solution: still well Pos. I put a platina wire round it and returned it in its solution to the sand bath for a few days (5951).

5932. It occurs to me that perhaps the plumbago, etc. were not all at one temperature, and that the difference may be due to a difference of this kind in the parts concerned. If plumbago can shew the thermo electric effect of electrolyte, then it is very possible some effect of this kind may be here. Especially as the Plumbago is *Pos*.

5933. A Piece of the heated plumbago (5896) was broken into two pieces and put at the same moment into a solution of sulrt. potassa. They were neutral to Platina—then in a few minutes one was Pos. to Platina, and whilst contact continued was the next moment Neg. to Platina. The other was Neg. a little to the Platina—but both gave an unsteady needle as if changes were going on within. When compared with each other, one was Pos. and the other Neg., with a much greater deflection of the needle than either had shewn to the Platina.

5934. It appears that either this sulphuretted potash solution or an acid solution (both being electrolytes which act on the plumbago) develope these differences of piece with piece far more than Potash does. See below (5935).

5935. *Heated Plumbago; Platina; Potash.* A piece of the heated plumbago ( ) was broken in two and put into a solution of potash (caustic); both were a little Neg. to Platina, either the wire or plate—only a little. When compared together, one was a little Pos., the other Neg.; but the difference was no ways comparable to the effect in acids or sulrt. potash, for now it was not greater than between either plumbago and the platina.

5936. Hence the effects in the acids, etc. may be effects due to the acid strata in the plumbago, to which the plumbago serves as a conductor, and not to the direct activity of the Plumbago as an electromotor.

5937. The theory of contact is not sustained by Plumbago in Potash, for one is Pos. and the other Neg. Contact, if any thing at all, should give consistent results.

5938. In illustration that reason why *Plumbago* will seem Neg. to a metal when Platina will not, is because it can discharge a current better, compared both with *Silver* in dilute Sulc. Acid; and found that silver appears very little Pos. to Platina in such acid, but very Pos. to Plumbago in the same acid, though Plat. and Plumbago differ very little in that acid.

5939. *Plumbago; Platina; N.A.* A piece of the *heated plumbago* (5896), was broken in two and put at once into strong Nitric acid —both were *Pos.* directly to Platina wire and plate. Still one was much more Pos. to the other than it had been to the platina plate of equal surface. So here the Plumbago had the *Pos.*, not the *Neg.* state, and the two pieces differed at once from each other. Against contact theory.

5940. *Plumbago in water.* Have boiled some slices of plumbago (Morden's) in common water in a flask (containing also a platina wire to assist discharge and assumption of a final state) for several hours. Then left all to become cold together.

5941. Being turned out into a Wedgwood's basin with the water, all were found a little Negative to a fresh platina plate (i.e., one just cleaned (5909)); but effect with all went down to 0° or nearly so.

5942. There were slight variations when the pieces were compared together, but not much.

5943. Took one of the pieces, broke it into two, put the two into strong Muriatic acid. There was very little difference between them, but both were Neg. to the Platina plate, and though more at first than afterwards, still a little Neg. permanently. On washing and wiping the platina, it became each time more Pos. to the plumbago.

5944. A third piece of plumbago from the water, on its first introduction into the Mur. acid, was well Pos. to the other two: it then shortly changed and became *Neg.* to them, and was then a little Neg., though scarcely sensibly so, to the platina plate. Then pieces 1 and 2 were powerfully Pos. to piece 3, though they and 3 also were nearly insensible to the Platina plate. On every repetition, 1 and 2 were very Pos. to 3, holding the Galvanometer needle nearly steady at 60° or 70°. 1 and 2 were also one Pos. to the other now a little, and all were acted upon by the acid, the iron appearing in the solution.

5945. The most Pos. of them was not sensibly less Neg. to the Platina plate than the most Negative one was.

5946. There was a storing up and an exhaustion of power even among the plumbagoes themselves, i.e. the first contact of 1 and 3 was more powerful than the continued contact. Still, a good current of 70° or so could be kept up for a minute or more and the effect probably depended upon the partial exhaustion of the film of acting fluid.

5947. Plumbago which is most Neg. to other plumbago is probably that which is best fitted to shew the effect of discharge with Platina, etc. etc., and is probably the most neutral and inactive, i.e., if the solution within its pores is the same as that without. Otherwise, that solution may be actively Neg. to the solution around.

5948. Had some plumbagoes in two different muriatic acids which, though strong, differed in strength, and one had more iron in it than the other. A piece in its acid was 0° to Platina plate. Another piece in the other acid was also 0° to the same platina plate. Put the first piece out of its acid into the other acid, and it was then well Pos. to the Platina plate and very Pos. to the Plumbago there.

5949. So that differences in the solutions are shewn to be very important.

5950. In a little while the piece which had been so Pos. at first became Neg. to the other piece, to which it had been Pos. and well so—and this second piece was rendered Negative to a piece to which it had before been neutral.

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5951. The Plumbago, Platina and Sulrt. Potash of 16 Novr. (5867, 5931) being examined, the Plumbago was still Pos. to the Platina, as if conversion of the iron in it to sulphuret was still going on.

5952. Decomposed a pretty strong solution of sulphuret of potassa by a small battery of 30 Pr. one inch plates, feebly charged, using Platina electrodes. Sulphur was evolved at the anode, and a little gas, probably hydrogen, at the cathode. The sulphur was very pure and, in appearance, abundant.

5953. Will Platina and sulphuret of Potassa form a secondary battery? Yes, for two platina wires, A and B, were put into a solution of the sulphuret; A was connected with the Positive and B with the Negative end of the small battery (5952) for about 10 seconds; they were then separated from the battery and connected with the wires of the galvanometer. Immediately a considerable current was produced, and that wire which had been made by the battery Positive to the other was, when connected by the galvanometer, Neg. to the other through the solution, and that constantly so, whichever way the wires were used. The expt. was very Good and striking.

5954. Iron and Nickel are Negative to Platina in solution of Sulphuret of Potassa (5269). What is the cause of this: contact or chemical action? (5980).

5955. Iron and Platina come ultimately to a state of rest (   ).

5956. *Iron; Platina; and sol. Sulrt. Potash* (5269). Used plates of the metals. The iron being ever so well cleaned by sand paper and clean cloths, etc., was always Neg. to the Platina on the first immersion, but when well cleaned soon went down and became 0°.

5957. An iron plate which was dim from rust formed by vapours of the laboratory in the course of the last day or two, was far more powerfully Neg. to the Platina, and the hase of rust became a haze of black sulphuret of Iron.

5958. I believe that the effect of clean iron is due to a film of oxide, and in this respect consider that iron is an exceedingly oxidizable metal. When in state of sponge, it fires spontaneously; with water and platina it gives an instant voltaic current—when out of sulphuret, if washed in water and wiped, it gives instant smell of hydrogen—if cleaned with sand paper and dipped in water, it gives instant smell of hydrogen; so that when exposed clean to the air, it is not wonderful if a film of oxide instantly forms on it; and it is probably the reduction of this oxide at the moment of contact with the sulphuret which renders the iron Negative.

5959. With respect to the power of Plumbago to conduct Neg. or reverse currents (   ), a clean *iron wire* in sulrt. potash was *not* sensibly more Neg. to Plumbago than to Platina—but the piece of plumbago was found to be *Pos.* to the Platina in the solution. Using the *Iron plate*, it was very sensible to the plumbago, but not in any very particular degree, as contrasted with Platina.

5960. *Nickel; Platina; Sulrt. Potassa* (5983). The Nickel was at first well *Neg.* to the Platina—it then fell very quickly to 2°. Then rewashed, wiped, etc., and was now Neg. only a little. Again washed, wiped, etc., and it was now less Neg. than last time. Now cleaned with sand paper and wiped dry (i.e. not having been wetted): it was more Neg. than last time, but not so much as the first.

5960½. This piece of Nickel is a small bar. It is not perfectly solid, but has little rough depressions and holes which I cannot file out, and these contain oxide, etc. etc., black matter from former experiments, etc. These probably are quite enough to produce all the effect by the oxide, etc. which they contain.

5961. Must remember that a plate in sulphuret, if taken out and exposed to air, is Neg. when put in again (5127, 5177, 5325). So these holes, when the matter in them is exposed to air, would by that exposure be Neg. to the platina when reimmersed.

5962. Some results with Plumbago, Platina, etc., using the Green hydrated Ns. acid as the Electrolyte (5353).

5963. *Platina; Platina; Green Ns. Acid.* It is impossible to clean two platina wires so well and so alike, though both were from the same piece of wire, as that they shall be 0° when first immersed; though all care be taken to immerse them together. The effect first produced is quickly at 0°, a balance of the conditions of each wire having taken place.

5964. If, when the two wires give 0° effect, one be raised not more than the $\frac{1}{8}$ of an inch and instantly lowered again, so that the part which had been for a moment in the air should be again reimmersed, that wire became strongly Neg. in consequence of the change of the acid on its surface ( ); so delicate is this electrolyte, in consequence of its good conducting power, etc. etc., in allowing currents to form and pass.

5965. *Plumbago; Platina; Green Ns. Acid* (5353). Made the circuit complete, dipping only one end of the piece of plumbago used into the acid. The plumbago was strongly Negative to the Platina—the force gradually diminished, but after some time 20° or 30° of deflection remained.

5966. Put the piece of plumbago quite into the acid, so as to cover it over and exclude the effect of air at the part where it passed the surface of the acid; and now touched it with one or other of the platina wires employed, so as to make it the electrode at one or other side of the surface at pleasure, the untouching platina being of course the other electrode. Now the plumbago was strongly Pos. to the Platina, having changed, as has happened before with the Nitric acid and the Muriatic acid. It continued Positive for all the succeeding experiments.

5967. That platina wire which has for a moment touched the Pos. Plumbago, when removed from it and placed by the side of the other wire (still in the acid), is strongly Neg. to the other, no matter which it was. Suppose the wires to be A and B: A is touching the Pos. Plumbago and B is consequently Neg., for it is the other electrode. Then removing A from the plumbago and putting it by B, B becomes for the moment powerfully Pos. and A is as powerfully Neg. All this depended simply on the circumstance that, to put a wire on the plumbago, it was raised a little, and when removed from it, it was depressed and so became Neg. (5964).

5968. *Per oxide Manganese; Platina; Green Ns. Acid* (5353). The per oxide is Neg. to the Platina. The manganese was disintegrated, and being left in the acid for a while, it converted it into Nitric acid, being itself reduced in its degree of oxidation; and then Iron and protoxide of manganese were found dissolved in the acid. All very consistent with chemical theory.

5969. *Per oxide Manganese; Plumbago; Potash.* The old preparation of Per oxide and potash was taken (5887) at common temperatures, and a piece of plumbago put into the solution. The Plumbago was *well Neg.* to Platina. The per oxide was a *little Neg.* to Platina. The Plumbago was *very Neg.* to the Per oxide, which was Positive.

5970. After a little while, all three became neutral to each other.

5971. After a while longer, the Plumbago was 0° to the Platina plate and was then *well Positive* to the per oxide (as if it did conduct a reverse current well); and this relative state continued.

5972. These changes are very remarkable, and it seems as if the previous currents had some strong effect in causing alterations in the relative states of the bodies. Must try this.

5973. *Plumbago; Platina; Sol. Sulc. Acid and chromate potash.* The plumbago was Neg. to the platina, and more if the solution was warm, but there was no remarkable or special action. Not more than in sol. of the acid or salt alone.

5974. Can such solid bodies as per oxide lead, per ox. manganese or sulphurets of some of the metals, be used in place of the electrolytes in constructing an active circuit?

5975*. *Zinc; Platina; Per oxide of lead* (   ). The per oxide was that which had been well washed, etc. When all were pressed together there was a current, Zinc being Pos. to the Platina. When left quiet, the current fell. When the zinc was moved so as to produce a grinding action, pressure being continued on *a*, then the current was renewed, but it could not be continued. On removing the plates, etc., the per oxide was found adhering considerably to the zinc, and the latter seemed a little tarnished.

5976. *Lead* was a little pos., not much, and was tarnished at place of action.

Cadmium—not sensible effect.

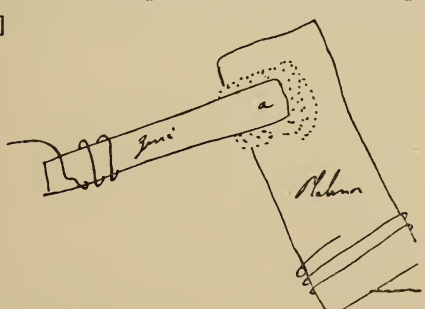
Iron—not sensible.

Zinc—produced the effect again.

Zinc with Powdered oxide of Manganese: no effect.

5977. As a piece of wet paper between zinc and platina produced

* [5975]



a powerful current, proceeded to dry the oxide in a basin. Found some moisture came off. Then put it hot between Zinc and platina. Now very little deflection and that as if Zinc were Neg.

5978. Put zinc and Plat. together and warmed place of contact: is as if Zinc Neg.

5979. So no exciting action of the per oxide of lead and no evidence it can act as an exciting electrolyte. Its neutrality when in such contact is surely against contact alone doing any thing.

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5980. Procured a clean piece of iron (5958), rubbed it over with sol. sulphuret of Potassa and put it into a quantity of solution so as entirely to cover it. It was now *Pos.* to a platina plate feebly. Whilst the contact was complete, rubbed it with a piece of wood under the surface, so as to expose a fresh surface to the solution. This did not make it neg., but the smallest degree more Pos. So that iron is not Neg. to sulphuret potash and Platina, except because of previous action of air, etc.

5981. A bar just clean from the Instrument Makers was actually Pos. in the first instance, but by soiling its surface and tending to oxidize it, could make it give a first effect of Neg.

5982. Different Pieces of iron differ, some inclining more than others to be Neg. at the first immersion; but none continue neg. if entirely under the solution, though rubbed.

5983. *Nickel* and *Platina* in *Sulrt. Potash* (5960). Put the nickel under the solution; rubbed it there. It was Pos. to the Platina—whilst in contact rubbed it; it was still Pos. a little. On taking it out it was found a little tarnished, as if sulphuret had formed on it. Then dipped it in water, washed, wiped and restored it, but now it was, as at first, strongly Neg. to the Platina. So that is evidently the consequence of a state taken by washing and exposure to air, and not a state due to contact with the sulphuret. So that difficulty is cleared away.

5984. *Potash; Platina; and Per ox. Manganese.* Reverted to the experiments and preparation of (5887). The potash was yellow. Examined it for Manganese: could not find any; only a little silica or alumina derived from the ore or the vessel.

5985. The per oxide has changed colour on the exterior, and when a portion of it was washed and dried, it had a greenish surface, and is evidently tarnished as if it had suffered from chemical action; but what is that action? Can it be partial reduction by some organic matter in the potash?

5986. Added a little alcohol to the solution of potassa and now the Per oxide was well Negative to the Platina plate, continuing even at 30° or 40°. So this is the explanation of the whole, and what the alcohol does here, the dust and other reducing matter which falls into the potash does there.

5987. The well crystallized Manganese shews a much better effect than the permeable amorphous kind. The dynamic action is probably better developed and determined.

5988. *Plumbago; Platina; Rhodium; and Mur. Acid.* Expected that Rhodium would have less action than Platina on Mur. acid, and so might prove neutral to Plumbago, which might be Neg. to platina. A piece of plumbago which had been digesting several days in Muriatic acid was taken and cooled with a little of its acid, the last being used as the electrolyte. Plumbago was Pos. well to Platina at the first contact, and the effect then went down nearly to 0°. Upon breaking and remaking contact, there was very little difference, but the Plumbago the least Pos.

5989. The Plumbago was still more Pos. to the Rhodium at first contact, but that went down nearly to 0°, and then on breaking and remaking contact the Plumbago was only a very little Pos. to the Rhodium, but more than to the Platina under the same circumstance.

5990. The Platina was a trace Pos. to the Rhodium. The effect of Fresh Platina to Rhodium is as nothing compared to the effect of either fresh Platina or Rhodium to the Plumbago.

5991. Hence it appears that Rhodium is more Neg. than Platina; but that it probably produces a first *pos.* effect, since it takes up the second or inert state. Must try this with Platina, which is Neg. to it instead of positive to it.

5992. The Piece of plumbago was turned into common water, and in it, it was a little Pos. to Platina in the water. Here the Plumbago was wet with strong acid and the platina with water, but the water is a bad conductor. The Plumbago was washed and wiped dry by a cloth; then put into the acid again, and now it was well Neg. to the Platina, so that the current was reversed with the states of the Platina and plumbago. The effect gradually went down, and in 5[1] the Plumbago was at 0°, or rather a trace Positive to the Platina.

5993. Conduction without decomposition of the electrolyte. Try to decompose by Gassiot's dry piles; if can not, then perhaps obtain proof of conduction without decomposition.

5994. *Voltaic pile*: reference to reservations, etc. Exp. Researches. Paragraphs: see note to 856; 875, 6, 7; note to 921; to 928; end of 960.

Opinion reserved on source of V. Electy. at 857, 872 par.

[1] Presumably 5 minutes.

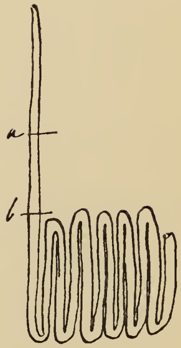
5995. The current of a single pr. antimony-bismuth element passed across five interruptions of strong solution of sulphuret of potassium (   ), the interruptions being by platina plates. The immersed part of the plates was about $1\frac{3}{4}$ inches by $\frac{3}{4}$ wide on an average.

5996. The same thermo current passed 5 similar interruptions made by platina across the dilute green nitric acid (   ) from Grove's experiments, and so freely that I think it would have passed 20 almost as well, and probably a hundred sensibly.

5997. Platina and Iron associated in dilute sol. sulphuret potassium, 1 vol. Strong sol. (   ) and 10 vols. water. Iron was at first Negative but in ten minutes it was quite neutral.

5998. Platina and Iron were associated in the hydrated green nitrous acid (   ) and were soon neutral. The thermo current excited at *x* travelled round the circuit moderately, but nothing like as if, for iron, oxide of iron or platina had been used. The chemical action of a little dilute Sulc. acid at *x* caused a current which passed powerfully.

5999. Oxidized iron plate and platina in red nitric acid strong. The acid contained some muriatic acid and was in so far bad. The iron was a clean wire, bent thus, and then blued in the spirit lamp flame, as Schonbein directs. Whilst this with Platina was in the acid, the iron ox. was Pos. at first 60°, but soon fell to 25°, and in 15′ was 20°, but part of this is due to the opposing state the platina takes up.



6000. When at 20°, the antimony-bismuth thermo current could pass well, raising the needle to 50°. This would not have happened had it been metallic iron instead of oxide. The chemical action of dil. S. A. at *x* gave an excellent current, passing in the opposite direction to the one tending to form in the nitric acid.

6001. Made the same arrangement, but used a dilute acid consisting of 1 vol. of above and 1 vol. water. The oxidized Iron was Pos. and rather strongly so, also irregular, the needle vibrating much. On examg. the iron, found that the oxide was dissolved away at the part cut by the surface of the acid at *a*, and metallic iron was acting there. Cut off this part down to *b*, oxidized the termination by heat, attached the scaled piece to a platina wire and replaced it, putting the whole under the surface. Now there was nothing like so much current, but the scale iron was still Positive and the needle at 20° *steady* in less than a minute.

6002. The thermo current at *x* passed very well indeed.

6003. The chemical current of iron and dil. S.A. at *x* passed powerfully.

6004. In 40′ the deflection was only 5° and steady and the thermo current was well conducted.

6005. I put a platina plate down into the acid before and against (i.e. in contact with) the platina plate to discharge its state; this caused the iron to seem negative for a moment, but I expect it also discharged the first platina plate of its state. Then taking out the second plate, the scale iron was pos. about 10° only.

6006. Boiled dilute solution of potash caustic, then cooled it. Iron and platina in it. At first immersion, iron was Positive to the platina, but soon fell much and nearly to 0°.

6007. Chemical action at *x* passed feebly—the action between the Iron and platina being by dilute Nitric acid.

6008. The following pairs of metals were arranged with dilute solution of potash at one end and dilute Nitric acid at the other, to ascertain whether the current it formed would pass the potash.

6009. Iron and silver: current did not pass sensibly.

Iron and Palladium
Iron and Gold
Iron and Platina
Iron and scale oxide Iron } the current passed feebly.

6010. Platina and rusty iron in dilute sol. of sulphuret of potassium. The Rusty iron strongly Negative.

6011. Oxide of Iron made into a paste with water and applied on a platina plate—dried well—then this made into a pair with clean platina in dil. sulphuret of potassm.: it was well negative.

6012. An Iron wire dipped into a solution of dilute nitric acid—shaken—left a few seconds—put into vapour of ammonia to throw down oxide—warmed to dry it—then washed well in water—dried again. This was well neg. to Platina in sol. sulrt. potassium.

6013. Fuzed pure *protoxide of lead* ground fine, made into a paste with water and applied uniformly on a platina plate, then dried by warmth and cooled. This with a platina plate into dil. sulrt. of potassium was well Negative, and the deflection increased as the coat was penetrated and the action went on nearer the substratum of platina. If left long enough the action came to an end.

6014. The cleaned platina plate was quite neutral to the other platina.

6015. The investing coat must be put on uniformly so as to cover the whole of the immersed part. If it be dabbed on so as [to] make little spots, one part of the platina being clear and the neighbouring part covered, then there is local action and no great current.

6016. When a platina plate similarly covered was connected with a clean platina in dilute Nitric acid, the first was strongly positive to the second, not negative, and no oxide was left.

6017. When a platina plate covered with *red lead* was in dil. N. A. in connection with a clean platina plate, the red lead was strongly Negative, as in Sulphuret of potassm. because it was now giving off oxygen. When in Sulrt. potassm. the red lead was Neg. and became sulphuret of lead.

6018. When a platina plate with per ox. lead was in dil. N. A. as before, the Per oxide was *very Negative*. In dilute solution of sulphuret of potassium it was also Negative.

6019. Carbonate of lead on platina plate to clean plat. plate in dil. sol. sulrt. potassm. Carbonate was Negative and deflection increased as above for a while.

6020. Carbonate of lead on platina plate in dil. N. Acid was Positive to clean platina plate.

6021. Per oxide of lead in solution of caustic potash was very negative at first to clean platina—the force of the current quickly fell, and then adding a little alcohol or a little sugar did not sensibly increase the current. The effect is probably quickly produced at the surface of contact between the per oxide and the alkali.

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6022. Iron and platina plates arranged in strong sol. sulrt. potassium. Iron strongly Negative 30° or more. In 10′ it was at 4°.

Then separated the platina plate, put another down into the solution touching it so as to discharge the state of the first platina plate, removed the second plate and at the 20′ from the commencement of the experiment rejoined the platina and iron plates. The iron was Neg. only 3°. The thermo current of Antmy. and bismuth passed so fairly as to make the 3° equal to 18°, so that to 15° of thermo current passed.

6023. Iron and platina plates in a strong solution of caustic potassa (potassa fusa). The iron was Pos. at first considerably, perhaps 40°. In 20′ it was Pos. 10°, in 100′ it was 2°. Separated the platina and discharged its state as above. Then reconnected it with the iron: the latter was Pos. about 8°, falling quickly to 4°. So that Platina, being Neg., had acquired a reacting state in the sol. of potash. The antimony bismuth thermo current did pass, but very poorly, both the iron and the solution of potash against it—scarcely sensible.

6024. Made a fresh solution of proto sulrt. potassm. by passing sul. hy. through sol. potash. By dil. S. Acid the solution gave much sul: hydrogen and very little sulphur. It tasted somewhat and I think must have been pretty good. It has been going on from the eveng. of the 11th until this morning, or 40 hours.

6025. *Lead and Platina* in this proto sulrt. potassm. Lead well Positive about 40°; quickly fell to 10° and then seemed to remain steadily there. It does not go down so beautifully as in the yellow sulphuret.

6026. *Bismuth and platina* in the proto sulrt. potassm. The Bismuth was at first well Positive, but after about 20′, nearly at 0°, so fell more completely than the lead.

6027. The Negative platina assumes rather a strong reacting state in this solution.

6028. Two platinas in, not alike: difficult to make them alike. Scraped and cleaned one well—then came nearly together—all the difference was due to a little insensible matter lodged in a crevice near the end.

6029. On taking out one and reintroducing it from the air, it is very slightly Negative to the one which remains in, but the difference is not very sensible. Taking one out, washing, drying and reintroducing it, made it a little Negative to the other.

6030. Action by production of *iodides.*
6031. *Lead and Platina* in sol. iodide potassm. Lead is well Positive and an insoluble iodide of lead forms on it, but not as an investing crust, for it wipes off.
6032. *Cadmium and platina* in sol. iodide pm. Cadmium very positive.
6033. *Cadmium and platina* in sol. *iodide of zinc.*
6034. *Lead and platina* in iodide zinc—lead very Pos.—continues so. The iodide of lead formed is rough; part wipes off, part adheres.
6035. *Silver and platina in iodide zinc.* Silver very pos. indeed and sharply so—the power falls slowly—silver becomes coated with iodide.
6036. *Silver and Lead* in iodide zinc. Lead Pos. to the silver.

Action by production of chlorides.

6037. *Silver and platina* in Mur. acid, 1 vol. sol. strong, 1 vol. water. The silver was strongly positive. The current diminishes and in 10′ was about 8°. Put in a second plate of platina to the first in the acid. The Positive state of the silver rose instantly, shewing the reacting power acquired by the first platina plate.
6038. Native Carb. Iron—argillaceous Iron ore applied as paste to a platina plate, dried, etc. and then associated with platina in dilute sulrt. potassm. The carbonate was Neg., but not strongly.
6039. Per oxide of lead in strong solution of caustic potassa: part soon dissolves and lead is found in solution.
6040. Dilute Nitric acid, 1 vol. strong and 5 vols. water, had some per oxide of lead put into it; in $1\frac{1}{2}$ hours a very sensible portion had dissolved, and sul. hydrogen shewed it in the acid, though it could shew none in a portion of the acid preserved for comparison and in which per oxide had not been put.

6041. The following is the order of ten metals in seven solutions ascertained by re-experimenting to-day.

i	ii	iii	iv	v	vi	vii
dil. N.A. 1 vol. strong Acid 7 vols. water	dil. S.A. 1 vol. Oil V. 13 vols. water	M.A. 1 vol. strong sol. 1 vol. water	strong Nitric acid pure	strong sol. caustic potassa	dilute colourless Hydro Sulrt.	yellow Sulrt. Potass. 1 vol. strg. sol. 5 vols. water
1 Silver	1 Silver	3 Antimony	5 Nickel	1 Silver	6 Iron	6 Iron
2 Copper	2 Copper	1 Silver	1 Silver	5 Nickel	5 Nickel	5 Nickel
3 Antimony	3 Antimony	5 Nickel	3 Antimony	2 Copper	4 Bismuth	4 Bismuth
4 Bismuth	4 Bismuth	4 Bismuth	2 Copper	6 Iron	8 Lead	3 Antimony
5 Nickel	5 Nickel	2 Copper	4 Bismuth	4 Bismuth	1 Silver	8 Lead
6 Iron	6 Iron	6 Iron	6 Iron	8 Lead	3 Antimony	1 Silver
7 Tin	8 Lead	8 Lead	7 Tin	3 Antimony	7 Tin	7 Tin
8 Lead	7 Tin	7 Tin	8 Lead	9 Cadmium	2 Copper	9 Cadmium
9 Cadmium	9 Cadmium	9 Cadmium	10 Zinc	7 Tin	10 Zinc	2 Copper
10 Zinc	10 Zinc	10 Zinc	9 Cadmium	10 Zinc	9 Cadmium	10 Zinc

The bismuth and antimony inversions are very striking in iii, v, vi, viii[1], and so are many others. In iv the first moment of action will perhaps put zinc pos. to cadmium. In i it is doubtful whether bismuth or Nickel is uppermost.

[1] vii is evidently intended.

6042. *Nature of Electrolyte.*
Becquerel's acid and alkali battery is probably connected with Daniell's results on electrolysis of sulphates. Water playing a part.

6043. So also prot. oxide of lead on platina plate is Pos. to the Nitric acid dilute in which it is dissolving.

6044. How would Daniell's results work out if pursued with such a salt as the per sulphate of copper, when the oxide is supposed to be a per oxide.

6045. *Iron and platina* in strong solution of pure potassa. Iron is Pos., but quickly falls, quicker than in dilute solution of the same potassa. Platina assumes reacting state rather strongly, so iron seems to fall faster. Iron continues Pos. to perhaps 8° or 5° for an hour or more when freed from back action of platina. The arrangement conducts chemical action at *x*.

6046. *Nickel and platina* very like iron and platina in all respects in this strong sol. of potash. Chem. action passes, etc.

6047. *Silver and platina* in this strong solution of potassa: scarcely any motion—at 0° very nearly from the first.

6048. Used dilute solution consisting of 1 vol. above sol. potash and 6 vols. of water. This dil. sol. pure potash.

Iron and platina „ palladium „ Gold „ Silver	The iron was pos. and more permanently I think than in the strong solution. Perhaps the air dissolved in the diluting water may help this action.
Nickel and Platina „ Silver „ Palladium	Nickel Pos.—it falls soon—washing and wiping does not restore it, but sand paper cleaning always makes it Pos. again—so a film of oxide is formed, etc.

Copper and silver—Copper Pos. and tarnishes.

Silver—Platina Silver—Palladium	Silver very nearly neutral, not quite. Is a little Pos. at first.

6049. Iron wire scaled by spirit lamp is Pos. to platina in dil. sol. Potash and steadily so at first. I think the solution penetrates the crust.

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6050. Iron wire oxidized in spirit lamp flame and platina in strong pure pale nitric acid. The scale iron quite under the surface as before (   ). Oxide film is Pos. at first; the action falls and after an hour is very small, but still there. Thermo current scarcely passes. Chemical action at x passes current well.

6051. Same experiment with dilute Nitric acid, 1 vol. of the strong pure acid and 4 vols. of water. Used the same iron-scaled wire. There was the least possible deflection, almost at 0°, but the Iron a trace Pos. The thermo current passed very feeble, but the chemical current at x very well.

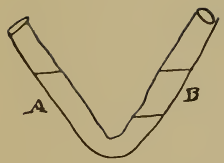
6052. When two platina wires are in the strong pure Nitric acid and at 0°, if one be taken out, washed and wiped, it is hardly possible to wipe it so dry (the wires had been sand paper cleaned) that, when immersed, it is not for the moment positive strongly. Yet if, being wiped, it is warmed in the lamp to dry it perfectly, then cooled and immersed, it will be quite passive. So easily does the needle shew the current due to state of surface of the metals.

6053. Iron and Platina in the solution of proto sulphuret of potassium. The iron at first well Negative; then quickly neutral and then went on to be Pos., rising to about 10°. This change occurred in about 5′. The first state is produced by deoxidation of the iron, but the sulphuret is I think quickly dissolved and then sulphuration of the iron and solution of sulphuret goes on. The solution partakes of potash characters.

6054. Chemical action at x made current pass well. So also did thermo current pass, because though one metal iron, yet large plates were used.

6055. Nickel and platina in the proto sulphuret of potassium. The nickel was Positive from the first, and though the current fell, yet it continued slightly positive all the time.

6056. Thermo action did not pass sensibly, but chemical action did well. The metal plate was small compared to the former iron plate.



6057. The tube contained strong sol. of yellow sulphuret of potassium in A and dilute solution (1 vol. strong, 5 vols. water) in B.

6058. *Platina and Platina* in A and B. After first irregular action, settled quickly to the platina in A being about 2° Pos. to the Platina in B.

6059. *Iron and Iron* in A and B. After first action, settled to iron in A being about 2° Pos. to iron in B. Changed the irons about, but was same thing.

6060. *Platina and Iron.* After first action of iron, the Platina in A was as nearly as might be 2° Pos. to the Iron in B.

6061. *Iron and Platina.* Still the same result, i.e. the iron in A was about 2° Pos. to the Platina in B.

6062. So quite clear that contact of Platina and Iron went for nothing; and strong and weak went for nothing, except the small effect of 2°, and this I think due to the gradual combination of the strong and the weak solution. Is in fact constant in direction only for that circumstance, and the nature of the metals go for nothing.

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6063. Iron in green hydrated Ns. Acid. Clean iron and platina wires. Iron first very Pos., then neutral nearly—though platina is discharged.

6064. Iron wire scaled by heat with Platina in the same acid: first a little Pos., then neutral nearly.

6065. Clean iron in strong solution of pure potash—was considerably and constantly Positive. Perhaps due to air. Used a weaker solution of potash; the iron was still positive—added a little drop of sulphuret of potash to remove the oxygen—still Iron was positive—added a little more, yet iron pos.—more still and still iron was positive.

6066. Scaled iron wire in strong sol. of potash—iron pos. and continued so. In dilute sol. of potash it was still somewhat positive. Added a single drop of sulphuret to remove air and now the scale iron was powerfully *negative*. Is in striking contrast with the iron, where the sulphur does not reduce an oxide but combines with a metal.

6067. Clean iron plate in strong sol. of sulphuret of potassm.: after 14′ put in a platina plate and connected: the iron was about 2° Neg. In 7′ more put in a large platina plate not connected; in 5′ more connected them and there was the least possible deflection, the Iron being a trace Neg. Hence no reaction back of platina here and iron neutral.

6068. A weaker solution consisting of 1 vol. strong and 6 vols.

of water was made; the same iron put into it and left a few minutes. Then connected with platina and it, the iron, was a little pos.—going down if contact continued, partly from accumulation of force at the platina. On breaking contact a while and then remaking, it was again Pos. Took the iron out, washed, wiped and dried it and replaced it: it was then well Neg.—but in 4 or 5 seconds was again pos. The Pos. is no doubt a sulphuration of the iron.

6069. Made a very weak solution of sulphuret of potash. The iron was Pos., and if washed and wiped, it was positive from the first. But here the iron quickly blackened in places, from the formation of sulphuret upon it.

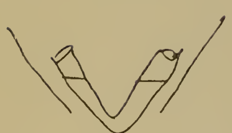
6070. Rusty Iron was Negative in this weak solution.

6071. Nickel in strong solution of sulphuret of potash was Pos. to a platina plate. In 5′ it was the least degree negative to a large platina plate. So the first action was from matter in the interstices. At 8′ after, it was quite neutral. When taken out, it has a yellow tint on it, as if an investing coat had been formed of sulphuret.

6072. Prot. oxide of lead on a platina plate in moderate muriatic acid is a little Pos. to Platina.

6073. Prot. ox. lead on platina plate gave no motion of needle by association with clean platina in dil. S. A.

6074. Plat. Plate dipped in potash is Pos. when put into dil. N. A. in which another plate is present.



6075. *Tin and dilute S. A.* Acid 1 vol. O. V., 13 vols. water. Put the two clean tins in in succession. The last in is always *Neg.* Taking one out, cleaning and restoring it, it is Neg. If both in and action equal, moving one makes it neg. So that the coat which forms at the tin increases the final current. This may be from the first salt of tin produced tending to acquire some other state, as an acid salt, and so adding to the force of the current.

6076. With Mur. acid, the first in was also Positive, not the last. Moving one about made a difference, but not as with Sulphuric acid.

6077. With dilute Nitric acid, weak and very weak, the *last* in was Positive—also the one moved in the solution was Positive. So this acid not as the Sulphuric or the muriatic.

6078. Examd. the Iron and Platina filings of 26 Octr. (5359). The iron was rusted, the surface being covered with a net work of rust and the intermediate parts being bright. This net work had *no* reference to the platina filings; for the latter were sometimes on a bright part and sometimes on rust, but the form of the rust no ways disturbed by them.

6079. In reference to the Negative deflection by heat of several cases, as 5394, 6, 428, 40, 1, 70, ascertained, if two pieces of the same metal were taken and one piece plunged in first, which, if there was any difference, was positive.

6080. Used first dilute N.A. of 1 vol. N.A. and 9 vols. water.

6081. *Lead; Lead; Dil. N.A.* (5394, 470). The one in first by a few seconds is *positive* to the one in last, but there is so much difference between the two wires that a particular one always became pos. in the end.

6082. *Zinc; Zinc; dil. N.A.* (5396). Whichever is in first is Positive to the other—distinctly well indeed. Can this effect depend in part on the circumstance that the one which is in first is well cleaned—or on that of a storing up of force accumulated during a moment by a state of tension assumed by the first immersed wire?

6083. *Copper; Copper; dil. N.A.* The one first in was the least trace Positive to the other—but nothing like in the degree of zinc or lead.

6084. *Iron; Iron; dil. N.A.* The one in *last* is Positive—regularly; but only for a moment.

6085. *Copper; Copper; strong sol. potash* (5428). No difference.

6086. *Lead; Lead; strong sol. Sulrt. pot.* (5440). No difference if one was in quickly after the other; but if one in first by a minute or more, then the last in was *Pos.*

6087. *Zinc; Zinc; strong sol. sulrt. pot.* (5441). The last in was a very little Positive and only for a moment.

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6088. In relation to dynamic induction (see Researches 1660, etc.).

6089. For the present let P——→ or ←——P represent a primary, i.e. a common voltaic or Leyden current; let S——→ or ←——S represent a secondary effect, and let T——→ or ←——T represent a tertiary effect; and let S—⊖— or —⊖—S represent the supposed state of the secondary wire after S——→ or ←——S has occurred. Then are not the following propositions true?

6090. It is clear that whilst a primary current is *rising*, thus P——→, a short current or rather a demi current ←——S is produced—that whilst P——→ continues, S is —⊖—S or apparently nothing, and that when P——→ ceases, —⊖—S passes into S——→.

6091. The cycle of conditions in the primary wire is: the natural state; the rising of the current P——→; the continuance of P——→; the fall of P——→; the natural state, etc. The corresponding cycle of conditions in the secondary wire is: the natural state; ←——S; then —⊖—S; then S——→; then the natural state.

6092. Now is it possible that P wire natural or —P—, and ←——P and also P——→, can be exactly alike in their action on —S—, producing not the least difference of effect on or in it?

6093. P——→ and ←——P are magnetic; —P— is not.

6094. As —S— approaches P——→ or ←——P, it is acted upon by them and becomes more and more ←——S or S——→, and as it receeds and is relieved from this influence, it returns more and more nearly to its natural state —S—.

6095. So it would seem that —⊖—S or S—⊖— is in a peculiar state of tension and not in the natural state; and that these changes with distance are the rising and falling of the state by such alteration of distance.

6096. But then how is this related to the effects with magnetic curves, for when a wire is taken towards a horseshoe magnet so as to pass between its poles, S——→ is produced; but when it has passed through and is receeding to a distance, still S——→ continues? Now the state of that wire at the first and last must be the same surely, and the neutral state; it can hardly be supposed to be —S— at one time and then afterwards S—⊖—.

6097. Especially as, by making it go round the pole, one could make the S⟶ state continue for any length of time, and this would imply an enormous accumulation of S–⊖– tension; and yet that wire might be removed without any return through the corresponding ⟵S state and would in no respect differ from a common wire.

6098. Surely this would imply that S⟶ or ⟵S are discharges of a certain state which the metal wire, *being a conductor*, cannot retain; but which, if it were a non conductor, it probably would retain. This would, as on former occasions, direct one's views to the surrounding insulating dielectric. But at present go on to view the action as one at a distance and exerted solely between the conducting bodies.

6099. That the wire S–⊖– has a state seems to be shewn by this, that the moment the ⟵P which sustains it ceases, an ⟵S is produced.

6100. The ⟵S and S⟶ currents must not be confounded with ⟵P and P⟶ currents. They are in no way similar. A ⟵P current, however short it is, produces S⟶ as it rises and ⟵S as it ceases, and nothing during its continuation. On the contrary, an ⟵S produces T⟶ during its continuation; but it cannot be said to produce either ⟵T or T⟶ as it rises and it certainly produces no current of any kind as it ceases. (See 6187, etc.)

6101. S⟶ and ⟵S, as also T⟶ and ⟵T, etc., are only the ends or elements of currents, and have not the full properties of complete or continuous currents, and must be carefully distinguished from them at present.

6102. But then magneto electric currents ought to have very different properties from ordinary voltaic currents, and so very probably they have. This an important matter for research; for a magneto electric ⟶ should continue to produce an ⟵S, whereas a voltaic ⟶ does not, but only an effect of that kind at its commencement or increase.

6103. Work this out (see 6187, etc.).

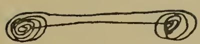
6104. If a difference, then follow that into other forms of current: Thermo, etc. etc. etc.; Friction, etc.; Electrolytic, etc.

6105. A complete current is equivalent to S⟶ and ⟵S and a certain duration of the supposed S—⊖— state. But an S——→ is only equivalent to an ←——S and no —⊖— tonic state.

6106. Approaching a wire to a P——→ or a ←——P is equal to an ←——S or an S——→, i.e. is equal to the rising of the P——→ or ←——P current. So receeding from P——→ or ←——P is equal to the falling of these currents in effect.

6107. Still the question is, has the approached wire an electrotonic state, or is it in a discharged and indifferent state? If it has an electrotonic, that state may be represented as before by —⊖—S or S—⊖—.

6108. S—⊖— seems to be in an electrotonic state because, when the P wire is either removed or its current stopped, it produces an ←——S; so that whilst as S—⊖— it neither carried a current nor yet was indifferent, it was not as P——→ or as P natural, i.e. —P—.



6109. But then supposed S—⊖— lengthened out as in Henry's experiments and that one half of it (that away from P——→) were removed and carried to a distance. It could produce no effects and nothing particular would happen either with it or the left half. How then can the S wire be in a tonic state? Surely it is the medium around which is tonic; the conducting matter is relieved, the wires being only places where, because of their nature, discharge occurs. Then both may be in dependant condition on P——→, but the one discharged whilst the other is sustained.

6110. If so, what is the state of the S insulating dielectric and what are its qualities?

6111. When a wire S has been brought alongside a P——→ and had an ←——S produced in it, another wire brought up either before or beside or even behind the first S wire, or placed any where else near P——→, will have its ←——S produced with little or no interference. (But see Henry's screening effects connected with this point and also my non screening results).

6112. Will masses of metal placed in any way as circuits or otherwise affect the inducing power of P——→ or ←——S?

6113. And must remember here that these lines of inductive force as given by a magnet do not seem to differ in the least through conducting or non conducting matter provided both be still.

6114. But if the electrotonic state were discharged in the metal and sustained in the insulating matter, and if the action were carried on by contiguous particles, how could it be a matter of indifference whether the space was filled up by a metal or an insulator?

6115. That S——→ or ←——S are not currents is shewn by their being active during their whole continuance and to their full amount, whereas currents are not active at all for their continuance. By their producing ←——T or T——→, whereas currents produce nothing. By their producing only ←——T or T——→, whereas a current began and finished produces both ←——S and S——→.

6116. Any increase of a current is equivalent to either a beginning or exaltation of state; and decrease is equivalent to the contrary. This shews that the continuous current is a very different thing to the *change* of degree or state of that current.

6117. Any approximation of the S wire to a current P——→ is equivalent to an increase in the force of that current, or the addition of the beginning of another current; any removal is equivalent to the contrary change of the P——→. This is a very simple and natural relation, and shews that it is not the current as current which produces any of the secondary effects—except the electrotonic or quiescent state if it exist.

6118. When currents are placed side by side, as thus ←——P over P——→, or thus ←——P over ←——P, then we have the effect of two interfering electrotonic states (or their equivalent states), either exerted in the same or in contrary directions; and we learn by the facts that Magnetic attraction and repulsion is the consequence.

6119. So we ought to trace, in attracting and repelling magnetic poles, the full and consistent effects of the supposed electrotonic states.

6120. Then remember these take place just as well through metals as through air or shell lac.

6121. In Magnets the Electrotonic states are sustained. What does this lead to in simple helices?

in soft iron cores?

in mere ordinary magnets?

6122. When the S currents, or rather halves, act like complete currents, as in deflecting the needle, electrolyzation, etc. etc., what is the amount of the effect? It ought not to be the same both in *kind* and in *quantity*. Perhaps its amount is only a half.

6123. Will an S⟶, whilst producing the same continuous effect as a P⟶, also continue to induce a corresponding ⟵T. The P⟶ used *will not* induce a corresponding ⟵S whilst it continues.

6124. Must make out the true relation of static induction and the electrotonic state—and the direction of the forces in that relation.

6125. Usually there is no corresponding effect at the rising of a current like that at the conclusion. But probably produce it by beginning the discharge by an electric spark.

6126. The cause of the non-effect at rising is perhaps because the action is expended in retarding the primary current itself—but why?

6127. Follow out this in the action of a long wire carrying a current, and also into effect of beginning spark in cases of Gassiot, Crosse, etc.

6128. The inductive result is the experimental analysis of a voltaic current or an electric current, and these two, S⟶ and ⟵S, appear to be the elements of the current; then a current will be proved to be a compound action; and then the two elements will be important in the consideration of all inductive and electrical phenomena, and especially in all those of discharge.

6129. A current appears most clearly to be a compound effect.

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6130. If P⟶ differs so much from S⟶, does S⟶ and ⟵S differ from each other except in direction? i.e. would ⟵S turned round make S⟶ or no?

6131. Perhaps not, if ⟵S and S⟶ have any relation to the places of separation of oxygen, hydrogen, etc.

6132. Call the rising S⟶ or ⟵S, $S\xrightarrow{1}$ and $\xleftarrow{1}S$; and the falling S⟶ or ⟵S, $S\xrightarrow{2}$ and $\xleftarrow{2}S$. Then a P⟶ on rising would produce $\xleftarrow{1}S$, and on falling an $S\xrightarrow{2}$; so a ⟵P on rising would produce an $S\xrightarrow{1}$ and on falling an $\xleftarrow{2}S$.

6133. Then if there be these currents, what curious association of these with each other and with $P\longrightarrow$ or $\longleftarrow P$ may be made, even in the *same* carrying wire? and must be examined.

6134. What will $\begin{array}{l} S\xrightarrow{1} \\ S\xrightarrow{2} \end{array}$ in the same wire do?

6135. What $\begin{array}{r} S\xrightarrow{1} \\ \xleftarrow{2}S \end{array}$?

6136. Will $\begin{array}{l} S\xrightarrow{1} \\ S\xrightarrow{1} \end{array}$ make only a stronger $S\xrightarrow{1}$?

6137. What will $\begin{array}{r} S\xrightarrow{2} \\ \xleftarrow{2}S \end{array}$ do?

6138. What $\begin{array}{r} S\xrightarrow{1} \\ \xleftarrow{2}S \end{array}$?

6139. What $\begin{array}{r} S\xrightarrow{2} \\ \xleftarrow{1}S \end{array}$? Not the same as the last.

6140. Is $S\xrightarrow{1}$ and $S\xrightarrow{2}$ alike?

6141. What will $\begin{array}{l} P\longrightarrow \\ S\xrightarrow{1} \end{array}$ do?

6142. What $\begin{array}{l} P\longrightarrow \\ S\xrightarrow{2} \end{array}$?

with a great many other changes.

6143. Surely it is impossible the distinctions can exist. The greatness of the consequences almost forbid it. Still, if Henry be right and if my first papers be right, there must be some difference between a principal and an induced current. See (6187).

6144. Will $\begin{array}{r} S\xrightarrow{1} \\ \xleftarrow{2}S \end{array}$ make an ordinary current? They ought. If so, what will be its direction, $P\longrightarrow$ or $\longleftarrow P$? and what decides the direction?

6145. But $P\longrightarrow$ and $\longleftarrow P$ make nothing.

6146. What particularly will $\begin{array}{r} S\xrightarrow{1} \\ \xleftarrow{1}S \end{array}$ make?

6147. Also $\begin{array}{r} S\xrightarrow{2} \\ \xleftarrow{2}S \end{array}$?

6148. And also $\begin{array}{l} S\xrightarrow{1} \\ S\xrightarrow{2} \end{array}$?

6149. Turn a current round in the wire, so as to traverse in its course thus, using a secondary current, not a primary.

See 6187, etc.

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.

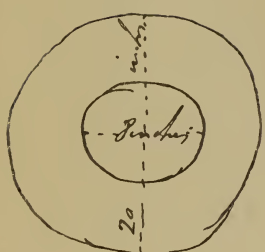
6151. Whether the iron was in or out of H made no difference.

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.



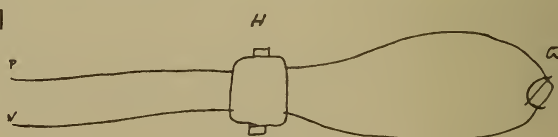
6155. Have arranged to examine the inductive action of an induced *current*. I arranged a powerful Electromagnet. Then I set my revolving copper wheel at work and carried its current round a large coil of Henry's; and then induced by that upon another coil, whose current circulated round a delicate galvanometer. The arrangement as seen nearly in plan was as below†.

D, a Daniell's battery of 3 cells, 18 inches high.

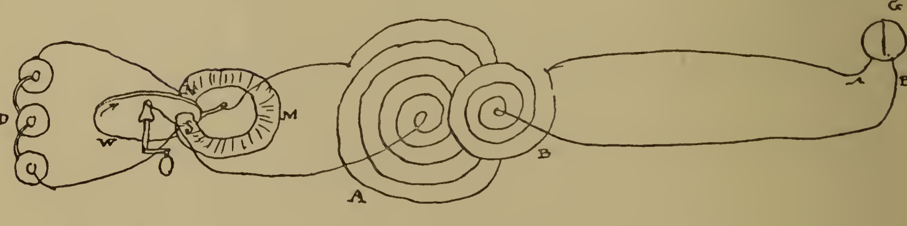
M, own large ring Electromagnet: as connected, N and S now the north and south ends. This includes the Primary current.

W, my revolving copper wheel, with collectors at the axis and periphery, passing away to Gassiot's large flat coil A.

* [6150]



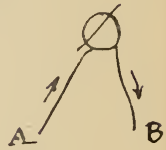
† [6155]



A, Gassiot's coil of 300 feet of copper plate $1\frac{1}{4}$ 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 $12\frac{1}{2}$ 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.

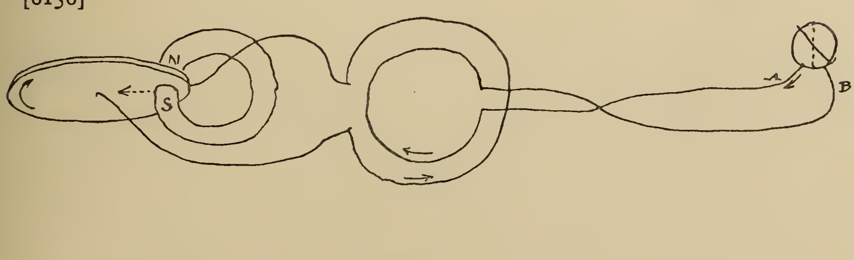
6159. Then it was to be expected that the secondary current would induce at its beginning and its ending, like other currents, and in contrary directions; and this was found to be true; for keeping the wheel in continual direct revolution and making and breaking contact at the periphery of the wheel, 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.

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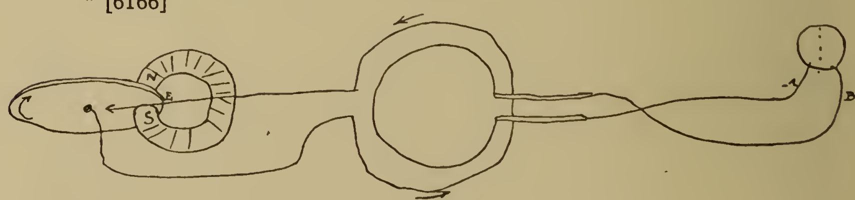
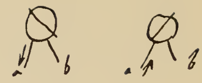
6166*. The arrangement was as before and as above[1]. The direction of the permanent induced current when the motion of the copper wheel was continued is as marked by the arrows above[1].

[1] i.e. in diagram below.

* [6166]

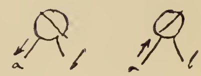


6167. When the wheel revolved direct, the Galvanometer was thus. When wheel revolved reverse, thus. This agrees with the former observation (   ).



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.

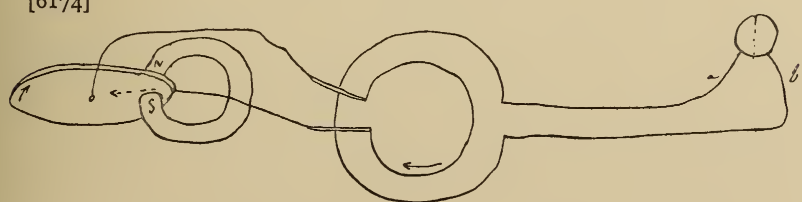
6171. Indeed, such making and breaking of contact ought to do nothing, for then the radius of the wheel and the magnetic curves do not move *across* each other, but the curves are projected end ways, so to speak, through the radius. It is as if a wire were brought *right up* to a magnetic pole and not 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.

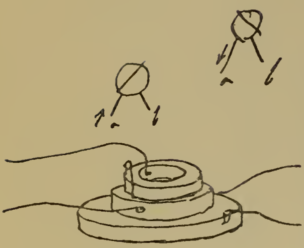
6173. To prove the great point that no current induces during its continuance, I connected the Daniell's battery with the first coil (Gassiot's), and though the secondary current at making contact was very powerful, yet the *continuance* of the primary contact gave no effect at all.

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].



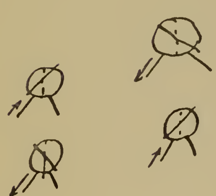
The effect was about the same as in the former case. The direct motion of the wheel caused deflection thus, and the reverse motion thus. So that here, on making direct revolution, the secondary and tertiary currents were, as before, in opposite directions.

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.

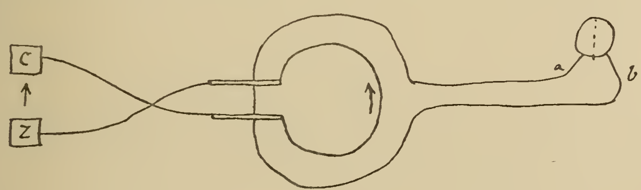
6181†. Put in our great cylindrical helix of thick wire so that one of the helices in it should, with the Gassiot coil, form the secondary current circuit, and the other helix in it with the Galvanometer form the tertiary current circuit. But whether the iron core was in it or not, I could not perceive 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.

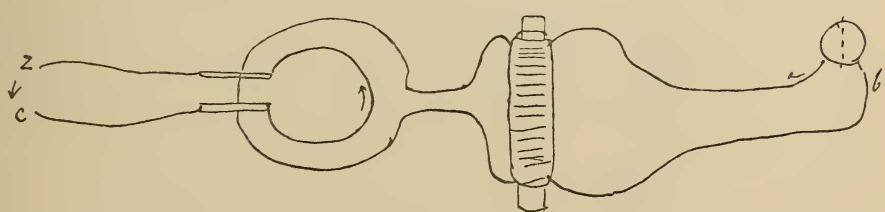
6183. There is I think great power lost at each inductive step.

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.

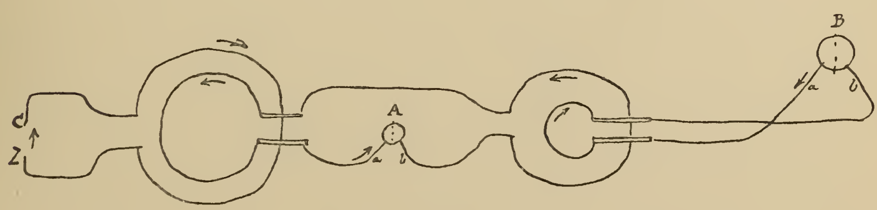
* [6180]



† [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, $1\frac{1}{4}$ 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 $1\frac{1}{2}$ inches wide, in a coil $12\frac{1}{2}$ inches external diameter and 8 inches internal diameter; the copper ribbon covered with cotton.

6190. *Solly's coil:* copper ribbon $1\frac{1}{2}$ inches wide, covered with cotton, made into a coil $9\frac{3}{4}$ inches external diameter and $7\frac{3}{4}$ 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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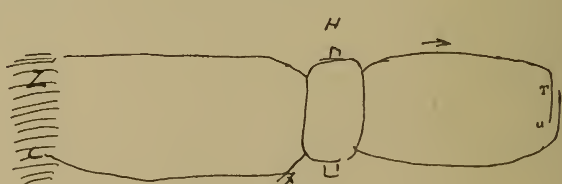
6193*. Gassiot made an experiment here to-day to shew me that the induced current always went in a peculiar direction and against expectation across air under certain circumstances.

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 $\frac{1}{20}$ 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.

1 JUNE 1842.

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.

2 JUNE 1842.

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, plumbago, 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.

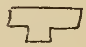
6225. When boiler was uninsulated, still metal of rod gave strong *Neg.* when held in the stream.
6226. Worked from 10 until 2 ocl'k., about 4 hours: in this time level of water had sunk from 11 inches to $5\frac{1}{2}$ inches. On filling it up to 11 inches again, $4\frac{1}{2}$ gallons of water were 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.

4 JUNE 1842.

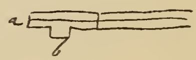
6230. Boiler up and insulated. The side and end cocks the same as before ( ); the side cock over hot air stove.
6231. The side cock instantly and always renders the boiler *Neg.*
6232. The End cock at first with low pressure rendered the boiler *Pos.*, with little exit of steam, but *Neg.* with more 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.*

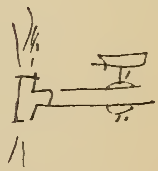
6234. At last, when the pressure of steam was up, the end connector did nothing, i.e. the boiler was not charged. But if the side cock was opened for an instant only, it made the boiler well Negative.



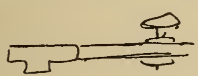
6235. Have a two way end piece thus; when put thus on to the pipe*, it gave no signs of electricity to the boiler. But when put on thus†, it made the boiler fairly *Positive.*

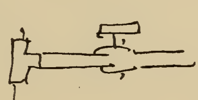


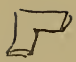
6236. When piece on thus, the passage of steam was so quick through the termination that all went out at *a*, and air actually entered at *b*. So finger drawn to *b*. Expect that steam should not touch sides of channel again when it has passed the place of quickest motion, or it becomes discharged again.



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] 



6238. Used another end piece which had a very open way, for the former piece had a contracted way in the middle. This open piece on the end cock gave the boiler *Positive*. It was in a pretty clean state.

6239. A jet, a gas pipe, etc., were put on the end cock, but nothing particular occurred. Suspect all the effect takes place at the first contraction of passage and that after attachments only tend to discharge steam and modify effects.

6240. It is clear that a certain cock will sometimes make the boiler Pos. and sometimes Neg., and from yesterday's expts., the state of the steam is reversed at the same time, being always contrary to the boiler.

6241. It is very remarkable to see how little steam from the side cock will make the boiler *Neg.*, whilst it is comparatively difficult to obtain effects from the end cock and piece. I rather think this is because the side cock has a clear unbroken way through, which allows the steam that has brushed the edge of the inner aperture and become charged to travel unmixed down the channell and escape, whilst the others mix it up.

6242. I think much depends upon the free escape of the steam after it has past the place of friction, and also on the free space before that place.

6243. Used the side cock and held stretched string across the issue of steam close to jet. Boiler uninsulated and an electrometer attached to it. The boiler electrometer perfectly quiescent, but the string electrometer became charged *Neg.*, for the string was Neg. This not derived either from the boiler or the finally charged steam, for first is neutral and the second *pos.*; was due to friction of string and steam.

6244. Now a good expt. Boiler uninsulated and could not charge its electrometer. The string across distant steam, i.e. 3 or 4 inches from the cock jet, was a collector and made its electrometer *Positive*, that being the steam electricity. But the same string close to the issue, so as to be rubbed by the steam, then became *Negative*—and these effects could be produced at pleasure. So the same stream of Steam, issuing from a metal cock, gave either Pos. or Negative electricity; whilst the boiler, being uninsulated, could do nothing. Hence the generation must be by the friction of the steam, at least when the string was rendered Negative.

6245. Now used Platina wire instead of string. Excellent. For boiler did nothing all the while to its electrometer, whilst the wire near the cock was *Neg.*, but in the steam more distant was Positive. The first kind was excited on the wire by friction. The last only collected from air rendered Pos. by friction in cock.

6246. When a string in jet, beautiful vibration or rather revolution either on one side or the other of the centre of jet. Must, to have friction, adjust so that string is in the centre and not moving, or else it does not charge. Hence again effect of the friction.

6247. Used Plumbago as *collector*: then was	Pos.
,, ,, as *exciter* close to cock: then .	Neg.
,, Copper wire as *collector*	Pos.
,, ,, ,, *exciter*	Neg.
,, Chip of wood as *collector*	Pos.
,, ,, ,, *excitor*	Neg.
,, wire gauze diaphragm as *collector* . . .	Pos.
,, ,, ,, ,, as *exciter*	Neg.

All these bodies near the jet were *Negative*, but in the more distant steam became *Positive*.

6248. Easy theoretically to make a famous Steam electricity battery that may perhaps replace even the Voltaic battery. We shall see. I see no positive objection. The indications are quite as good as those relating to the voltaic battery were in the first instance.

6249. The effects with a wire gauze diaphragm are very beautiful. Held near the jet, the gauze and all connected with it is Neg. Held a few inches off, it is Positive, and by moving it to and fro I could make the Positive electrometer collapse and then become Neg. or Vice Versa; or I could find out and hold the gauze in a place where the effects were equal and it was not charged at all. A quarter of an inch from this on either side would make the diaphragm Pos. or Neg. at pleasure.

6250. If a diaphragm of fine wire gauze were in a glass tube, and a stream rushing by it, it might be exceedingly powerful as a charger.

6251*. *a*, circle of very fine and close wire gauze in a glass plate *b*; *c*, steam chamber; *d*, escape chamber, open to air except that wire gauze collectors at *e*; collecting wire[s] go from *a* and *e* to *f*, which are the electrodes.

6252. Silk thread, stretched across retort ring, was used wet and dry. On the whole, when placed near to the jet and when the electricity conveyed away, could not be either of issuing steam or of boiler, but that produced by friction; it was *Neg.*, as the other rubbed bodies were.

6253. With this silk thread on retort ring observed a certain peculiar first effect at this side cock, as follows. After resting a while, the least puff of steam sent against silk and ring as a collector would instantly open the leaves of its electrometer; but on repeating the trials the puffs lost their effect, and at last the continuous stream did, not so much as a puff before. The steam was now evidently not so electric as before, i.e. directly after the rest. On shutting the cock, waiting a few minutes and beginning again, the same effects recurred. The electricity was always Positive, but the point was the going down of the power, the force of the steam not going down. This was with a low pressure.

6254. When a metal collector was used, the same was the case.

6255. This was not the case at the end cock, but that very feeble as to any action compared with the side cock.

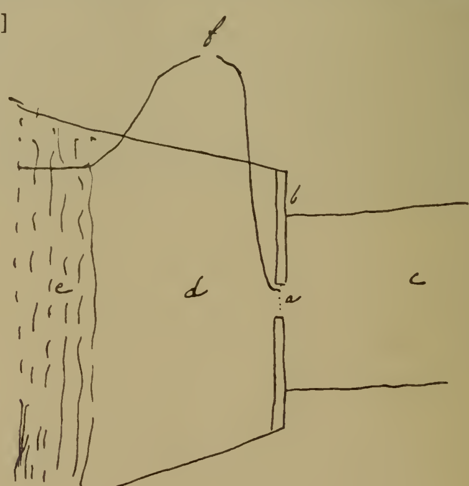
6256. The cause is doubtful—is not dependant on the degree of pressure—is apparently dependant on some state assumed by the stop cock and not on the steam.

6257. Insulated the boiler and tried it for this first effect at side cock at low pressure, and effect was certainly far stronger on beginning of the emission of steam than afterwards.

6258. Must find out the cause of this.

6259. The end stop cock has no power at low pressures as com-

* [6251]



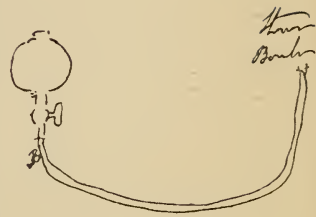
pared with the side cock—and the above phenomena of changes are brought out at the side cock better at low pressures than at higher ones.

6260. Have worked from 10 to ½ p. 1 ocl'k. to-day, or 3½ hours; have consumed 3¼ gallons of water.

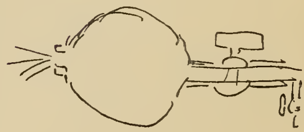
6261. The pipe for carrying off the issuing vapour is excellent. It prevents all interfering inductive effects of the steam on the electrometer and keeps the place dry.

6262. The electricity of steam and steam boilers is not due to evaporation, but friction.

SATURDAY, 11 JUNE 1842.

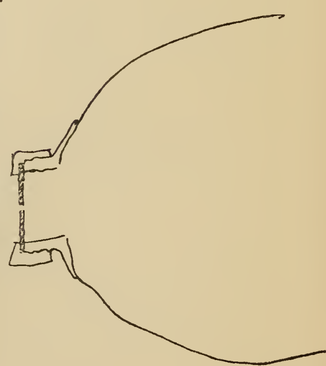


6263. Have had a new tube made, inches long and of inch in diameter within; at end of it a stop cock with steam way equal to the tube, and after it a brass globe inches in diameter, with a wide mouth at furthest side to which various apertures, stop cocks, etc. could be attached. The object was to make this globe a steam chamber and if necessary in it to de-electrify the steam (supposing it electric in the tube); and so experiment on effect of friction of steam alone. This globe had aperture of inch in diameter.



6264. All this in its place and the boiler tested as to its insulation and keeping of charge, which it did well. Now, when the principal cock open and the steam issuing by the wide aperture of globe alone, there was no signs of electricity either in the boiler or in the steam. Yet if boiler charged by shell lac whilst the steam was going off, it kept that charge unchanged. So evaporation of water in the boiler and its issue by this open aperture neither *charged* nor *discharged* the boiler. It did nothing.

6265. Shutting the steam cock, the issue of steam by valve of the boiler did slowly make the boiler *Neg.—friction.*



6266. I had plates with small holes in them made, to close the outer aperture of the globe, so as to give fair issue of steam out of a small hole. Steam issuing out by these did *not charge* the boiler.

6267. A fine wire held in this issue of steam produced no electricity, either in the wire or the boiler.

6268. Put a straight stop cock in thus*, and now the boiler was well *Positive.*

6269. Put on a right angled connecting piece, which had of course a female screw inside at both apertures. This made the boiler well Positive more than before, and when the conductor and connector were arranged to catch and try the steam, its electrometer was *Negative.* So *boiler Pos.* and *Steam Neg.*

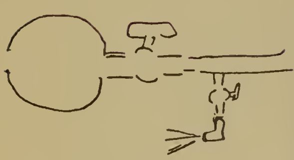
6270. After a few minutes, repeated the last expt., both boiler and steam conductor having their electrometer. The steam was now something *Neg.* and the boiler *Nothing*—over and over again. Then on a sudden both conductor and boiler electrometers diverged well, the *Conductor Neg.* and the *boiler Pos.*, as before. The mode of issue of the steam apparently made this difference.

6271. Then the last result happened again and again, i.e. the steam conductor was *Neg.* and the boiler *Pos.*; and also with this side issue I obtained by a wire the place of Neg. on the wire near the issue.

6272. It appears to me that, when the issue of steam is rattling and noisy, that then the exciting condition is very influential. This noise depends upon the way in which the air and the steam conflict, principally.

6273. There was more water present in the steam of these last experiments. The furnace beneath the issue was not over hot.

6274. Took of[f] the right-angled connecting piece and now the straight cock alone did nothing, i.e. it produced no divergence of the boiler electrometer—very remarkable.



6275. There is a draining cock on the steam pipe just before the large issue cock; it of course points downward. Put the side piece on to this, and the issue of steam here produced no effect on the boiler electrometer, but did a little affect the steam conductor electrometer, making it a little *Neg.*

6276. Took off the right-angled piece and tried the cock alone, and now it affected neither the boiler nor the conductor electrometers.

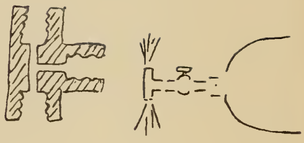
6277. Worked from $10\frac{1}{4}$ to $12\frac{1}{4}$, or two hours; used 2 gallons of water.

* [6268]

6278. Worked again. Boiler filled up to the mark—well insulated. Worked with my tube and globular termination. Rather indifferent about the fire beneath the termination or exit.

6279*. Had a new stop cock of ours at the end. No electricity in the boiler produced by it.

6280. Put on an old cross piece to this new stop cock. This old cross piece has its three female screws connected by a narrow air or steam way, as in the figure. The issue of steam was open right and left. The boiler electrometer opened fairly and *Neg.*

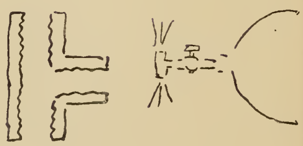


6281. After a few minutes repeated the experiment, and now boiler electrometer quite *nul.*

6282. After a few minutes more, repeated expt. again, and now the boiler somewhat *Neg.*, as at first, and more so as the pressure was higher.

6283†. Took off cross piece, and had the straight stop as at first, and now boiler neutral or nul.

6284. Now put on New cross piece, which has the female screws carried throughout, so as to be of large bore and rough throughout—and now the boiler electrometer opened out well and was *Pos.* Charged a Leyden phial by it—*Pos.*



6285. When the steam was left issuing, the power (electric) went down much, though all the time the pressure in the boiler was such as to lift continually the regulating valve. So diminution of power was not due to diminution of force of steam.

6286. Whether the steam was allowed to escape by opening the great or the small cock, i.e. the one on the one side or the one on the other side of the Globular steam vessel, still the boiler became *Pos.*

6287. When new cross piece was put on thus, there was no issue of steam at the side hole, but an indraught of air there, and the boiler was only a little *Pos.* Nothing like so good as if cross piece was in the former Position.

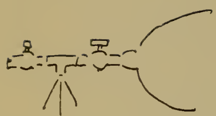
6288. Stopped up the side hole by a stop cock closed and now the boiler was fairly *Pos.*

6289. When the steam issued only by the regulating valve of the

* [6279]

† [6283]

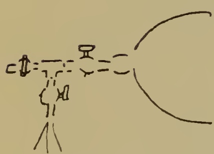
boiler, it shewed its power of producing *Neg.* electricity by causing the boiler electrometer to diverge gradually Neg. This valve is always *Neg.*, i.e. it always charges the boiler Neg., giving *Pos.* steam—at least, I expect so, but have not tried it.



6290. I now stopped up the end hole of the same new cross piece, leaving the side hole open, and now boiler was very *Pos.*, more so than when we had two lateral issues. Had also the cracking, crushing, sharp sound in the issue of steam which always seems to accompany good evolution of electricity.

6291. Tried the steam of this side issue by insulated conductor, its tin collector and the electrometer. It was very variable, the leaves opening and closing continually and making the conductor electrometer sometimes *Pos.* and sometimes *Neg.* The boiler electrometer was always *Pos.*

6292. After a few minutes, repeated the expt. and same variable results.



6293. Now put Grove's Stop cock on at the side aperture and had a fine electrical effect at once. But the boiler was now regularly *Neg.* and the steam *Pos.*

6294. Still, this full effect did not continue, for though the issue of steam went on, the Electrometer fell and often became quite *Nul*, rising a little now and then.

6295. The going down is an effect common to the two issues, but this stop cock makes boiler Neg., and the cross piece alone made it *Pos.* Remember the cross piece is *roughed* by screw thread and that the stop cock of Grove is *smooth* throughout.

6296. Now put Grove's Stop cock on to the globular vessel; it made the boiler freely and well Negative.

6297. Turned the cock end for end—same effect as before.

6298. Put on in the same place a new stop cock of ours, and it always made the boiler *Pos.* So these two stop cocks in direct opposition to each other as to their electrifying power.

6299. Now the only difference I can see as yet is that Grove's Stop cock is smooth throughout, and ours had a female screw thread turned in it at each end, to the depth of a quarter of an inch. I believe that is the cause of the difference. The smooth tube makes boiler Neg., the *rough* tube makes boiler *Pos.* This falls in well with all the preceeding effects (Is the effect of Oil, see further on).

6300. Again experimented with our stop cock as before, but now four times in succession it made boiler *Neg*. Turned end for end—still boiler *Neg*.

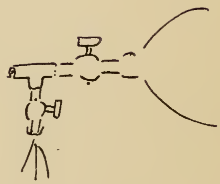
6301. More steam so as to produce rattle probably would have made it *Pos*.

6302. Put some fine wire up Grove's Stop cock so as to offer resistance and roughness, but now issue of steam quiet and small and no electricity. Steam must get away. Removed the wire and now the boiler made Neg.

6303*. Tied fine wire gauze over the end of Grove's Stop cock and then put it on to Globe. The furnace and boiler was *Neg*., and better by far than if no gauze were there. So this appears to improve the effect.

6304. I observed, whilst continuing the issue of steam, that it was not so noisy as before—but more quiet and tranquil. But whenever it broke for a few moments into a crackling sound, as often happened, then instantly there was an increase of electricity shewn by the boiler electrometer; and when by increase of pressure this rough issue was continued, then more electricity was produced than if quieter.

6305. Still, the *first* effect was always the best (6294, 6295).



6306. I then put in this Stop cock (of Grove's, so covered at the mouth) sideways. It made the boiler well *Neg*.

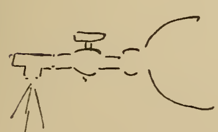
6307†. Took off the cock and left the issue at the side by aperture of cross piece, and now the boiler was *Positive*. So the effect of the exit very evident.

6308. Perceived by the shaking of the ball and issue of water that it was half full of water, i.e. up to the level of the issue aperture. Drew it off by a syphon and then lo! little or no electricity, either by the good cross piece or by Grove's stop cock, both so effectual before.

6309‡. Put common water into the globe till it was as full as it could be in its usual position, our New stop cock being at the

* [6303]

† [6307]



‡ [6309]

end, and now there was very good excitement, the boiler being *Negative*. Put on the rough cross piece and still good excitement, the Boiler being now *Positive*.

6310. Now put Grove's Stop cock into side and there was no effect, the steam issue being quiet in sound. On steam getting higher, then effect produced and boiler *Negative*.

6311. Took out the water from Globe and now all bad again.

6312. Worked from $9\frac{3}{4}$ hours till $12\frac{1}{4}$ hours, or $2\frac{1}{2}$ hours; had used $3\frac{1}{4}$ gallons, bringing down the depth from 11 inches to 7 inches. Made up to 11 inches in depth again.

6313. Probably steam alone does nothing, but water and metal are the bodies which by friction produce electricity.

6314. *Change of water and ice into each other.*

6315. In Lavoisier's Calorimeter the water thawed at one part—froze at another.

6316. In the Glaciers, according to Agassiz, water freezes within the crevices.

6317. When wet snow is squeezed together, it freezes into a lump (with water between), and does not fall asunder as so much wetted sand or other kind of matter would do.

6318. In a warm day, if two pieces of ice be laid one on the other and wrapped up in flannel, they will freeze into one piece.

6319. All this seems to indicate that water at 32° will not continue as water, if it be between two surfaces of ice touching or very near to each other.

6320. Also that in such cases, an accumulation of such pieces of ice in one heap or portion can have freezing going on within, whilst no part is below the freezing point and whilst thawing is going on at the outside.

6321. Now the freezing must evolve heat, and this heat must either be conducted to the outside through the mass of ice as a solid, or else be carried off by the water that is flowing off from the part where the ice is two [? too] far separated for the effect to take place.

6322. The effect appears due to this, that a particle of water is cold enough to freeze when solid ice is on both sides of it, though it is not cold enough to freeze if ice be only on one side of it, equally cold water being on the other.

6323. In that case, the interior of a piece of ice is warmer than the exterior, where it is melting, for it will require a higher temperature to melt a particle of ice having ice on *both* sides of it, than a particle having ice on *one* side of it.

6324. In that case, ice is warmer than ice and water.

6325. The ice probably acts as a nucleus, but it appears that the effect of one surface of ice on water is not equal to the joint effect of two.

6326. The phenomena relate to the stability of ice and water, or of these state[s] of the *Ox. Hy. combination*, and therefore are connected with the beautiful results of Macaire or Marcet on boiling water. I forget which it was made the expts. lately.

6327. If ice can serve as a nucleus, so may other bodies, and there may be some which assist the conversion of water into ice, and perhaps other favouring the conversion of ice into water, of course excluding combination forces.

6328. In the case of water and steam, water against water changes with more difficulty into steam than water against other bodies. So in this case, water against water changes more difficultly into ice than water against ice or perhaps other bodies. *Water* therefore seems to be the more stable state—the normal state.

Now as to *experiments*.

6329. Put one piece of ice upon another in a deep dish, covered over to keep in cool air. After half an hour, one was frozen to the other. Could even find two pieces sticking together after 5 minutes, or even 2 minutes, but only at a few points.

6330. A piece of ice wrapped in flannel and left. In half an hour the flannel is frozen to it, as if it were glued on by the surface filaments.

6331. If four or five thicknesses of dry flannel be pressed by the hand on to the melting surface of the piece of ice, in less than half a minute it is found sticking to it, as if frozen.

6332. If a single thickness be pressed on by the hand for 5 or 10 seconds, notwithstanding the warmth of the hand, signs of sticking will be perceived.

6333. A piece of flannel put on a piece of ice and wrapped up in flannel, soon well frozen to it, remaining dry as it was at first. Again, put it under the ice, i.e. at the bottom. After 3½ hours found it all swimming in water, but flannel much frozen to the ice.

6334. *Linen cloth:* a duster. Ice wrapped in it—some sticking after half an hour, but cloth and ice very wet.

6335. Linen cloth folded up and pressed on to the ice for a few seconds, stuck as if frozen to it. Wiped the ice dry and pressed a dry place of the linen on to it for a minute. It was much frozen, i.e. it stuck very much to the ice.

6336. In this way linen stuck well to the ice.

6337. Ice wrapped up in a linen cloth did not stick at the top surface, but did at the bottom surface, where the weight of the ice preserved a contact—but not much adhesion—was half an hour.

6338. Piece of linen canvas put on top of ice and wrapped up in flannel, after half an hour stuck a little. Again, put on *under* the ice, after 3½ hours was very wet and flowing, but frozen to ice a little.

6339. Tin foil, copper foil and paper in pieces were laid on to ice wrapped up in flannel and left. In half an hour did not stick, but they are very wet and had not been kept close—water between them and the ice. The wrapping flannel stuck every where.

6340. A better way for trial experiment is to put the things to be tried between two flat pieces of ice and wrap them tight up together in flannel; or put them in a deep basin for a while and so that the water shall drain from the experimental part.

6341. *Dry flannel* between two ice: frozen strongly to ice in 40′.

6342. Do. „ „ : in two hours took some force to separate the pieces of ice, probably 4 or 5 lbs. weight.

6343. *Flannel wetted* by boiling it in distilled water for an hour and put wet between two ice: well frozen to ices in 40′.

6344. *Canvass* linen between 2 ices: froze and stuck in 40′.

6345. *Wet Canvas* Do. : in 40′ froze the two pieces together.

6346. *Black silk ribbon* dry
„ „ soaked in water } put between ice, had frozen a little to the ice in 40′. The pieces, arranged as before, but better, and left for 1½ hours, both well frozen to ice. Could lift the lower piece of ice by the upper.

6347. *White silk ribbon dry—and soaked*—between ice—had frozen a little in half an hour.

6348. The day was warm and I believe the effect is due to an actual congelation of the water through the influence of the solid bodies present. I do not think the ice could have been of a temperature within lower than that at its surface, so as to have caused freezing by conduction of cold.

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6349. A piece of ice sawn into two pieces; the flat surfaces put together; in less than 10″ the two are frozen into one piece by the touching points; that it is not water between which holds them is shewn by this, that not only do they resist direct separation, but also any lateral or twist motion. The feeling soon shews this to the judgment.

6350. The two pieces put into water at the laboratory temperature and then brought into contact. In 10 seconds and less were frozen together.

6351. When the pieces are pressed together, the adhesion by freezing is almost instantaneous.

6352. Tried the following *woollen* substances by putting them between 2 pieces of ice (   ) and binding them up in flannel wrappers for 10 minutes:

Dry white flannel
Thoroughly wet flannel
White Kerseymere
Green drugget
Brown drugget
Gray drugget
Brown Marino
Black Bombazine
Worsted cruels in threads: white, red, green, blue, black, gray.

All these adhered by freezing in the course of 10 minutes.

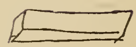
6353. A piece of ice was put *under* common laboratory water; a piece of white flannel thoroughly saturated with water was put *under* the water on to the ice, and then a second piece of ice was put on to the flannel—all being under the water and pressed moderately together. In less than a minute the flannel had frozen to both ices, though under the water the whole time.

6354. Another good and quick test method for flexible things, as cloth, is to place a piece of ice with a flat side upward; fold a linen cloth into a pad with a smooth under surface; dry the ice with the pad, at the same time cooling the surface of the pad; then lay on the piece of flannel or other body to be tried and hold it down with the linen pad, or with a pad of flannel, for half a minute or a few seconds. The body pressed against the ice will be found frozen to it if of woollen.

6355. Thus flannel (dry) pressed against dried ice freezes very quickly to it.

6356. Wet and saturated flannel Do. frozen to the ice in 20 or 30 seconds.

6357. The other things of wool ( ) tried in the same way; all froze to the ice.
6358. Put ice in ice cold water, then tin foil, less than the ice, on it, and then another ice over that; kept all together for some time, but could procure no evidence of freezing to the metal.
6359. Place[d] tin foil between 2 ices wrapped up in flannel, but no trace of freezing together was apparent.
6360. Tried some other ways to find if metal could freeze to thawing ice, but could not find any such result.
6361. The conducting power of the metal is no doubt one very great difficulty, but still, metal does not seem to favour the effect as ice or flannel does.
6362. Wood: put pieces of deal between two ices in flannel; generally no signs of sticking and freezing. But in one case did appear to stick—the pieces had sunk partly into the ice; that is, there had been more thawing where they touched; but why? For though the wood pieces (three) kept the two pieces of ice apart, yet there was more ice dissolved than the first sensible heat of the wood could I think do. Why then did it go on?



6363. Three thicknesses of thoroughly wet kerseymere were put between 2 pieces of ice for 2 hours. The ice and the kerseymere were frozen together on each side, but there was *no* freezing between the kerseymeres. Hence surfaces of wool alone and water will not induce freezing at this temperature.

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6364. Must make these points of freezing power of ice bear together some day, when I am able to devise a good expt. to shew the temperature or other effect which may measure the difference of ice and ice and water.
6365. Connected with it is I think the integrity of Sul. soda and other crystals if surface be not broken.
6366. Perhaps also the inactive state of Iron. Is not Elba iron ore unchargeable at Neg. pole, inactive, etc. and like inactive iron?
6367. Also the effect of polished surfaces, as of steel, not to rust, or of metals in battery or acid not to evolve hydrogen. Zinc planings are smooth on one side, rough on the other—in acid the smooth inactive, the rough active.

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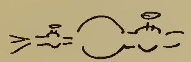
6368. Renewed steam working to-day, with Grove's boiler and our tube and ball, as before, insulating the boiler and observing all the electrical effects by its state.

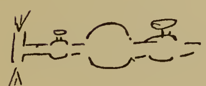
6369. Now the *boiler* became electrical as follows, by difft. arrangements:

Globe wide mouth open . . . . Pos.

Our new cock (   ) in globe strait . . . . Neg.

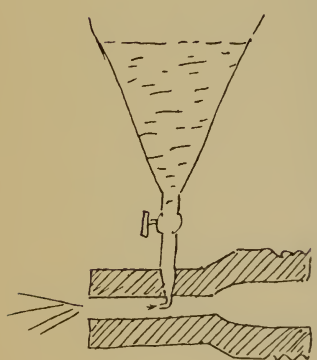


Grove's stop cock	Do. . . .	low pressure . . .	Pos.
Do.	Do. . . .	higher pressure . .	Neg.
Do.	Do. . . .	intermediate pressure .	nul
Our new cock .	Do. . . .	low pressure . . .	Pos.
Do.	Do. . . .	intermediate . . .	nul
Do.	Do. . . .	higher pressure . .	neg.



Piece doub. side way, wide passage . . . . Pos.

Do. . . Do. . . narrow passage . . . . Neg.



6370. Have had a funnel apparatus made, like sketch, intended to supply a jet of water into the steam current to see effect of that water. By fixing a glass cylinder 12 inches high on to the funnel, had pressure of water enough to cover a small but free jet from end of little internal tube when in the ordinary state.

6371. But when attached to the boiler and steam issuing through the apparatus into the air, little or no water descended by the jet. In this state it made Boiler . . . . Neg.

6372. When water seemed to go down a little, either at moderate or at higher pressure of steam, there was no effect; was . *nul.*

6373. It may be that water has not rubbing space on sides of the tube beyond this issue. Perhaps the tube ought to extend an inch or two beyond it, and I think the issue of water should be rather on to the side or surface of steam way rather than to the middle (6414).

6374. Now went to first effect (6294, 6295, 6305, 6548), for now as before found the first issue of steam far the most effectual and soon made this out.

6375. Believing that difference depended on cold and hot state of the cock or issue pipe, heated and cooled it in various way[s]—heating either by letting a little issue of steam through or else by outward application of spirit lamp; and cooling either by allowing it to stand a few minutes alone or else running a jet of cold water on the outside for a minute or two. Excellent effects were obtained. Thus, using Grove's Stop cock:

Grove's stop cock,	first issue of steam	strong effect
” ”	after issue continued a minute	weak effect
” ”	shut it half a minute and opened again whilst hot . .	*Poor*
” ”	shut half a minute and cooled by jet water, then	*Strong effect*
” ”	heated by small issue of steam, then full steam let on . . .	Very poor
” ”	heated by spirit lamp, no steam going through—then steam on	Very poor
” ”	cooled by water jet, no steam going through—then steam on	Strong effect
” ”	being hot, little or no effect; then whilst steam going through, cooled by jet of water. Strong effect came on as cooling proceeded. Excellent	
” ”	cooled part of cock at issue end only and now no good effect	
” ”	cooled part only at boiler end, keeping issue end hot, and now excellent effect, better than if the whole cooled	
” ”	did this whilst steam passing through—excellent effect.	

6376. Our new stop cock—exactly the same effects.

6377. Turned the stop cock end for end. Both with ours and with Grove's it seemed as if one position was better than the other, but all the effects of heating and cooling were the same as before.

6378. The electricity of the boiler was always *Neg.* in these heating and cooling expts.

6379. Emptied the great ball of water—still the same effects. When cock and all were hot, little effect; when cooled, then good.

6380. Now put two cocks on thus*, with a wide way connecting piece. When all cool as at first, Excellent effect. When they had become hot, effect little or none.

6381. When all made hot by spirit lamp before the steam let on, then scarcely any effect.

6382. If whilst steam issuing, the outer cock made cool by jet, no effect.

6383. But if whilst outer cock was hot, the inner cock or that nearest the boiler, or the wide connecting piece were made cool, then full and excellent evolution of Electy.

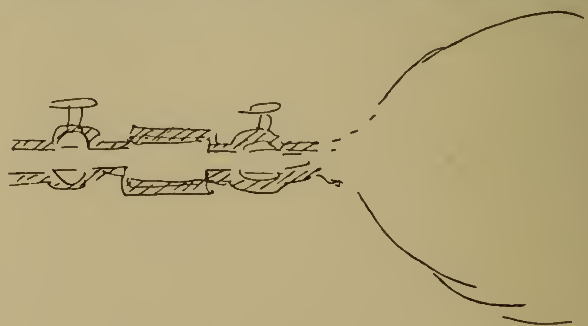
6384. So with two or more pieces; same effect as with one.

6385. Would seem as if the water particles which were condensed wanted a certain space of metal afterwards to rub over, or else cooling near the end ought to do all the good.

6386. Now put on a side piece so as to have a steam issue which made the boiler *Pos.* Then the effects of heating and cooling seemed to disappear, i.e. cooling did not produce more evolution and continuing the jet did not seem to lower the Pos. state. But must repeat these more carefully.

6387. It is astonishing how well the boiler keeps its insulation and how little cloud about it affects it, either to charge or discharge it. If an issue of steam be going out from a hot stop cock so as to produce no charge of the boiler, though much steam issue, and if then the boiler be charged by excited stick of lac, that charge will be shewn by the electrometer to be perfectly steady, though steam issue, and is taken off only by touching the boiler or its connection. The hand may be put in the cloud of steam within 2 inches of the jet, or the steam of the jet may be driven back on

* [6380]



the cock and ball by a basin put up to aperture to within an inch or less of it, and yet the electrometer shews a perfect and steady charge, not derived from the steam but from the previously applied shell lac. The vessicular cloud is not a sensible conductor.

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6388*. Newman has made me three thin short brass tubes, each $2\frac{1}{2}$ inches long and of three sizes, so as to slide into each other telescope fashion so as to give either three separate issue tubes or a lengthening and shortening tube. The large tube has a screw on it to attach to the globe apertures. The others I fitted in when separately used by putting them through a phial cork and screwing the cork into the aperture of the globe, and this did very well. The diameters of the tubes were a little less than the drawings[1].

6389. The boiler was right—all insulated—had its gold leaf electrometer, and when charged by shell lac, held its charge. Had a cooling jet from funnel attached to the issue end, which also was insulated, so that could cool and observe effect from the first moment.

6390. Always observed the electrical charge given by the examination of the state of the *boiler*, i.e. of the *metal against which the steam and water rubbed*. The steam and water took the opposite state.

6391†. The length of largest tube, No. 1, was used. The first effect was poor and the boiler *Pos*. But when the steam pressure was good the electricity more abundant and *Neg*.

6392. When with Good issue of steam the current was smooth and sound steady and hissing, then little or *No* electricity, but when it was a shaky rattling issue, then plenty of Electricity *Neg*.

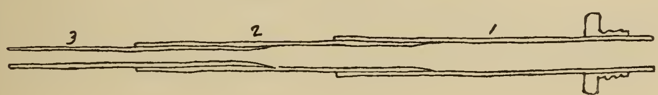
6393. This simple tube did as well as our best stop cock, but not so well as Grove's stop cock, i.e. the one which came with the boiler tube from the London Institution.

6394. The boiler valve produced Neg. state of boiler slowly.

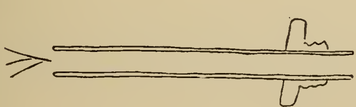
6395‡. Used larger, No. 1, and second tube, No. 2, in one length—

[1] The drawing is reduced to $\frac{3}{4}$ scale.

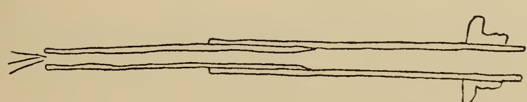
* [6388]



† [6391]



‡ [6395]



issue hissing and quiet; *no Electy.* Could not force this length into a rattling issue.

6396. Again two tubes—hissing issue—no electy.

6397. The three tubes in one—hissing issue of steam—no electy.

6398. Tube 1: hissing issue—little or none—when roaring issue, *Plenty Neg.*

6399. „ 2 alone—when hot, then hissing issue and scarcely any Electy.

6400. Do. „ cooled outside and rattling—then Good evolution, though not so much as No. 1, but there more steam and water pass in the same time.

6401. Tube 3 ( ) alone and hot and issue hissing—no electy. evolved.

6402. „ raised to rattling issue. Good evolution of Electy.

6403. „ when cooled by ice at end near boiler, *evidently improved.*

6404. „ at times a little water would hold on to the outside edge of the issue end, and then it appeared as if there was *more electricity*; but whether the action of this water caused more, or whether it was a mere result of that state of the steam issue which caused more electricity, cannot now say. Think the latter, because of results to be described with pure and common water (6416).

6405. In using these tubes, cooling of the tube at the boiler end evidently produce[s] much good effect. But I believe entirely by producing the water which by friction produces the electricity.

6406. Now experimented with pipe No. 1 and the ball of steam tube, in different states as to quantity of water.

6407. Ball full (as it could be) of water—excellent evolution of Electy. *Neg.*

6408. Ball emptied of water and good jet of steam—scarcely any.

6409. Do., even cooling tube with ice—very poor.

6410. Ball now filled with common water (6555)—*Still None*—no trace.

6411. Judged that this was due to the saline matter of the water and its increased conducting power in consequence of their presence. So emptied the ball and filled and emptied it thrice with distilled water, leaving it charged with distilled water, and now when the steam passed over, it had abundance of Neg. Electy.

6412. So if no water in the Globe or pipe, no excitation. If common water in Globe or pipe, no excitation—but if distilled water, Electy. evolved. Hence water, and that distilled water, essential. So not due to the friction or evolution or any thing else belonging to the steam as steam. Is essentially due to the rubbing water.

6413. Looks very like the case of the rubbing of a bad conductor, as flannel, against Metal. There also the metal rendered *Neg.* So if water not made as good an insulator as possible, it ceases to shew the effect.

6414. Now in the expts. with the supply of water by a funnel (6370) used common water. Hence probably cause of failure—must repeat them with distilled water, etc. etc. etc.

6415. With Tube No. 1 on steam Globe and Globe charged with distilled water, had abundant evolution of Electy. Neg.

6416. Applied common water to the *outside and end* of the tube (6404), where it partly adhered and *did not* destroy the excitation of Electy.

6417. Applied a solution of potash—and also dilute sulc. acid in the same way to the *outside* and end of the tube, but there was *no diminution* of the electricity evolved.

6418. Put a little solution of Sulphate of Soda into the water in the steam globe—now all effect gone. *No electricity.* Good.

6419. Used Grove's stopcock, which is a better excitor than any of our tubes or stopcocks. No use—*no electricity* was evolved.

6420. Cooled the stopcock—still *no electricity.*

6421. Put on the side piece which usually makes the boiler Positive, but now *no effect*—no electricity.

6422. Then emptied the ball and washed it well out with distilled water: left the ball empty and used Tube No. 1. *No electy.*

6423. Ball empty and used Grove's stop cock. *No Electy.*

6424. Filled the ball with *distilled water* and use[d] Tube No. 1, whilst the excess of water poured out like a stream. No effect, but immediately that over, excellent effect.

6425. So with Grove's stopcock—excellent effect.

6426. So the difference between common and distilled water very evident.

6427. As ammonia increases conducting power of water but little, anticipated that its addition to the water would not prevent it from evolving electricity—added some, and its presence was shewn by the steam which issued being able to redden damp turmeric paper.
6428. With Grove's stopcock, the development of Electricity was pretty good, but not so good as when distilled water only in the Globe.
6429. With Tube No. 1, excitement of Electy., but not much.
6430. So Ammonia does not destroy the power, but does diminish it.
6431. Then put in the smallest drop of Sulc. acid to form a little Sul. ammonia in the water, and now, as was expected, the power of generating electricity, either by Grove's Stopcock or our tube, even at the highest pressures of steam, was *entirely gone*.
6432. The quantity of water retained by the Globe when steam passing through it is   cubic inches or 4 ounces fluid measure.
6433. Emptied the globe, washed it well out and left it filled with distilled water. Now the effect was very good.
6434*. The wide aperture of the globe, when open, always makes the Boiler *Pos.*; and now to see whether the action of the water which produced this Pos. state was also affected by the conducting power of the water, made an expt. With pure water in the Globe the Boiler was *Pos.* Added a little sul. soda solution to the water and now *no action whatever*. So this affected in the same way as the Neg. state.
6435. There is a cock under the steam pipe before the boiler, meant to withdraw the water from the pipe before it enters the Globe. This cock makes the boiler well Neg. when full issue of steam from it, and it did so now. But Grove's Stop cock at end of the Globe could do nothing, because water in the globe now saline. The water issuing at the pipe cock was of course pure. A fine illustration of the dependance on water and the effect of its impurity or conducting state.

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6436. The boiler with our steam tube, steam Globe, etc., all insulated and steam up. The apparatus held a charge when charged

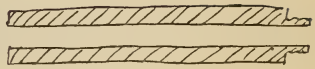
* [6434]

by shell lac. The Globe has been well washed and is now free from water.

6437. Whenever I say the Globe is filled with water, I mean as full as it can be, having the steam rushing through it. It is of course in the state shewn (6434) or nearly so, and then contains nearly 4 oz. fluid of water. The whole contents of the globe is fluid ounces.

6438. The brass tube No. 1 (6388) on to the steam globe gave after the first moment no electricity, no water being in the steam globe. Filled up steam globe with distilled water and then it gave plenty of Electy., making boiler *Neg.*

6439. Used a *wooden tube* made of box wood, its inner dimensions being the same as those of the metal tube No. 1 (6388). It excited as well as the metal tube, making the boiler *Neg.* When rattling issue came on, the Electrometer charge rapidly rose.



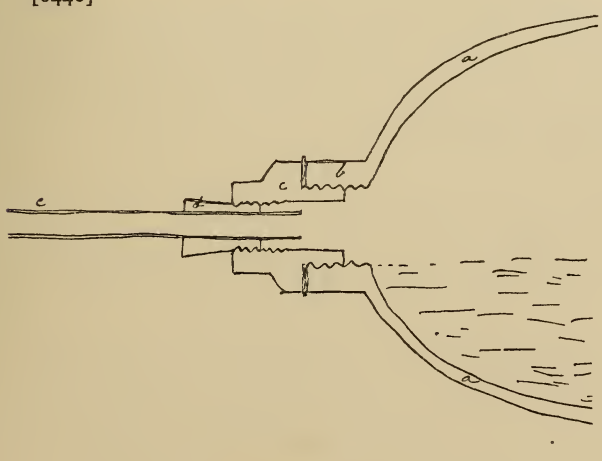
6440. Now used tubes of various materials and lengths; they were about the same diameter as the brass tube No. 1 (6388) and were fixed into the steam globe by putting them into a cork and then screwing the cork into the steam globe aperture. To understand this and the following results, let this drawing* illustrate, which is nearly the right size[1]: *a* is the steam globe; *b* its largest aperture,   of an inch from screw thread on one side to screw thread on the other, i.e. a cylinder   of an inch in diameter would enter it; *c* is a brass mouth piece to supply a smaller screw aperture   of an inch in diameter from thread edge to thread edge (called air pump screw); *d* is the cork into which a tube may be fitted, and *e* a tube so fitted.

6441. A quill mounted as a tube, about the size of Brass tube No. 1. It produced no effect.

6442. Another quill tried—not the slightest effect. Quills may be very useful as giving uncharged streams of steam and water.

[1] The drawing has been reduced to $\frac{2}{3}$ scale.

* [6440]



6443. Piece of thin *flint glass tube*, about same inner diameter as Brass tube No. 1 (6388), and this length* was used. The effect was excellent and continuous. The boiler was rapidly charged Negative, giving a good and quick succession of sparks, and the effect surpassed any former termination, even Grove's Stop cock.

6444. Little Sul. soda in the water of steam globe took away all this excellent power from the Glass.

6445. Washed out globe and left it empty of water—still no signs of electricity by *glass tube* (6443). Again at higher pressure there was a first effect, and after that a little effect, but not to be compared with that of the Globe fully charged with water.

6446. The quill tube with the empty steam globe. *No Electricity.* The length of the quill tube was †.

6447. Used a green glass tube (bottle glass) of the same dimensions as the flint glass tube (6443) ‡. It gave very feeble signs of Electy. Now filled up the globe with distilled water, and after the excess of water was driven out, the green glass was as good in excitation as the flint glass. Boiler *Neg.*

6448. Another green glass tube: same results as to good excitation.

6449. The flint glass tube (6443) restored to its place *as good as ever* now water was in the globe.

6450. The quill tube in again and water (distilled) in the globe gave a very little Neg. Electy. to the boiler. So it does excite with proper proportion of steam and water, but is very poor as compared to glass or metal or wood.

6451. *Wood tube* again. Is the best of all—had rapid succession of sparks and *fired powder by them.*

6452. Again the quill tube—*nothing.*

6453. It seemed to me as if the wood tube was best because it was wet, and suspect the excitement of Electricity is by water against water. So put the wooden tube to soak in distilled water.

6454. Put also the quills to soak in water.

6455. Glass is most likely only a water tube, i.e. a tube wetted thoroughly, and if I had a tube that could not wet, it would probably not excite at all—as the quill, or excite differently. The glass must be a wet tube or it would not conduct its state to the boiler part of the apparatus.

* [6443] 

† [6446] 

‡ [6447] 

6456. Use tubes of Sulphur, Shell lac, oiled matter, varnished matter.

6457. Now used the flint glass tube (6443), but observed what effect was produced by altering its position in the mouth piece of the globe. This was effected by pushing it more or less through the cork, and the extent to which this was done may be indicated by the accompanying diagrams.

6458. Tube in thus*. Good effect.
Tube in thus†. No effect at all.

6459. But as this projection of tube into the globe would tend to take its inner aperture out of the way of the water thrown up by steam within, I filled up the globe with water and replaced the tube in its last state. Then when excess of water was thrown off, the tube acted better than just now, but not so well as at first.

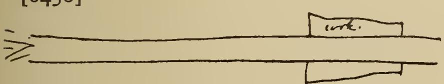
6460. All this shews that the excitement depends on the favourable supply of particles of water to the inner aperture of the tube, and when the shape of the Steam globe mouth is a fixture, gives a power of adjustment by trial dependant on the alteration of the tube in the cork or mouth piece.

6461. The tube now brought to this state‡ was as good as ever. When the cork was carried quite to the end, thus§, the effect was not so good. But when restored to this condition||, it was as good as ever.

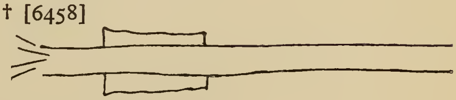
6462. Grove's stopcock (6419) and our own stop cock not nearly so good as this flint glass tube. In fact the tubes of *flint glass*, of *Green glass* and of *Wood* excellent–better than the metal tube No. 1 (6391).

6463. Now examined the effect of the position of a Brass tube in relation to the cork and aperture, in the same manner. The length of this brass tube is¶ and its diameter . When it was in the cork thus**, it excited pretty well, but not as well as the above tubes. When thus††, not so good as the last state. When

* [6458]



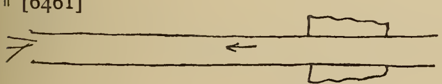
† [6458]



‡ [6461]

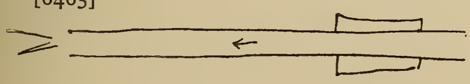
§ [6461]

|| [6461]



¶ [6463]

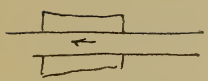
** [6463]



†† [6463]

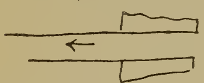
put back to this*, was improved and good again. When carried further thus†, the effect was better still–and still better again when it was increased to this degree‡. I then pushed the tube further in, up to this degree§, and now it was bad and the effect very feeble–but when it was withdrawn to this state||, it was very good again.

6464. In order to have some idea of effect of long tube and also of position in steam vent, I now used a longer brass tube of same diameter as the last, indeed a piece of the same, its length being as this line[1]¶ and its inner diameter of an inch. I shall merely represent the cork end and its distance from the cork in the figures. At first it was thus, and very good in effect.

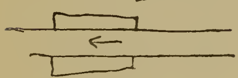


When thus, it was very good–

and thus, as good, perhaps even best of all.



When returned to this state, it was certainly not so good, and



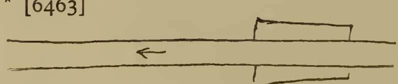
when it was still further pushed in, to this state**, there was scarcely any electrical effect, and only the smooth hissing sound of the steam issue.

6465. After this I put the three brass tube[s] (6388) on in one length–the effect was very poor and the sound hissing and smooth. Took off the parts 2 and 3 and now the effect very good and the sound rattling.

6466. Took a piece of the same brass tube as that already described (6463), but shorter, of this length††: inches, and

[1] Reduced to $\frac{3}{4}$ scale.

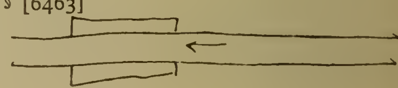
* [6463]



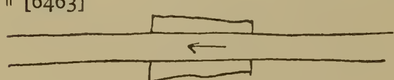
† [6463]

‡ [6463]

§ [6463]

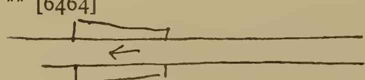


|| [6463]



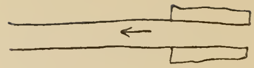
¶ [6464]

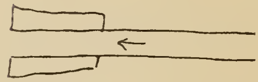
** [6464]



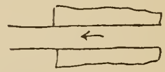
†† [6466]

shifted it in the cork holder as before. When its inner end was level with the cork the tube was as good as any of the former ones. When the end projected beyond the cork $\frac{1}{8}$, $\frac{2}{8}$, $\frac{3}{8}$ of an inch*, all were good positions and excited well; when it was thus, it was best of all and very good, the boiler being *Neg*. In all these cases the sound of issue was rattling.

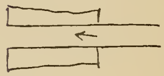




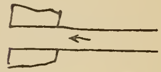
6467. A shorter piece of the same thin brass tube was used, being inches long and of inch inner diameter. When in this position, it excited very well;



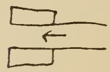
but when this, it excited better and equal to any of the other tubes, except perhaps the last (6466).



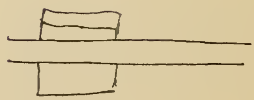
When further in, as thus, it was now as good as any of them, though so short. So the frictional excitement seems to be limited to a certain part; the rest acts merely as a tube carrying off the particles, but probably not touching them, or if it does, probably discharging some which have been charged in an anterior part.



6468. Used a still shorter piece of the same brass tube, of an inch in length and of an inch in diameter. When inserted thus, it was very good, but not so good as the last two (6466, 6467), they being further through the cork. Perhaps this might have come up if fixed further in.



6469 Used the brass tube† (6466), putting it into a wider cork, and displacing the piece *c* (6440), fixed it into the larger aperture *b* of the steam globe. As this alters the form of the parts about the inner aperture of the jet tube, it also will probably affect the most available position. I made the distance from cork the good distance above (6466); it was also pretty good here, but nothing like what it was before.



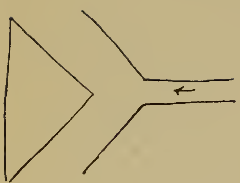
6470. I considered that the water level within would want adjustment, so filled up the globe with distilled water, replaced the tube and cork as it was, and let the steam blow off the excess of water. It was now much better than before, but not so good as when same tube in same position was fixed in the narrow mouth piece.

6471. So all these good tubes are good with the particular mouth piece.

* [6466]

† 6469]

6472*. I have had a cone apparatus constructed, conceiving it would be good to send stream of water particles against a piece of metal or wood, etc. on which they should beat and then be driven off. All was at first of brass: *a* is the steam passage, screwing on to mouth piece *c* (6440) of the steam globe, terminating in a funnel *b* having an angle of 90°; *c* is an arm and screw, holding a cone *d*, having also an angle of 90°, which could be made to approach and receed by the use of the thumb screw.



6473. First the cone was far back from the funnel and the effect was excellent, as good as any thing I had had. Sound very rattling. There was also sudden increases of rattling and then sudden increases of the Electricity developed.

6474. Screwed up the cone nearer and nearer to the funnel. The effect continued good until the cone came pretty near the funnel, and then the rattling diminished and also the electricity. Whenever suddenly the rattle changed into a hiss, the Electric development ceased and vice versa.

6475. When cone very close, there was only a hissing issue and no electricity. When returned to half open and quite open, the apparatus resumed all its power again.

6476. Increases of pressure greatly increased the power of the apparatus.

6477. Took the cone off, and though had good pressure, had *no* Electy. Put the cone on again and now fine developement. Value of the cone is apparently very great.

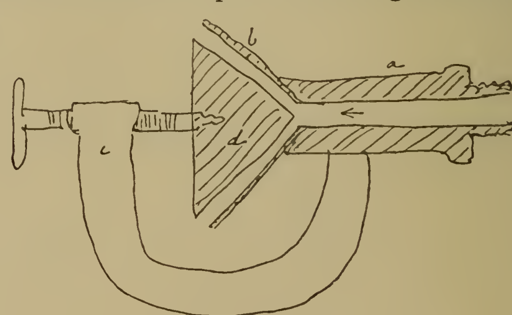
6478. Remember, all this is without the arrangement of the internal aperture, which is probably bad, as there is no effect without the cone. (6477).

6479. I had two other cones made of brass, one having an angle rather less than 90° and the other an angle rather greater. Used the flatter cone; was in all respects as the former, except that in certain position the issue changed quickly from rattle to hiss and hiss to rattle, and the corresponding developements of electricity beautifully seen by the Electrometer.

6480. Used the acute cone—was the same as the two former.

6481. There is more electricity evolved when the cone exit is much open than when little open, even though rattle in both cases;

* [6472]



but remember that then also more steam and water passes, and that ought to cause a corresponding increase in some degree.

6482. Without the rattle there is nothing, and that appears to depend upon the manner in which the water is either thrown against the metal, or carried through the steam passage smoothly without touching it. Hence the value of those forms of tube which determine the manner of supply to the inner end (6457, etc.).

6483. It is probable that the adjusted tube inner aperture and the cone outer aperture may give a still better result.

6484. I stuck a little piece of brass tube to the steam globe end of the cone apparatus, to effect this in part. After this addition, the apparatus generated about as well as before when cone wide open—but better than before when cone closed up.

6485. This junction of cone without and tube within gave us more changes from hiss to rattle, and the correspondant effects of electric evolution were beautifully seen.

6486. Now proceeded to try different forms of tube as to length, etc.

6487*. Used a long flint glass tube of same diameter as the former (   ) of an inch and   inches in length[1]. It was not so good as the first flint glass tube (6443). That tube was as good as the cone.

6488. Used a short thin brass tube, same diameter as the others, but notched at the outer end to favour the rattle; but observed no good effects nor any thing particular.

6489. Having a metallic tube issue, I held a glass tube, closed at one end, as a pandean pipe before it, to try if sonorous vibration would effect as the rattle does, or would induce favourable condition of the issuing column of steam and water—but could find no effects any way.

6490. Turned an insulated metal communicating piece (connected however with the boiler by a wire) into a mouth aperture and steam whistle, and obtained very high and strong sound—but now had a diminished effect of excitation. In this case, some of the steam or water excited in the passage of the tube was probably discharged again in this mouth piece or whistle, which would act more or less as a *gatherer*.

[1] The drawing reduced to $\frac{3}{4}$ scale.

* [6487]

6491. These sonorous vibrations seem to do no good.
6492. When silk rubs silk, which is P. and N.? Is that which moves quickest P., as in water case—and is it so always?
6493. Used $4\frac{1}{2}$ gallons of water from the boiler in this day's experiment.

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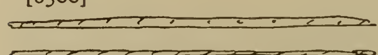
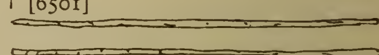
6494. Boiler hot, etc. Found the insulation very bad and traced it at last to the conducting power of surface of the insulating pieces of shell lac, which had been exposed to air on the ground under the feet of the boiler. These being well wiped—all was in order again.
6495. The metal tube (6463) good as before (6463).
6496. The Glass tube (6443) Do.
6497. The soaked wooden tube (6439, 6453) admirable.
6498. The soaked quill (6454); little or no effect.
6499. A dry quill—the same.
6500*. A new thin wood tube in a cork not so good as the old wooden tube (6497); when the rattle sound on was the best.
6501†. Ivory tube in a cork, etc. gave nothing.
6502. A round metal tube with the end flattened, thus‡: quiet issue and no effect of excitation.
6503. Now worked with the cone apparatus, without the tube which has been constructed to fill its steam way and allow adjustment of that passage—so that steam way larger than before (6472).
6504. With metal cone (6473) as before, very good effect.
6505. Wooden cone, same size as metal, produced admirable effect; good effect with comparitively low pressure, and when pressure up, the electrometer leaves opened rapidly and could procure good sparks every half second. This best of all as yet.
6506. Wooden cone (boxwood) and globe empty of water. At first an effect of excitement, but when passages and cone hot, then little or no effect; for *no water* to rub.
6507. Put water distilled in steam globe—now full exciting effect again. Beautiful.
6508. Put a small crystal of sul. soda into water in the steam

* [6500]



† [6501]



‡ [6502]

globe—after a few moments, when it dissolved and diffused through the water, no excitement.

6509. Washed out the solution and put in distilled water—was good again.

6510. Left distilled water in steam globe, but dropped distilled water on to wooden cone. Then steam let on; instant good effect and continuous.

6511. Dropped sol. sul. soda on to wooden cone, distilled water being in the globe—turned on the steam and now no excitement. *But* as the continued steam and water issued forth, it gradually washed off solution from cone, and as that washed off, the exciting power returned. Beautiful.

6512. Distilled water on cone again—not interrupt the evolution.

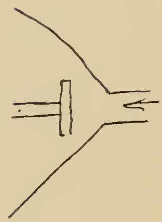
6513. Sol. of common salt acted exactly as the sul. soda solution described above (6511).

6514. Common water on the cone does not shew its bad effect because soon washed off, but in the Globe I think it did. Must try it in the globe again.

6515. Took the wood cone off; then effect was very feeble.

6516. Put the small brass cone on; did excite moderately, but not so good as the large cone. The large cone appears to be so good because it best disposes of the Pos. excited steam. If that touches other parts of the apparatus, then it is discharged by them and lowers the effect on the boiler.

6517. Small brass plate on: when nearly up to steam issue or when as far back as possible—no excitement. At the middle distance did excite, but nothing like so well as full sized cone of metal.



6518. Put the tube (6503) into the funnel steam way. Effect with large metal cone on much worse. When tube away and steam passage free open, very good again.

6519. Lined the steam passage of the funnel part with a *quill*, not letting it project on either side, and had a wood cone in place. In this case the excitement excellent. The quill must have nullified all effect there, but the excitement is in fact on the cone.

6520. Covered the wood cone with a stretched layer of caoutchouc—as good as ever. Observe that the caoutchouc was thoroughly wet on its surface.

6521. Oiled a cone of wood on the surface. It excited well, but on examining it, found the oil driven in and the surface thoroughly wet—hence the effect.

6522. Used four gallons of water in these expts.

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6523. Have reinsulated the boiler—all in excellent condition.

6524. Glass tube (6443): good excitement. *Neg.* as before, all right; distilled water in the Steam Globe, which was the former round globe (6440).

6525. The old thick soaked wood tube (6439): admirable excitement. *Neg.* always, counting the boiler state.

6526. The smaller wood tube in cork (6500): good and *Neg.*

6527. The Ivory tube has been in water since the 5th (6501). Gave at first nothing, then a very little only and that *Neg.*

6528. The soaked quill (6498) better than Ivory and *Neg.*; by use the heat of the steam made it dryer, and then it was worse and nearly nul.

6529*. A flint glass tube with a quill outer termination—effect moderate and very nearly the same whether the quill on or off.

6530†. Another flint glass tube with a quill outer end—moderate action whether quill on or off. The quill does not destroy the effect of the tube of glass.

6531‡. A quill tube with a glass external termination. No excitement, i.e. the quill tube seemed to gain no power by the addition of the glass termination.

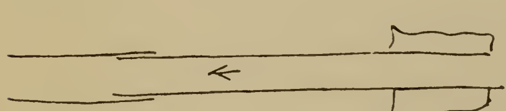
6532§. Lined a glass tube with a close spiral of brass wire from end to end, the inner diameter of the spiral being about the inner diameter of the former tubes (6443). This excited very well and *Neg.*

6533. A wooden tube (6500) has been boiled in Olive oil until water expelled, and then left to cool under the oil. This tube, mounted in a cork, etc. and steam sent through, was at first *nul* in action; but on continuing the steam, it gradually gained power and in a little while became very good, when there was rattling

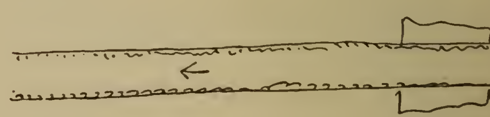
* [6529]

† [6530]

‡ [6531]



§ [6532]



issue. The boiler was *Neg.*, and when the tube was examined, it was found that the steam and water had displaced the oil, and that in fact within the tube it was a *wet wooden tube.*

6534. A similar wooden tube (6500) has been heated under melted sulphur until all the water was expelled, and then left to soak in the hot sulphur, and at last withdrawn [illegible] and coated with sulphur and allowed to cool. This tube excited as well as the best when there was rattling issue of steam, and made the boiler *Neg.* as before (6879).

6535. A metal tube with holes in the side at one end. If this end outwards, the excitement not so good as usual, but nothing remarkable. When holes and end were in the steam globe (the tube being turned end for end), still nothing particular.

6536. Now put the cone apparatus (6472, 6544) on to the steam Globe.

6537. The wood cone as before (6505): admirable and boiler *Neg.*

6538. Metal cone (6472) as before: also good.

6539. Have had a wood cone boiled in oil and allowed to cool and stand in it 24 hours. This cone, when wiped and in place, was at first *nul* in effect. But as steam issue continued, it gradually gained power and was as effectual as a mere wood cone. When taken off, it was found that the wood at the apex of the cone and about ½ inch from it was quite wet and soaked with water at the surface; the rest oily. This had made it equivalent to a wooden cone.

6540. A sulphur cone was an excellent exciter and made the boiler Neg., just as wood did, but it was not so powerful as wood.

6541. The Air pump aperture of the Globe produced weak *Neg.*

6542. The widest issue aperture of the globe gave weak *Pos.* (cause of this perhaps oil; see forward (6570, etc.), for white lead had been used here).

6543. *Supply Apparatus.*

I have had an apparatus made by which I could supply liquids to the steam channel at pleasure, so as to use water with a steam globe empty of water, or oil, etc. The kind of apparatus is figured on the next page[1] and is easily understood by inspection.

[1] i.e. at par. 6544.

6544*. *s*—steam and water globe.

a—a mouth piece to which, by a connecting screw *h*, is attached

b—the steam passage piece.

c—a stop cock with cap above, into which a tube is fixed by a cork.

t—the glass tube (conical) for water, oil, etc., 10 or 12 inches high.

e—the cone apparatus, which can be attached at pleasure to *b* by the connecting piece *d*.

All these parts are of brass, except the tube, which is of *glass*; its *cork*; and the cone *w*, which is of *wood*.

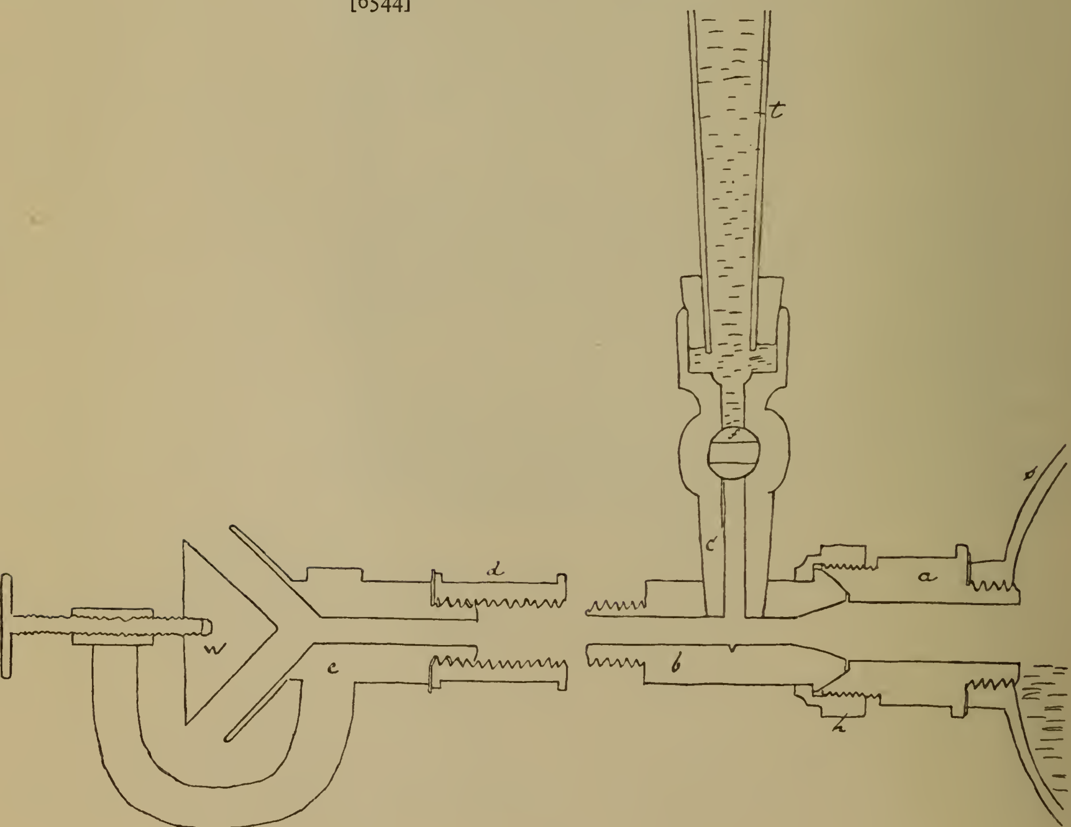
The drawing is the size of the original[1].

6545. Can easily insulate the exciting cone.

6546. First the parts *a*, *b* and *c* were attached in place to the steam globe, the latter having its proper supply of water, and the stop cock *f* was shut. The apparatus was now a mere steam passage, and it excited moderately, as a mere tube would do, making the boiler *Neg*.

[1] Reduced to $\frac{7}{10}$ scale.

* [6544]



6547. Then put on the cone apparatus at the end (6472, 6544), attaching it by a common adapter, and now ascertained the effect of the cone of wood under these circumstances. It was very good, making the boiler Neg., and so far accorded with former experiments. Shewing that the steam passage had nothing particular in its mere action but served as a tube only.

6548. Emptied the steam globe of its water but left the wood cone, etc. as just described (6547). At first there was good excitement (6294, 6374) whilst the parts able to condense water. But when all raised to full temperature, then the cone, etc. produced very little. There was some effect, because the apparatus being uncloathed, there is always some water produced and hurried on through passage, but it was very little by comparison with effect when water was in the globe.

6549. Now tried the effect of distilled water in the tube *c*. When water was in this tube (14 inches long) and the cock *f* open, the steam being shut off, the water instantly ran through, part probably going into the steam globe and part running out at the mouth of the apparatus. But when the cock *f* was opened *after* the steam was turned on from the globe to the apparatus, then this water descended very slowly. There was a series of rapid small concussions due to condensation at the contact of water and steam, and the consequence was that the water descended as slowly as I wanted it, supplying by successive small portions that fluid to the interior of the steam passage, to be carried forward by the steam and driven against the cone.

6550. The consequence was that excitement was now abundant, though there was no supply of water from the steam globe.

6551. On shutting off the water in pipe *c*, after a few moments the evolution of Electricity fell nearly to nothing, although the steam was issuing. On opening the communication for water, the excitation rose again greatly.

6552. Observations. The supply of water is probably not the best in its kind for the tube: it runs into a little channel cut round the inside of the steam passage to distribute it, and then is carried forward.

6553. The water cools the brass piece *c*, and that alone makes water condense there and so causes excitement. Probably water might be better supplied by making part of the steam channel thin and condensing there a portion of the steam within.

6554. So the effect of water thus supplied is very evident and good.

6555. Now tested common water, putting that in the tube *c*; immediately the apparatus lost all power of generating electricity (6410, 6411).

6556. Put distilled water into *c* and let it in. In a few seconds, i.e. as soon as the common water was driven away, up rose the exciting power and was as good as ever.

6557. Common water in again—down went the excitation.

6558. Distilled water—again brought it up as before—BEAUTIFUL.

6559. Used a mixture of half alcohol, half water—was not so good as distilled water, but excited; and boiler *Neg.*

6560. Used good *Oil of turpentine* in supply tube *c.* It descended as the water did and gave good excitement, but to my delight the boiler was *Positive.* Good sparks, deflexions, etc. etc. etc. etc. and all *Positive.*

6561. Thinking that by this time there was water in the steam globe, I turned off the oil of turpentine and continued steam issue. Excitement soon came on in a very excellent degree, but boiler was *Neg.*, as for water only.

6562. Let down a little oil of turpentine and then put on steam. The first excitement good and *Pos.* This continued for a while, perhaps a minute, gradually decreasing, and then Neg. excitement came on as before.

6563. Let down continuous supply of oil of turpentine and then had a continuous and strong *Positive* excitement in the boiler. So can easily obtain *Pos.* and *Neg.* when wanted.

6564. When the apparatus was giving Pos. state, I stopped the steam for a moment and poured oil of turpentine over the surface of the *wooden cone only*; then turned on steam; but the earliest specimen of electricity I could take was *Neg.* I incline to believe however that for a moment at the very beginning it was *Pos.*—but before that could tell upon the whole boiler it had become neg. by the driving off of the Oil of turpentine.

6565. Letting a little oil of turpentine descend into tube by *c*, however, immediately made state *Pos.*

6566. A new and dry wood cone was soaked in oil turpentine and then put in place, but still the first electricity I could take was *Neg.*, and the cone when taken off was wet with water.
6567. Steam and water easily and instantly displace oil of turpentine from wood. Hence the surface is instantly a wet surface.
6568. It is very convenient to be able so freely to clear out the oil of turpentine by the steam, etc. of the issue.
6569. Now passed distilled water through *c* and brought all to its *negative* aqueous condition.
6570. *Olive oil* into *c*. Let a little into the steam passage, then put steam on; there was good excitation and boiler was *Pos.* Continued the steam and by degree the excitement was reduced and changed to *Neg.*
6571. Let the *Olive oil continue to run in*, and then excitement contnd. Pos. and strongly so, as strongly as with Oil of turpentine.
6572. After this, putting water in *c* and letting it in to steam passage, I found it difficult to clear off this *Pos.* state and that state returned. Found that some of the oil had flowed back towards the steam globe, and that passage was nearly permanently *Positive*. Very Good.
6573. Took off wooden cone; still Pos. excitement. Left only piece *a* and *b* and in place of *c* inserted a cork; still this proved to be a good *Positive* excitor.
6574. Put on another wood cone: still *Pos.* and powerful.
6575. Put a little Olive oil in the steam Globe and now found it always gave Pos. with metal tube.
6576. The oil and the water become milky together and very little oil appears to give the water a Pos. power.
6577. When wood cone off, still excites *Pos.* With pure water it is *nul* in this state.
6578. The mere flint glass in cork was a *Pos.* excitor to boiler when the oil was in the globe.
6579. Even the Soaked wet wooden tube was *Pos.*
6580. Drew off water from steam globe—a few drops of oil came out, but no doubt some left in—and now with no water in steam globe, used the old wet wood tube. Whilst cold, i.e. at first, excited *Pos.* Again a sample of Electy. was *Pos.* But when all

hot, so that no condensation of water in steam passage, then was *Nul.*

6581. Hence *water is needed* in these excitements as well as in the former.

6582. Still, if allowed to cool so that on first rush of steam an effect may be produced, that effect is *Pos.* The *Pos.* state is now very permanent—and valuable.

6583. Put on cone apparatus (6472, 6544). When hot, the effect *nul* for want of water. Stopped steam—cooled cone passages and then let on steam—then first effect *Neg.*, as it ought to be for pure water.

6584. Introduced a little oil at the steam end of the cone passage and now it gave fine *Pos.* effect as its first effect; passing to *Nul* as the heat rose and all became water dry.

6585. Quantity of steam used $5\frac{1}{2}$ Gallons.

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6586. Of the two *Syringes* for condensing air, they send through the following amounts of common air at common pressure on *both sides*—each stroke

Large syringe—8 cubic inches
Small syringe—3 cubic inches.

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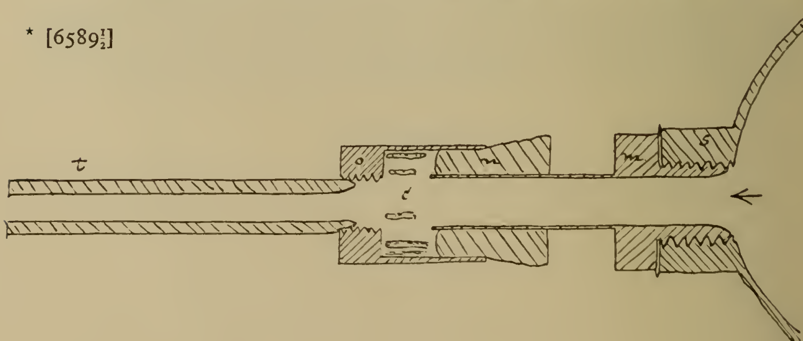
6587. Steam used this day was 6 Gallons.

6588. Had the steam globe on to-day as before.

6589. Connected it with a Marcet's boiler to find the pressure of the steam. The utmost pressure I have used up to this time has not been more than 13 inches of mercury—not half an atmosphere above atmospheric pressure; the usual force has been from 10 to 13 inches of Mercury.

$6589\frac{1}{2}$*. Now used this apparatus: *s*, steam globe; *m*, a mouth piece terminating in a thin piece of brass tube inserted into the *cork n*, which itself was inserted into mouth of a retort cap *o*, into which could be screwed the wooden tube *t*. The retort cap

* [$6589\frac{1}{2}$]



had been thoroughly cleaned by hot sol. of potash and water, and the arrangement produced a little chamber *c*, easily accessible by taking *o* of[f] the cork, and into which I could introduce liquids, etc. at pleasure. For this purpose I used pill box lath: rolling a piece up into a spiral, putting it into the chamber *c*, and then fixing *o* upon the cork *n*. These spirals were either moistened after they were in their place with the oil, oil of turpentine or other liquid to be used, or else, as with Sulphur, Wax, etc., prepared before hand by being dipped into a little of the substance made hot in a capsule.

6590. I found that any liquid used in the chamber *c* could be quickly and thoroughly cleaned out so as to restore the apparatus perfectly to its first or clean state.

6591. First, with nothing in chamber *c*, found that the old wet box wood tube used as *t* was as good as ever and *Neg.*

6592. A new dry beach wood tube (smaller in bore than old box wood) gave scarcely any thing (water was of course in the steam globe). But a similar tube wetted, much better and *Neg.*

6593. A similar beech wood tube boiled in distilled water and left in since yesterday, better still and good, but not so good as the old box wood (not so wide in bore).

6594. One made of more open bore—better still—but old wet boxwood stands first. All gave *Neg.* in boiler.

6595. Arranged a discharging electrometer with the Gold leaf, and it does exceedingly well to tell cases of very strong excitation, by the number of unit discharges in a given time.

6596. Now began to introduce substance into the chamber *c* on the coils of box chip.

6597. *Oil of turpentine.* First effect strong and Pos.—second observation Pos. but weaker—next Pos. a little—observed the Electrometer leaves and they went gradually down, for a moment were collapsed, and then opened quickly, and found them now permanently and strong Neg. No further change. On opening chamber *c*, found all the oil of turpentine dissipated and the chip, etc. thoroughly wet; box free of smell.

6598. This a beautiful case of power of producing *Pos.* state and the disappearance of the state with the disappearance of the substance.

6599. Put a little more oil of turpentine in to same wet chip and had the same succession of effects.

6600. *Camphor*. New chip, and lodged pieces in and between it of the camphor. The Electy. was *Neg.* from the first and continued so. Yet camphor came over with the steam. After 4 or 5 observations there was still camphor in the box—continued to send small stream of steam through and found *all* camphor quickly volatilized. So camphor is not as Oil of turpentine, but *neutral* apparently.

6601. *Pyroligneous ether* and new chip. No electy. for a time and then rose up *Neg.* Put more Pyroligneous ether on to the same chip. As before. No electricity at first and until all the fluid driven off, and then Neg. as with water. So this prevents excitation. Does it do so as a saline solution by its conducting power?

6602. *Liquid ammonia*. First *Pos.* for two or three electrometer observation[s]; then became Nul—then rose *Neg.*, as if ammonia had power to produce *Pos.* whilst present.

Again, *Amm.* on to same chip. Now *Neg.* from very first.

New chip and *amm.* Neg. from the first.

Turned the cork *n* end for end and then *Amm.* on to last chip—was now Pos. over and over again, but at last, by continuing the steam, it became *Neg.* There is therefore something here which can be exhausted, but the ammonia must have been gone long ago and it looks rather as if the cork end had supplied it.

Fresh ammonia, all the rest being unchngd. Excitement was small, but *Pos.*

6603. Used Good box tube and ammonia—at first Nul; then power got up and always *Neg.* Repeated the expt.; same results.

6604. Remember that a substance may shew Pos. power by making the natural effect of water alone *less Neg.*

6605. *Acetic Acid.* Old box wood tube and New chip in *c*. Was *Neg.*; then put in little strong acetic acid; effect was Nul steadily and for some time and then gradually rose up *Neg.*—weakly. Stopped off steam a while—a minute or two—then on again. Nul for a short time, because of Acetic acid in the wood of chip, which in that time came to the surface; and then, as that acid dissipated, effect again came on *Neg.* So Acetic acid takes off power of pure water.

6606. *Olive oil* on chip. At first *Pos.* strong and for some time, then fell in power—became *nul* and then rose *Neg*. Put a little more oil in. Some effect again and again. Beautiful.

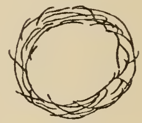
6607. Stopped the steam a while and then put it on—was *Neg*. at once, shewing that the oil was all cleared out. On examining the chamber *c*, no signs of oil there.

6608. *Hogs lard*: as oil. Pos., then Neg.

6609. *Spermaceti*: as oil. Do.

6610. *Yellow wax*: as oil. Do.

6611. To obtain a longer *Pos*. state, soaked a thick string in melted yellow wax; made a coil of one or two rings of it and put that in the box; was now Pos. for a long time. Did not stop to exhaust it.



6612. When these things are used with chip, long before they are dissipated the Neg. state comes on, for they are driven by the heat, water and steam into the inner dry parts of the wood. Then the surface is not an oiled but a water surface and the effect is that of water alone.

6613. Water with steam easily displaces Oil and such like things from the pores and fibres of wood.

6614. *Sulphur* on chip. Neg. from the first—does nothing.

6615. *India rubber*—a piece held in by chip circle. Does nothing, i.e. was Neg. from first. It wet just as wood does, *thoroughly*.

6616. *Some India rubber* heated in candle so as to melt and smeared over chip. Neg. from the first. So though rendered a liquid and a hydro carbon, has not the effect of oil or oil of turpentine.

6617. *Caoutchoucine*. This liquid was so volatile in part that on dropping it into the chamber *c* whilst warm, much boiled off at once, frothing up; but trusting that the wet string ring in the chamber retained the more fixed parts, it was tried and was *very Neg*. from the first—not Pos. as oil of turpentine or sweet oil. It was certainly more *Neg*. than water alone.

6618. Cleaned out the chamber, put in fresh chip and on it *oil of turpentine*. Was as before. Well *Pos*. awhile—then neutral and then *Neg*. Dropped in a little caoutchoucine and instantly *Very Neg*., more so than water and *very continuous*.

6619. *Olive oil* again; fresh chip; surely and well *Pos*. Good.

6620. *Rosin oil* (so called): strong *Pos*.

6621. *Castor oil*: strong Pos.

6622. *Laurel oil volatile.* At first well Pos.; continues some time, then goes down; becomes *nul*; then expands Neg. Fine exhibition of the order of Phenomena; found all oil dissipated.

6623. *Alcoholic sol. of camphor.* At first Nul: after a time, when spirit and camphor off, was *Neg.*

6624. *Sol. Potash* on chip. Nul—destroys effect—easily cleared out again by washing and probably also by continuing steam.

6625. *Sol. Sul. Soda. Nul*—destroys effect—easily cleared out by washing.

6626. *Sulc. acid.* 1 drop to 1 oz. of distilled water—a drop of this on chip made apparatus *Nul* for ½ a minute or more—then gradually washed away and Neg. effect came on. *Good.*

So far for these kind of expts. at present.

6627*. Tried this apparatus: *r* is a thin brass tube fixed on to the steam globe and passing water tight through corks and the little cistern *c*; *t* is the usual wet exciting wooden tube.

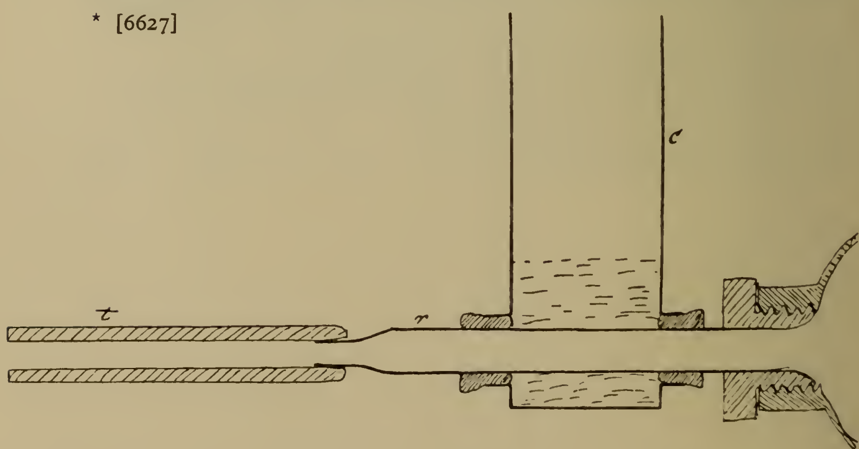
6628. Emptied the steam globe of water and then sent steam through when all was hot—there was little or no electricity produced for want of water. Poured some alcohol into the cistern *c*; this soon heated up and began to boil and of course produced water on the inside of the tube *r*. But this water was not enough or not effectual, for the excitement was poor.

6629. When the steam Globe was *replenished with distilled water* and the apparatus above restored to its place, then the excitement was very fair, far surpassing what it was before.

6630. Probable that, as steam sweeps over water in the steam globe, it take[s] up and carries on globules of water; and this a more effectual form than sweeping over the inside of the tube from the condensing place.

6631. Now fixed the Ivory tube (6501) into the steam globe to have as neutral a jet as possible, and by an insulating stand opposed various cones before the jet, collecting on an electro-

* [6627]



meter the Electricity of the *cone*; the proper proportion of water being in the steam Globe, but no liquids or any thing but water and steam in the steam jet.

6632. New *beech* cone: Neg. and Strong.

6633. *Thin oiled silk*: 4 thickness over a cone . Neg.

6634. *White silk* on cone (instantly wetted) . . Neg.

6635. *Kerseymere* on cone (Do.) . . . . . . Neg.

6636. *Japanned leather* on cone . . . strong Neg.

Did not wet through the Japanned leather.

6637. *Linen* on cone: instantly wet through strong Neg.

6638. *Brass cone* . . . . Neg.

6639. The *wet white silk cone*, covered with *olive oil* . well Neg.

6640. Do., moistened with *caoutchoucine* . . . . Very Neg.

6641. *Oiled silk cone*, oiled with *Olive oil* . . . . . Neg.

6642. *Sulphur cone* . . . . Neg.

6643. So all these neg. No positive; but probably very different when the stream of steam carries the oil, etc. etc. etc. with it. If not, ascertain why.

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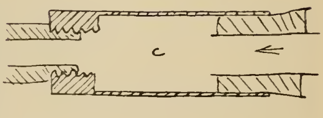
6644. Used 5 Gallons water this day.



6645. Boiled all right; insulates well. Steam issue arranged as on 17th (6589), but used for a chamber a longer and narrower retort cap, and instead of chip box to hold the fluids, have washed a piece of thick sail canvas in distilled water and now use that. Is a good material for the coil (6589).

6646. Canvas alone in chamber *c*—the old wet box tube being the termination. Excites well and *Neg.*; so canvas alone makes no change.

6647. A slip of new cork put into the box. *Pos.* a little at first moment, then afterwards always *Neg.* So apparently a little of something in cork volatile and causing *Pos.*, but that quickly exhausted.

6648. *Liquid Ammonia* on to that slip of Cork: first *nul* for a time, and then when Ammonia gone, *Neg.*

6649. Canvas in box—Sol. Ammonia—first *Nul*—then gradually rose *Neg.*

6650. *Oil of turpentine* on same canvas wet. First strong Pos.—

then Nul–then Neg. Oil turpentine on dry new canvas–same thing. Pos. once or twice–then Nul–then Neg., when Oil of turpentine gone. Change soon occurs (6597).

6651. *Oil of laurel.* Same and as yesterday (6622).

6652. *Strong Alcoholic Solution of Camphor. Pos.* for a moment or so, then quickly *Nul*–and *Neg.*

6653. *Alcohol* alone. Gives weak *Pos.* at first; this quickly gone and then permanent *Neg.*

6654. *Camphor* alone in particles and dust. Perhaps least trace *Pos.*, but appears *nul* for some time and then Neg. gradually came up to full extent. Camphor is certainly below Alcohol in making state *Pos.*

6655. *Sulc. Ether.* First *nul*; then quickly *Neg.*

6656. Boracic acid in crystals, etc. *Nul* at first–then Neg.–box found empty.

6657. *Sulphur* sublimed–washed in weak ammonia–then in two or three distilled waters. The wet paste put in and about the coil of canvas in the old large box *c* (6589). The state Neg. from very first, and very strong–more so certainly than water alone.

6658. *Naphthaline* in crystals, on new canvas. Neg. from first and very strong. Sent steam through till all smell of Napthaline gone, and then the canvas alone as good and as Neg.

6659. *Rectified Naphtha*, so labled. Believe it is German Naphtha distilled. *Neg.* from first and very strong–stronger than water alone. Charges electr. very quickly–frequent sparks–rather continuous.

6660. *Naphtha* (the previous before rectification). First *Pos.* a moment–then nul–then Strong Neg., continuing when the Naphtha gone. Repeated with same effect.

6661. *Rectified Naphtha again* (6659). When Plenty of it on the canvas, was same result, i.e. first Pos. a moment, then *Nul* and then *Neg.*–and the Neg. very striking and good.

6662. Is this residual *Neg.* only the water state cleared to its best condition, or is it a direct effect of part of the Naphtha?

6663. *Sulphuret of Carbon.* Very Neg. from first moment–more than after–as if it had Neg. effect, but that gone instantly.

6664. *Strong Acetic acid.* Quite Nul for some time, as Sul. soda, etc., and then gradually *Neg.* rose up as the acid washed away.

6665. After this, washing out the box and tube and using a new canvas restored ful[l] *Neg.* effect, shewing that could clear residue of its effect away.
6666. *Condensed oil gas liquor. Pos.* for some time and strongly—then fell—*nul*, and rose *Neg.* This effect again and again.
6667. *Caoutchouc melted* in a flask: thick black fluid. This smeared on inside surface of canvas ring. Very *Neg.* from first instant and continued so long time. On opening the box, found the matter melted and dispersed about, but plenty of it here and there within, on cork, etc.
6668. Distilled some caoutchouc, collected about $\frac{3}{4}$ first, and then the denser last portion by itself.
6669. Caoutchine (the first portion (6668)). First instant *Pos.*; then quickly Nul and *Neg.* Again, first *Pos.*—then *nul* and *Neg.* If steam let on more moderately, at first the *Pos.* more distinct. If rapidly, the first Pos. is often lost in final *Neg.* state.
6670. The second portion (6668). First *Neg.—then Pos.*—then nul and Neg. When rattling came on well, the Neg. state suddenly started up. The supply of water has to do with these feeble states and is more influential in changing them than with the stronger states.
6671. *Melted caoutchouc again*—as before.
6672. Removed canvas and put in fresh canvas with oil of turpentine to clear out all Neg. influence of the Caoutchouc. Was first *Neg.*, from caoutchouc in tube probably—then Pos. from its displacement and effect of oil turpentine—then after a time *Nul*—and Neg. as usual.
6673. *Resin, common, in Alcohol.* On dry canvas—*Pos.* from first, strongly and very continuously. On opening the box found the rosin melted and sticking about. Took out the canvass, but the cork and cap alone made state strong *Pos.* Put in a New cork—still strong Pos. Used a new wood tube—still strongly Pos.—so the resin adhering to the box enough to do all this. Washed the box with Potash, etc. etc. After this, with either the New or the old wooden tube was *Neg.* So now in natural water state again.
6674. *Olive oil* in canvass. Pos. from first; continuously and well so, but not so high as Rosin. Took out canvass and wiped

box well—still Pos. for a while, but at last all displaced and it took its natural Neg. state.

6675. The following are the general states produced by the different bodies tried, using a wet wooden tube as exit. But must remember that a body which makes a weak Neg. state of boiler, or Nullifies, may do so by having itself a real *Pos. power*. The states are those of the *tube and boiler*. Of course the issuing stream had the opposite state—and this will much depend upon whether the body used is more or less volatile than water, i.e. whether it is in globules sticking to the wood, etc., or in vapour passing through it.

6676.

Excite *Neg.* Electy.	Nullifiers	Excite *Pos.* in Boiler
Water alone	Ammonia	Oil turpentine: transient
Sulphur	Dil. Sulc. acid	,, laurel: do.
Naphthaline	Sol. Sul. Soda	Camphor: tr. and weak
Naphtha } ?	,, Potash	Alcohol: tr. and weak
Rect. Naphth. } ?	,, Salts certain	Naphtha } ?
Sul. Carbon: trans.	Sulc. Ether	R. Naphtha } ?
Melted Caoutchouc: perm.	Boracic acid	Oil gas liquor: tr.
	Acetic acid	Caoutchoucine 1st } ?
		Do. 2nd } ?
		Resin in Alcohol: *perm.*
		Olive oil: *perm.*

6677. Now endeavoured to ascertain whether any changes in the substance of the thing struck or rubbed by the stream would be affected by substances put into the box; or these again affected in their result by nature of the thing rubbed.

6678. So removed the wooden tube (6645) and replaced it by the *ivory tube* (6501). Tried this by itself and found that it made the boiler Neg. and pretty well, but not as wood tube.

6679. Put *oil turpentine* into box *c*. Made tube and boiler *Pos.*, as with wood tube, and well. It remained Pos. longer than with wood and gradually sank to *nul*—opened a very little *Neg.*, but was almost *nul* for long time, as if tube brought to its first state. Now it had been oiled before putting into place above (6678). Perhaps higher pressure might bring out the Neg. state.

6680. *Rectified Naphtha* and ivory tube. First Pos. moderate—then *nul*—then Neg.—as with wood (6661).

6681. Cleared out the box *c* and placed insulated cones before the jet from Ivory tube, connecting them and not the boiler with an electrometer, to learn their state.

6682. Ivory jet with water and steam alone gave the following states to the insulated cones put opposite, and these states are I believe the result of friction against the cones and not of the cones acting as dischargers to the issuing stream; that would give the contrary state.

Brass cone	Neg.
Wet beech cone	Neg.
Sulphur cone	Neg.
White silk cone (wet)	Neg.
Kerseymere (wool) cone wet . .	Neg.
Japan leather cone	Neg.

So all these different substances are made *Neg.* alike by the stream of steam and water.

6683. I have covered one wood cone with 5 or 6 thicknesses of flannel so as to have a cone which the jet of steam, etc. should rather penetrate into than rub over. Here friction may be said to be removed in part, but here also the particles excited by friction against the outer layer of wool may be discharged again against the inner layer, and so diminish effect. However

The cone of many wool thicknesses—Neg., but very little so.
„ „ one wool thickness—Neg., much more powerful.
„ „ one silk thickness (wet but close)—Neg. very powerfully.

Hence difference, but either cause would produce it.

6684. Now had Ivory tube (6678), but some *Olive oil* on Canvas in the box *c*. The jet now gave the following states to the cones:

Metal	Pos.	So all these different substances are *Pos.* with *oil* in box *c*.
Wet wood . .	Pos.	
Sulphur . . .	Pos.	
Silk	Pos.	
Japanned leather	Pos.	
Metal again . .	Pos.	

6685. With *Acetic acid* in box *c* gave following results:

Metal . . .	Nul	all alike and *Nul.*
Wet wood . .	Nul	
Sulphur . . .	Nul	
Silk	Nul	
Japanned leather	Nul	

6686. Sol. Rosin in Alcohol in box *c* gave following results:

Metal cone . .	Pos.	all Pos.—some of the rosin carried forwd. and deposited on the surface of the metal cone and wet wood cone.
Wet wood . .	Pos.	
Sulphur . . .	Pos.	
Silk	Pos.	
Japanned leather	Pos.	

6687. Changed the box *c* and put a new one with nothing in it, to see if the last cones would give a state with a nul stream. Had the ivory tube in. First tried the boiler state and found it *Nul*, so suppose the stream neutral. Then the jet sent against the metal cone with the rosin on it made the cone feebly *Pos.*

6688. Against the last wood cone—this became feebly Pos.

6689. But on using a new wood cone which would have been *Neg.* to a usual stream, it was also feebly *Pos.*, as if the tube had in these cases retained a little resin and so acted to the cones as the box before acted to it.

6690. Washed out the Ivory tube with Alcohol. Then used it. It still made the boiler itself a little *Pos.*; it made the insulated wooden cone (wet) nul; it made the metal cone with rosin on it a little Pos.; and the same cone when cleaned a little *Pos.*

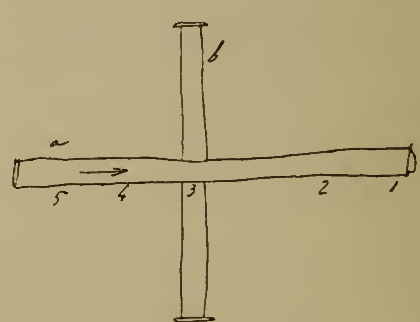
6691. Put a little melted caoutchouc on canvas into the box; made the cone *Pos.* but with very little power. Now, melted caoutchouc before made wood tube *Neg.* So here perhaps work out a case of 2 cones giving different states with the same jet.

Used 5 Gallons water to-day.

6692*. It is said that if two pieces of the same silk ribbon be drawn one across the other so that one has little friction over *all its surface*, and the other much only over *one* spot, that the states of the two pieces are different. Ought this to be and why, for as regards the rubbing particles, they should be mutual and alike in their action and reaction.

6693. A piece of good broad white ribbon well washed in soap and distilled water and afterwards in pure distilled waters was dried and cut down the middle and tried. Unless the friction was hard no excitement took place. When it was hard, the piece which moved and rubbed over the whole surface, as *a*, was *Pos.* and

* [6692]



the other, *b*, was *Neg.* Whichever of the two pieces was made *a* was Pos. and whichever made *b* was *Neg.* This distinct.

6694. 2 Pieces of clean flannel ditto. Quite neutral at first, but when rubbed, *both* became *Pos.* and much stronger than the silk. Is the air here rendered Neg. or where is the Neg. electricity gone to? (6938).

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6695. Experimented again with the ribbons and flannels (6692) and found the results quite unsteady and insecure. Thus all were clear at the beginning, but when *a* and *b* were silk, parts of both *a* and *b* were pos. and neg. Thus for instance, taking *a*, the part at 3 after rubbing was Pos.; but then parts at 2 and 4 were Neg., whilst the parts at 1 and 5 were neutral. Then *b* was found Neg. at middle but a little Pos. at sides. After another rubbing, each ribbon had places in different states, and without any distinct reference to the rate of rubbing.

6696. With the flannel, both were strongly *Neg.* on both sides as before. Here Air must have gone off Pos. Examine this further still presently.

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6697. Worked with boiler, steam globe, etc. Used $6\frac{1}{2}$ gallons of water in the whole. All well insulated. Put water into boiler once.

6698. Wet beech tube—excellent excitement and *Negative* boiler.

6699. Wet box tube, old—Do. Do.

6700. Dry beech tube. Pretty well and *Neg.* The wet by far the best. This probably because it conducts better to the boiler backwards.

6701. Had the retort cap on in these expts. (6645) as before, but with a rather narrower retort cap than then. Used the same in the following expts.

6702. *Dry beech tube* last night had inside filled with solution of rosin in Alcohol for 2 or 3 minutes, the part not soaked up poured out and the tube left to dry. So has a resinous inside. Excites very little indeed and that little Negative. Bad conducting power of the tube probably here effectual. Remember that here the rosin is fixed and cannot go with stream.

6703. A wet beech tube—the steam end dipped first about $\frac{3}{4}$ of inch into the sol. of resin in Alcohol, and then applied as jet. Was first well *Pos.* for two observations, then Nul as the resin carried away and then *Neg.* when no more resin to carry away. When examined, all resin gone. So the moving resin had become Neg., making the tube (wet) against which it rubbed Pos.; but when all gone, then the water resumed its power of making the wood neg.

6704. If water rubbing against wood is made Pos., i.e. tends to become Pos., and resin rubbing tends to become Neg., then resin rubbing against wet tube ought to give the highest state of Neg. to flying resin, and of Pos. to wet tube.

6705. Poured some Alcohol sol. of Resin through the tube to wet it well inside, it however being wet. Now *Pos.* for a long while, gradually decreasing—then *nul*, and after that rose quickly neg. As long as resin particles can be carried off they seem to have great power of exciting, i.e. very little seems to be effectual.

6706. Are not the exciting particles in that case globules of water, each with its film of resinous matter—and so with oil, oil turp., etc.? Know how easily a thin film spreads over water surface. Try balance expt. ( ).

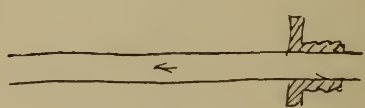
6707. *Wet beech tube.* A little melted caoutchouc smeared on inside of steam end, i.e. at end where stream of steam *enters*. Was Neg. from the first, and I think *better* than with water alone—then fell to natural condition. Again some more in—again same effect.

6708. *Wet beech tube* smeared with sulphur paste (6657) at the steam end. *Neg.* and excellent from very first and rather continuous.

6709*. Thin metal tube alone (6391). At first a dull effect till cleaned but then well Neg. Then a little sulphur in—was *nul* until sulphur gone (soon over) and then Neg. Sulphur has *no* effect here except to suspend or counteract the ordinary effect for a little time.

6710. *Wet beech tube.* *Outer* end dipped in Sol. of Resin in Alcohol. Is then either weakly *Neg.* or *nul* or very little *Pos.* But if the inner end be so dipped, then tube is *Pos.* and well so. So for resin to produce its effect, it has to travel over the wet part

* [6709]



of the tube. If it does not so travel, then effect is *nul*, and resin fixed at that outer part does not charge the boiler.

6711*. Have a *new Ivory tube*. Is nearly *nul*—a little *Pos.* perhaps due to the oil in the ivory as bone.

6712. *Ivory tube* and *Ammon.* in box *c*—first Pos. very little—then nul—then Neg. well—as if ammon. had little direct power, but had now removed the oil from the surface of the Ivory.

6713. Same *Ivory tube* as just left and *Boracic acid* in box *c*. Nul—after that Neg. when B. Acid gone.

6714. Do. and acetic acid in *c*. *Pos.* moderately and for a long time—as if the acid had set free a little oil or was itself *Pos.*, but was perhaps too continuous for that. So took out the Acid and washed the tube and box with distilled water. Now *Neg.* feebly for a while—then *Nul* and then rose *Pos.* again—as if the washing had taken off the bone state and continuance of steam had renewed it.

6715. *Ivory* or *bone* jet seems inclined to be Pos.

6716. *Dilute S. Acid* in box *c* with *Ivory tube* (6711). Nul and then a very little *Pos.*—less than bone alone before. Here the former usual effect of dilute acid.

6717. Same *ivory tube*—*sol. Potash caustic*. *Nul* and continues so—washed out the Potash, etc.—then in place with water only, and was *Neg.* a while, and fell to *Nul* again.

6718. Ivory *tube* in *this state*. Camphor and alcohol in *c* (6652) was strong *Pos.*, which fell to weaker Pos. and then *Nul*.

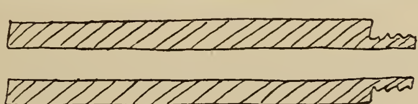
6719. Do. . . . . Alcohol in *c*—nothing—*nul*.

6720. Ivory or bone or quill tube probably easily made neutral by action of a little *Alkali*.

6721. Now experimented, using the Old *wet boxwood tube* (6699), the cap *c* just employed, and with a little solution of *sul. soda added to the water in the steam globe*. Was *Nul* as before (   ).

6722. *Same*, with *Oil turpentine* in box *c*. Pos. well instantly—then effect went down to *nul* as the oil dissipated. This an excellent effect: so when water deprived of its power by saline matter, still that of oil of turpentine shews.

6723. In this way get effect of bodies put into *c*, uncomplicated by the effect of pure water, i.e. by quill or ivory tube may have a normal jet with pure water and by sul. soda in water may have

* [6711] 

a normal jet with exciting tube, and in either case may perhaps have the effects of fluids put into box *c*.

6724. *Same*, with Olive oil in box *c*. *Pos.* from first and well. After a while, took out the oily canvass and wiped box as well as I could—then for the first moment effect was *Pos.*—but instantly after (the loose oil being driven off) the effect was *Nul.* Good.

6725. *Same*—with Sulphur paste in *c*. Was Neg. from the first and then fell to weak *Neg.*—but on washing out all sulphur found, when *c* clear, still was *Neg.* Believe that the water in the steam globe has been charged so far by that continually distilling over that now it alone begins to act. So put in more *sul. soda* and now again wood tube *nul.* Then put fresh sulphur into *c* and there was the least degree of *Neg.*, next to nothing—so think Sulphur Nul with Nul water.

6726. Added a little oil of turp. to box *c*—instantly well *Pos.*

6727. Whilst so, changed the cap *c* for a clean one—instantly, as expected, effect was *nul.* So here effect of Sulphur and oil of turpentine well separated from that of water—the former little or none, the latter very great.

6728. Same (i.e. sul. soda in Globe) and melted caoutchouc in box *c*—this with Old box tube *Pos.* again and again, not strong—with New ivory tube (   ), nul—with new wet beech tube, *nul*—with old box wood tube, *Pos.*—with metal tube, the least *Pos.*, rather nul. So took out canvass from *c* and cleaned it, then put back old box wood and it gave a small trace *Pos.*, as if something were in end of tube itself.

6729. Cleared out steam globe and left in distilled water—retained the box tube and chamber as they were in last expt.—was Powerfully *Neg.* from very first—finest we have had.

6730. So water is really very Pos. in its force and greatly opposed to all the other bodies tried. Melted caoutchouc I think has a little Pos. power here, whereas it was thought to be Neg. before.

6731. Now melted Caoutchouc in *c*, and effect was well *Neg.*, but not better than water alone.

6732. Another Cap with no melted caoutchouc in it; and the effect was quite as Neg. and as good.

6733. Put some *dilute Sulc. acid* into the steam globe (6890, 6765), and have two caps *c*, one quite clean (6732) and the other that with the caoutchouc in (6731). The clean cap and box tube—electy. was feeble but *Pos.* for water only (with little S. A.). Put on the cap and caoutchouc and it was much more Pos.

6734. So *dilute Sulc. acid* alone in the Globe is Pos. (6891), and the effect of melted Caoutchouc is also *Pos.*, but not strong.

6735. If a little acid or salt stops *Neg.* action of water and more gives it a Pos. force—more still may be more *Pos.* still. So put in so much dil. S. A. as to quintuple former quantity. Now acid to taste freely. Then with box wood tube and clean box *c* the effect was *Pos.*, but not so much as before.

6736. Same, with melted caoutchouc in box *c*—more *Pos.*

6737. Drew out acid in steam Globe and put in 4 oz. distilled water, not washing, so that a weak acid would be in globe. With the old box wood tube on, was very slightly *Pos.*, but on running a wet feather through the box wood tube and then trying it, the effect was *nul.* Hence dilute S. A. seems to have a Pos. power and very *dil. S. A.* only nullifies the Neg. force of water—very little does that.

6738. Then drew out this water, washed globe, left distilled water in and now with box wood tube was just as powerful and as *Neg.* as on former occasion of pure water.

6739. Put little *Sol. caustic potassa* into steam globe—with same boxwood tube, power nearly gone, but a little *Neg.* left—more potash (a little)—Power quite gone: *Nul.* Much more potash, till strong Alkaline to turmeric. Still Nul—no positive power shewn. More potash still—Nul.

6740. With this alkali in steam Globe and consequent Nul current examd. bodies in box *c*. First, Caoutchouc cap cleared out, and it proved *Nul.* Then *Oil of turpentine* into it—*Pos.*—cleared it out. Nul. *Olive oil* in: *Pos.* but weak—has lost part of its power here with the Alkali in the issuing water, and was to be expected. *Rosin in Alcohol* in *c*. Positive, but with little power—naturally: for these things do not infilm globules of alkaline solution, but dissolve in them.

6741. So solution of Potash in Steam globe does not itself produce *Pos.* or *Neg.* state, and hinders such things as oil or rosin, but not such as oil of turpentine and perhaps not wax.

6742. Washed out the Steam Globe and box wood tube with feather, etc. etc., and when all in place, with distilled water in Globe, strong *Neg.* as at first.

6743. Proceeded to experiment with distilled water in steam Globe, the clean old ivory tube (6501) as jet, with a clean box *c* in its place, and *insulated cones* before the jet—ascertaining the state of the cones by an electrometer (6631).

6744. Ivory jet by itself makes the boiler fixed to it a little Neg.

6745. *Brass cone* before it is first nul till washed clean and then *Neg.*

6746. *Brass cone* with rosin in alcohol put on it last night—was first nul—then *Neg.*—resin left on it after that at the apex.

6747. *Box wood cone* . . . . strong *Neg.*

6748. *Japanned leather* cone . . . . *Neg.*

6749. *Kerseymere* alone but wet . . . . *Neg.*

6750. *Kerseymere cone* wetted two or three times last night in *resin* in Alcohol and dried. First *Pos.*—then *Neg.*—then continually Pos.—not strong however, but was *Pos.* The jet from ivory tube alone did not charge the boiler, or only a very little. Probably rosin carried off from this Porous bulky structure or rather texture.

6751. *Kerseymere* cone covered outside with *melted caoutchouc*—*Neg.* from first.

6752. *Linen and rosin* as Kerseymere above (6750)—*Neg.* well from first.

6753. *Linen and melted caoutchouc* (6751)—*Neg.* remarkably strong.

6754. *Box wood and melted caoutchouc.* Neg. very. In both these cases, caoutchouc adhered to parts of the surface in different places.

6755. *Ivory* cone new . . . . Neg. moderately.

6756. *Sulphur* cone new . . . . Neg. moderately.
Non-conducting power evidently shewn, for until back became covered with drops of water, no charge of electrometer—and after that the leaves fell at intervals, owing to successive discharges from the similar deposition of drops on one of the insulating pieces of shell lac.

6757. Water appears to make all these things *Neg.* by rubbing against them and must be itself *Pos.*

6758. Again put on the Kerseymere with Rosin (6750)–it was Neg.–then irregular, as if it quickly changed from Pos. to Neg. and leaves had not time to take up or hold one state.
6759. The same, except that Rosin in Alcohol was put into box *c*. This gave following states to cones:
6760. *Metal Cone* . . . . Pos.
6761. „ „ with *Rosin* in Alcohol . . feeble and Neg., as if the moving rosin did not pass off but was caught by the cone.
6762. *Box wood cone*: nul–not Pos. as was expected. So put more rosin and alcohol into box *c* and now this cone was Pos.–and also the last, i.e. metal and rosin.
6763. Ivory cone . . . . Pos.
6764. Sulphur cone . . . . Pos.
So all pos. if plenty of rosin in the box *c*.
6765. As these have been with distilled water in the steam globe, I now added some Sulc. acid to it (6890, 6733). Then put on old box wood tube without box *c*–and it was to boiler Nul–no electy. Put on box *c* as it was, with remains of resin, and now boiler *Pos*. as before, but not strong. So independant value of the substance in box *c* clearly shewn.
6766. With same Dil. S.A. in Steam Globe put on larger metal mouth piece, being piece *m* (6589). Alone this did not charge boiler: with brass cone, insulated, opposite to it–cone–Nul
„ „ and resin on it by Alcohol –Nul
box wood cone . . . . –Nul
oiled silk cone . . . . –Nul
so acid in water has same effect on cones as on mouth tube.
6767. Used the old thin ivory tube fixed into steam globe as in (6440), and held before it stretched threads and wires insulated and connected with an electrometer. They were held about half an inch from the aperture; but if two or three inches off, the effect was the same, and there was no charge in issuing stream, so there was no power of collection from stream at 6, 7, 8 or more inches of distance. The steam Globe contained distilled water.

Zinc wire . . Neg.
Iron wire . . Neg.
Platina wire . . Neg.
Copper wire . Neg.

Silk thread . .	Neg. very strong
Linen	Neg.
Wool	Neg.
Horsehair . .	Neg.

All Neg. to pure water. Very excellent and quick effects this way; very good mode of trying things—as good as cones, and better for many things.

6768. Remark here that in using caps *c*, very great difference occurred between 2, both clean—one being very effectual in excitation and the other not at all. The projection of the first tube within the cap probably very important. Perhaps something of this kind good*.

6769. *Air box*—smaller of the two—compressed air in to two atmospheres perhaps—put on box *c* and cone, screwing the cork *n* (6589) of the box on to the male screw of the cone apparatus *e* (6544) and the cap screw of the box *c* on to the stop cock of the Air chamber—left the box *c* clean and dry and the wooden cone (*box*) of apparatus wet. Insulated the box and connected it with an electrometer, gold leaves (the small one)—let out the air—no trace, or the least, of Electy.

6770. Repeated effect, but with Oil of turpentine in the box *c*. Now Electrometer well opened out and the cone and box *Pos.*, as when steam used without water (   ).

6771. Repeated effect, with same state of box *c*. Again *Pos.*

6772. Cleaned out oil of turpentine only by wiping and put water in canvas into box *c*—and now extremely *Pos.* This the joint effect of water and oil of turpentine.

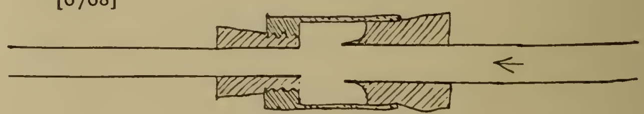
6773. Repeated, using large gold leaf electrometer—leaves struck and stuck to the glass. Abundant effect.

6774. Tried to clear all out of box *c* and use water alone, but found effect still highly *Pos.*, and on examination found that there was so much olive oil sent into stop cock from condensing syringe that it had been mixed jet of oil and water. This all right therefore and as with steam alone.

6775. Must have an Air box with 2 apertures, one to condense by and one to keep quite clean.

6776. Here there can be no evaporation, but rather condensation of vapour—so very clear effect.

* [6768]



6777. Experimented on effect of Oil, etc. in causing breaking up of water into globules and enveloping each with a film (6876).

6778. Balance put up, one pan touching a surface of clean water and the other weighted till nearly sufficient to pull up the pan from the water. Whilst the adhesion still on, dipped a slip of wood in Oil of Turpentine and touched a distant part of the water surface with it. Instantly the film spread and the balance pan went up. So shews how film forms and assists division or separation of water and envelopes each separate portion.

6779. Did the same expt., using clean water, etc. etc. and the smallest drop of Olive oil. Same fine effect.

6780. The same expt. with clean water, etc. and the melted caoutchouc. This very thick and the film spread slowly, but in less than half a minute caused the separation. When hot, as in steam, do this far more quickly.

6781. Powdered rosin sprinkled on water did not produce any effect. No film from it while cold and solid, but when hot probably do so.

6782. Try effects with hot water—perhaps far more instantaneous.

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6783. Experimented with Air box as before (6769). Having a second issue to the box and clean stop cock, etc., the cone aperture with box *c*, etc. was attached to this aperture as it before had been to the syringe stop cock. Hoped now to avoid the effect of the Syringe oil. Threw in generally about 30 or 35 syringe strokes of air into the box.

6784. With distilled water alone on clean canvass in box *c*—a little Pos., not much—suspect some oil or wax here from cap or cock or screw of the aperture.

6785. Again same effect twice—not much *Pos.* I must clear off all impurities by steam from cock, chamber *c*, etc. and then apply them to the air chamber.

6786. Had the wet box wood cone on in these expts. and now put a little Oil of turpentine as well as water into box *c*. Then Strongly *Pos.*—as with steam. Good.

6787. Another box *c* and fresh canvas with water and a touch of Olive oil in *c*. Powerful excitation and *Pos.* as with steam.

6788. Another box *c* with water and Rosin in Alcohol in *c*. At first *Pos.*, but only a little. Repeated and now well Pos.—nearly as with steam. Had forgotten the cone the first time, but the second time had it on and was good.

6789. Another box *c* with water and melted caoutchouc. *Pos.*, and pretty well, but not so well as oil or oil of turpentine or resin in Alcohol.

6790. Another box *c* with clean canvass and flowers of sulphur dry—wet box wood cone used, but wiped first; so is damp. Very little Neg.—scarcely sensible. Some of the sulphur driven into the apex of the wood cone so as to make it pale yellow, and could not wipe it off with wet cloth. Had to scrape it with a knife. This shews that the sulphur had been well dispersed and it shews also the *degree of friction* which impinging particles, as those of water and oil, have in these and such experiments.

6791. Same box *c*. Dry sulphur flowers against a dry *brass* cone: first time *Nul.* Again, now well charged and *Neg.* Again, well charged and *Neg.* So sulphur against metal is *Pos.*—metal being *Neg.*

6792. Dry Sulphur against dry wood cone. Not so much as with metal, but is Neg. Perhaps reason why not so much is perhaps that wood not conduct its state so well to the rest of the apparatus.

6793. Dry flowers of sulphur against sulphur cone—was *Neg.* and as well as wood—remember that cone has been used and exposed to air, and also that the issuing stream of sulphur dust runs between cone of one substance and funnel of metal; but think the latter does not act much except in the tubular passage.

6794. Dry flowers of sulphur against a freely wet wood cone. *Neg.* and fairly.

6795. Then the box *a* emptied and left dry and clear, air only being sent against the wet wood cone; and now was Nul. So *effect of Sulphur* and *non effect of air* evident.

6796. Thinking that these different expts. had perhaps cleared out the air ways of exit cock of all oil, etc., used the wet cone and *distilled water* in *c*, and the small Electrometer. Obtained fair excitation, and that *Neg.*, as with steam. Good.

6797. Used a new box *c*—new canvass and distilled water and the wet wood cone and large electrometer. Again very well *Neg.*, as with steam and water.

6798. To try common water, let a little run through box *c* and the cone, all the rest being unchanged; and now very little excitement, and that Neg. Effect of using common water very evident.

6799. Opened box *c*—with clean cloth pressed out the water of the canvass—then moistened it with very weak solution of sul. soda, and now very little electricity, and that Neg. Effect of salt here shown.

6800. A little more of same solution in *c*. Now power quite gone.

6801. Then simply took out canvass, washed the same box *c* and cork, put in canvass with *distilled water*, and now was *strongly Neg.* Beautiful.

6802. Then took out canvass and dried the same box and cone and used Air as dry as it might be, i.e. without water purposely present; and now a little electricity—it also is Pos., not Neg.; and on smelling to mist at mouth of cone perceived a distinct oily smell.

6803. Begin to think that a little oil mist is thrown forward with air in each case and so makes it a little Pos. (6784, 6785), when it would otherwise be nul—and in other cases adds its effect to what is in the box. Wonder almost that I obtained water Neg.

6804. As the wood cone here was damp, it might be the Pos. side of the friction, and therefore tried the same charge of dry air (6802) against a metal (brass) cone quite dry. Was very strong and *Neg.*—being the reverse of that against wood.

6805. In this case the condensed air expanding would form dew particles, i.e. particles of water. The metal also is a good conductor of its state to the box, etc. Would not these two things account for the Neg. state as a state of friction of water particles against metal? *Try Dry Air.*

6806. Again had the current of air, as just now, against the former damp box cone (6802), and again was feeble *Pos.* Here the globules of dew from cold air probably caught and kept by the wood, instead of rubbing off as on metal. There was also a distinct smell of oil or fat at the cone mouth, which would account for the little *Pos.*

6807. Very dilute Sulc. acid in box *c* against box cone wet with distilled water—not the slightest trace. *Nul.* Smell of oil or grease at cone.

6808. Repeated. *Nul.*

6809. Repeated, using a zinc cone, to observe if any chemical effect. Quite *Nul*—not a trace.

6810. Cleaned all out and used same box *c* with distilled water and a zinc cone, and now have strong *Neg. Good.*

6811. Box *c* wiped out and used air alone and wood cone—was moderate Neg. Would have been probably more, but opposed by oil vapour, etc. from the Air box. The neg. probably from the dew of the expanding air.

6812. Added a touch only of oil of turpentine to box *c*, and now it was *Pos.*

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6813. Yesterday the condensed Air box was well washed out with hot caustic potash—afterwards distilled water and dried to remove all oil and make all clear. Also a kerseymere bag was made to the inner end of the entrance Stop cock, to catch all oily matter sent in by the Air syringe.

6814. To-day adjusted all as on 27th (6783) and had rather a longish retort cap for box *c*.

6815. Box *c* empty and dry and damp wood cone in place. The effect of issuing air was to make cone and box a little *Neg.*, as if water issuing out. Now as air in expanding cools and forms dew, must remember this dew may act as water globules and should be Neg. So here is as with steam, and we seem free of oil.

6817[1]. Little water in box *c*—effect of issuing air *nul.*
Do. — ,, ,, ,, *nul* again.

6818. Used the smaller gold leaf electrometer and now with little water in *c*—effect was slightly *Pos.* The air issuing at the cone smells oily and very distinctly—can that do any thing?

6819. Jet of air from the stop cock alone smells oily.

6820. Sent the jet into a jar: has an oily smell, as if in passing through the syringe, and especially the silk valve, it took up particles of oil which remained pertinaciously in the air.

6821. Measured how much air left the box when 30 strokes of

[1] 6816 is omitted in the MS.

the syringe had been used to compress air into it. The quantity was 150 cubic inches and that was the average quantity in *each blast*.

6822. Again distilled water in *c*—little *Pos.* and smell oily.

6823. The Air box is very dry within, and on taking out the kerseymere bag of the entrance stop cock, it smelled oily. So wetted the bag with distilled water and put it into place again. This bag is about 4 inches long and half an inch diameter, like a pipe closed at one end, through the side of which all the air must filter that is sent in.

6824. Still little *Pos.* Again the same Pos. a little. Must be some oil somewhere, either in air or the exit passage.

6825. Sol. of Ammon. in box *c*—quite Null.
Do. —Do.

6826. Washed *c* well and left distilled water in (same box) with new canvas, and now little Pos. and an oily smell.

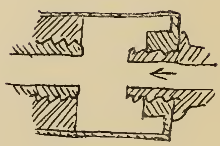
6827. Now all this time I have had a damp wood cone. Perhaps that *as the water side* is left Pos. by the friction—therefore try a metal cone.

6828. As before, but with brass cone—a little *Pos.*

6829. Put a little distilled water in *c*—more excitement, but *Pos.*

6830. A little oil of turpentine into this wet box *c*, and now all well Pos., as was to be expected.

6831. The cap *c* which has thus far been used is one, the female screw of which is long and does not well fit male screw of exit stop cock, so that the thread goes in only a little way; and I suspect that the rest of the thread, which is directly part of the air passage, contains a little oil still at the bottom of the cut, in the angle—which would explain all. So took another cap *c*, shorter and wider, the screw of which was not even so long as the male screw of the exit cock from the air box screwing into it.



6832. First dry. Was well Neg.; is therefore a clean cap at present and the Neg. probably due to the vesicles formed by cold of expansion.

6833. Distilled water in *c*. Fairly Neg., but not so good as Air alone—perhaps now too much water. When air alone, must remember that the condensed air is in contact with water within, and saturated, and so gives much dew on cooling.

6834. No water added—again air. Less than either before, but *Neg.*
Again a little water added. *Nul.*

6835. Cleaned the cap *c* out and left it dry, and now with air only opened out well *Neg.*

6836. Same cap—but with new canvas and distilled water. Less again, but still it is Neg.

6837. Changed the brass cone (6828) for a damp wood cone; now *Nul.*

6838. So addition of water does not better the air, i.e. of water in the box *c*.

6839. A touch of olive oil on the canvas in *c* and damp wood cone; effect weakly *Pos.*—due to the oil.

6840. Cleared out the same box *c*—left it empty—used brass cone and air—was fairly *Neg.* Repeated the experiment, but with *dry* box cone—was fairly *Neg.*

6841. I think the former Pos. effects may now be referred to some oil in the chamber *c* then in use (6831). The state is now Neg., as expected, but not so distinct with and without distilled water in *c* as I expected. Probably the aqueous condensation by expansion is the cause of this. Must try quite dry air against dry wood and metal.

6842. Experimented with some dry powders, just laying a little each time in box *c*, and not using canvas at all.

6843. *Sulphur flowers*—dry wood cone—almost nul, little Neg.

6844. Do. —brass cone—opened out well Neg. (6918).

6845. *Powdered Rosin* and brass cone—well Neg. (6919).

6846. Do. —wood cone. Electrometer leaves opened out well—then instantly collapsed and were left *Nul.* This no effect of contact by accident, but result of all the excitement taking place in the expt.

6847. Again *Powdered Rosin* and *same wood cone*—diverged strong and then suddenly fell; but caught part of charge, and that *Pos.*, as with steam.

6848. On trial found Electrometer and apparatus holds the charge well and steadily—no fault there.

6849. Again Rosin, etc. Jumped up and fell again and stood with small charge *Pos.* This as with steam.

6850. Now *Powdered Rosin* and *brass cone*. Opened leaves more slowly, but was *Neg*., so a difference here. Metal becomes Neg. and wood Pos. by these frictions of resin against them.
6851. Again *Sulphur flowers* and *brass cone*: well *Neg*.
6852. Do. *dry wood cone*: well Neg.; so sulphur not as rosin with wood, but is with metal.
6853. *Silica*: finely powdered rock crystal and wood box cone, dry—knocked the electrometer leaves off against the glass of jar.
6854. Do. and dry wood cone with only 10 strokes of air syringe for charge. Made large electrometer (repaired) well *Pos*.
6855. Do., dry brass cone—20 Syringe—also strong *Pos*.
6856. Left out the Silica and now effect Nul.
6857. *Silica* from Fluo silicic gas in water (very fine)—against the brass cone—15 syringes of air—excellent excitement and *Pos*. (6917).
6858. Do. and dry box wood. 15 Syringes. Strong *Pos*.
6859. When in these experiments a dry wood cone is used, the apex becomes damp and wet from the moisture which it receives from the mist of the expanding air. Shews the presence of water from this source.
6860. Cleared all away, leaving the empty box *c* on, and tried more *air* again upon *brass cone*—was fairly *Neg*. Good.
6861. Same with Air and wood cone. Good and *Neg*., so no oil interfering here.
6862. Put a few drops of water in *c*—quite *Nul*.
6863. Examined the distilled water—it is good.
6864. Put canvas and water into the box *c* and used 40 strokes of the syringe—was now the least degree *Pos*.—scarcely sensible, almost *Nul*. Smell of oil here, as all along, in the issuing air.
6865. Then cleared out the box *c* and left it dry, and now, using air only, it evolved *Neg*.
6866. *Powdered Gum* against brass cone ( ): very little Neg.
6867. Do., dry wood cone—very strong and Pos.—found a little moist gum at the apex of the wood cone.
6868. *Potato starch* and *wood cone*—good and Neg.
6869. *Gum* and *wood cone*—Neg.
6870. *Gum* and *wood cone*—diverged much, instantly collapsed and left quite *Nul* (6903).

6871. *Gum* and *wood cone.* Again diverged much, instantly fell and left quite Nul. There must be a different excitement here at the beginning and the end of the rush. The one must be contrary to the other and a compensation. The little moist gum at apex found here again.

6872. Now cleaned out box *c* and the cork and tried air alone.
The air against a brass cone made it and box—Neg.

Do.	dry wood cone	,,	,,	—Neg. and stronger.
Do.	ivory	,,	,,	—Neg.
Do.	sulphur	,,	,,	—Neg.

6873. Now took off the cone apparatus from the stop cock of the air chamber and used that alone with rush of air, and it was able to make the box *Neg.*—but not so strongly as when its power was combined with that of the Metal, the wood, or even the Ivory cone.

6874. So above (6815), when against wet wood cone it is *nul*, it may be that the cone merely discharges the air electrified *in* the cock.

6875.

Sulphur rubbed with	tin foil becomes	*Neg.* Tin doubtless is Pos.
Do.	platina foil	*Pos.* Platina Neg. then
Do.	copper foil	Neg. Copper Pos.
Do.	wood	Neg. wood Pos.
Do.	flannel	Neg. flannel Pos.
Brass	linen canvas	Neg. canvas Pos.

6876. Repeated the experiments with balance (6777), using very hot water, nearly boiling. The results with *oil* and oil of turpentine were the same as before, and now also rosin and rosin in Alcohol shewed their power of effecting a similar separation when in the melted state.

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6877. Worked with steam boiler—all insulated and in excellent order for experiments. Used in the whole   gallons of water.

6878. Old wet box wood jet—very strong excitement and Neg.

6879. Used the sulphur wood tube (6534): was well *Neg.*

6880. The thin old ivory tube (6501): a little Neg.

6881. As to the state of the part rubbed against. Held a piece of sulphur in the stream near the jet—the dry part of the sulphur insulating the place or part rubbed. Then brought it towards a diverged gold leaf electrometer and found the sulphur was left in a Neg. state (as expected) by the rubbing stream.

6882. Shell lac tried same way—also *Neg.*

6883. A piece of shell lacked wood, held in insulating forceps, also made Neg. by issuing stream.

6884. A Zinc tube used in place of the ivory tube—it rendered the boiler *Neg.*, but not very strongly.

6885. Now put the thin ivory tube (6880) into its place as a steam jet, as in 6440, and tried the effect of the jet against a great variety of substances, by holding them in an insulating handle just in the stream, about $\frac{1}{4}$ of an inch from the mouth of jet; and connecting the electrometer, *not* with the boiler, but with these pieces by a wire. The water becomes *Pos.* with *all* of them and renders them Neg. The following is a list of the substances—distilled water being in the steam globe.

Platina wire	Horsehair	Asbestus
Copper	Bears hair—feebly	Cyanate—strong
Iron	Flint glass—powerful	Hæmatite—powerful
Zinc	Green glass—Do.	Rock crystal—strong
Sulrt. Copper	Quill—feeble	Orpiment—strong
Linen thread—very	Silk and shell lac	Sul. baryta—strong
Cotton—strong	,, and sulphur	Sul. lime
Silk—very	Piece of sulphur	Carb. lime
Worsted	Plumbago	Fluorspar;
Wood	Charcoal	

and a platina wire made red hot by a voltaic battery. In the stream this was quickly cooled down and was probably only boiling hot there. All were *Neg.*; but apparently with some differences in degree or readiness. Much of this would however depend upon conducting power of the substance, and also upon its size, favourable position to the jet, etc.—for a fine wire of platina and a thick splinter of hæmatite are not alike in these respects.

6886. Then introduced some *solution of Sul. Soda* into the steam globe to ascertain whether this would destroy exciting power with some or all of these bodies (6885). Did so with every one of the above list that were tried, i.e. all but the red hot platina wire and the asbestus, which were not tried. *No signs of excitement with any one of them.*

6887. Washed out the Sul. Soda and left distilled water in the steam globe, and now upon trial with the slip of wood at end of insulating handle, was strongly *Neg.* and all right, as at beginning of the experiments.

6888. Put on a large box *c* as in 6589, and in the box thick canvass, and rosin in powder and dissolved in alcohol, to see if it would be *Pos.* to all these things, i.e. make them all *Pos.* Had occasion to renew rosin once or twice during the experiments. Tried all the substances in the list (6885) except the hot platina wire, *and every one* was rendered *Pos.*; no exception.

6889. Then took off the box *c* and ivory tube and tried *wood* at the end of the steam jet, of ivory, as 6440–instantly rendered it Neg. and strongly; so shewing that the steam globe and its contents had been in a right state the whole time and that box *c* did its own especial duty.

6890. Put a little Sulc. acid *into the steam Globe* (6765, 6733), so as to render it just sensibly acid to the taste, and now with ivory tube, as in 6440, tried all the former substances, with the exception of Sulphuret of copper, copper, silk thread with lac, silk thread with sulphur. Every one was *Nul*, and have no doubt these four would be also. Have indeed been tried before (I think).

6891. Could obtain no signs of Pos. any where here. Think that the former case (6734) must have involved a little trace of oil or resin or something of that kind.

6892. Then tried whether substances, as resin, put into a box *c* in its place, would appear Pos. with this acid water in the steam Globe. Put first rosin and after that, in addition, a little oil into the box *c*. Glass and linen were both tried and made a little Pos. by this arrangement, but power of the rosin, etc. excessively reduced by the acid in the globe.

6893. Put a wet wood cone opposite to the issue and it was made fairly *Pos.* Still power greatly diminished.

6894. On taking off the box *c* and putting the same cone opposite the steam jet in 6589, direct from the globe, now it was quite *nul*, shewing both effect of the acid and of the resin.

6895. Now wanted to use little ivory tube without box *c* and arranged it as 6440, having cleaned out the steam globe and left in it only distilled water. But the jet *would not excite* any of the list of substances; nor would it excite and charge the boiler.

6896. Washed it with Alcohol to remove any resin, boiling it in the spirit—it was apparently clean, but still would neither excite the boiler or any thing held before the stream of steam and water.

6897. The thick ivory tube put in its place (6711) excited the boiler a little, and matters held before it much, and *Neg.* as in the usual state; and the wet wood tube made both boiler and also any thing held opposite jet *very Neg.* So the steam Globe, water, etc. is in a proper state.

6898. The thin ivory tube therefore at present is not merely a non exciting tube, as in its ordinary state it is almost, but now it has power to take off the exciting property of the issuing jet beating against other things.

6899. There is a feel of rosin about the end of the tube and it is very probable that a little rosin still in its pores acts enough to neutralize the force of stream—for in situation, the tube is to the things held before it as in other experiment box *c* is to the attached tube.

6900. Have put the ivory tube to soak in Alcohol and left it there.

6901. The water used to-day was gallons.

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6902. Worked with Air box (6783, 6814) all day. The box has been well cleaned out and is now dry. Used no entrance cock bag (6823). Attached the metal cone, brass, and chamber *c* to the box, all being clean. Pumped in 30 strokes of air and on its issue box and cone became *Neg.* at once. There was the appearance of dew or mist to the friction of which I attribute the electricity. There was also the old oily or fatty smell (6802), but it does not really seem to interfere with or rather to stop the action of the water, so the Neg. effect comes on.

6903. Took off box *c* and used exit stop cock as the issue, mounting the insulated brass cone before it and connecting it with an electrometer. Now very powerful excitation of the cone and it Neg. far more than before. The brass was dim for a moment or two by the moisture deposited upon it. 30 strokes of Piston.

Again as last (20 strokes)—opened a little only and *Neg.*

Again (30 strokes)—opened and shut again (6871).

Again (30 strokes)—but the cock was opened badly, so issue slow by comparison and electricity *nul*—low pressure issue is bad.

Again (30 strokes)—cone nicely adjusted and cock well opened. Cone was well *Neg.* The cone so cold as to cause a falling current of dew from it into the air.

6904. *Ice.* A piece of ice with a cone end insulated and used as the cone (35 strokes). Ice was left a little *Pos.*

Again *ice* (40 strokes)—ice left *Neg.* and frozen white and dry at the place of friction.

Again (36 strokes)—*Pos.* well }
Again (Do.) —*Pos.* well } ice Pos.
Again (Do.) —*Pos.* }

6905. Then Brass cone (30 strokes)—weak Neg.
Do. (Do.) —weak Neg.

Water condensed on the metal from the cold issuing air.

6906. Dry wood cone—common air—bad issue and but little *Neg.*
Again —good issue—fair and *Neg.*

6907. Ice again (35 strokes)—Well *Pos.*

So it is clearly above all other bodies in its Positive relation.

6908. Proceeded to work with *dry air.* Put much Potassa fusa in cylinders into the air box through the entrance cock hole; about 4 or 5 ounces. Threw in 50 strokes of air. Placed the smooth brass cone used before (6905) on the insulated stand, and when the air had remained 15 minutes in the box with the potash, let it well off against the cone. There was plenty of air and good issue, but no signs of dew or mist and *not a trace of Electy.*

Again 50 strokes of Air, left in for 16 minutes and let off against a clean brass cone scratched with sand paper✧. Used the smaller and more delicate electrometer, but *no trace of electy.*

Again 50 strokes of air, left in 10 minutes—let out against the rough metal cone (above✧). Excellent rush of air and the cone the least degree *Neg.*, as if 10 minutes not enough to dry it.

6909. Remember, when left in 15′ there is more loss of air by leakage than when left in 10′. So that the last might be less dry and issued out with better rush; both circumstances favourable to developement of Electricity.

6910. Again 51 strokes of air, left in 18 minutes—good rush of air but no electricity.

6911. Now 20 strokes only put in and let out immediately against the same cone and under the same circumstances, and now Electricity to a fair degree and *Negative*. This very striking.

6912. Used a *sulphur cone* insulated—50 strokes of air left in for 15′—let out against sulphur cone—could find no trace of electricity in the cone. But 20 strokes put in, and let out at once against the same cone in the same manner, now made the sulphur *Neg.* and even the electrometer attached to it *Neg.*

6913. *Dry wood cone.* 54 strokes of air left in 18′—let out will produce nothing.

6914. *Damp wood cone.* 50 strokes left in for 15′—no electricity. 30 strokes in and let out directly against same cone—also nothing. 30 strokes again—no effect. Again 30 strokes—nothing either at the wood cone or the air box itself.

6915. Metal, brass, cone insulated from box (as all the latter expts.); 30 strokes let out instantly—box very little Neg.—cone not sensibly.

6916. Again and again these *nul effects* with air let out immediately occurred—can this be because the air is so quickly dried by potash and because some little moisture in passages is now removed—so that all they are dry? Or has some neutralizing condition of the cock come on like that before described of the ivory tube (6895)?

6917. Put on box *c* and the cone with it, making it one with the air box—used the *rough brass cone*. A little *dry powdered silica* in box *c*. 40 strokes left in 14′, and then good issue of air. Box, cone, etc. very strongly electrified *Pos.* Leaves of Electr. knocked about (6853).

6918. 50 strokes left in 20′—with *flowers of sulphur* in box *c* and rough brass cone. Strong and *Neg.* (6843, 6851).

6919. 50 strokes in for 13′, *powdered rosin* in box *c*, and the same rough brass cone—box and cone left a little *Pos.* (6845, 6847, 6850).

6920. 30 strokes let off at once, box, cone, etc. being untouched—there was the merest trace of *Pos.* Again 34 strokes at once. Now there was nothing; so that first effect must have been that of rosin, the second that of little residue of rosin, and the last shewed the box and passages clear of rosin and the air itself ineffective as before (6916).

6921. Again *Silica* powder, using that from fluo silicic gas this time (6857). 30 strokes let off at once made brass cone and air box *much Pos.*

50 strokes in for 10′. Silica, powdered rock crystal, in box *c* and smooth brass cone on. Excitement *very strong and Pos.*

30 strokes let out immediately against cone with only residue of silica from last expt. in box *c*—a *little Pos.*

6922. Wiped out box *c*—then 30 strokes let out immediately against brass cone. *Quite Nul.* So quite clear that, with a nul jet of air, either common or very dry Silica makes brass constantly *Pos.* and therefore itself must be neg.

6923. Rock crystal silica and dry wood cone (6913). 30 strokes let out immediately—cone and box *very strongly Pos.*

6924. Took off the cone and the cork fitting into box *c* and left box *c* with the issue cock as the jet. 30 strokes of air *produced nothing.*

6925. Put a little silica into box *c* and the cork in its place ( ), but not the cone or its parts. 30 strokes of air made the box well *Pos.*—so that the silica against the metal and cork of the box, and without the cone, tells, and makes Box (air) *Pos.* Must therefore use the insulated cone ( ).

6926*. Fixed brass pipe *a* into box *c* by the cork *b*; *d* being the air box issue stop cock and *e* the silica or other powder used. The cone was insulated and connected with an electrometer.

6927. *Silica* in the box *c*—50 strokes for 10′ made cone very strongly electric and *Pos.* still—20 strokes instantly let out and with silica in box *c* made cone *Pos.*—5 *strokes only* with fresh silica made the cone *Pos.*

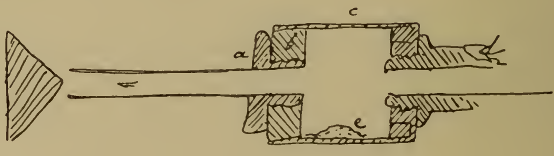
6928. Metal therefore is *strongly Pos.* by friction with Silica and air.

6929. Now box *c* cleaned out and brass cone put on to it, and 50 strokes left in 6′ and then well out—*nothing—absolutely nothing.*

6930. *Canvass and distilled water into box c.* 50 strokes left in 11′—and let out against smooth brass cone—quite *Nul.*

Again—but first washed box *c* and all the parts of the cone

* [6926]



apparatus in hot and cold distilled water—restored it to its place, leaving distilled water with canvas in box *c*—50 strokes left in box 9′, let out against smooth brass cone. *Nul*, quite *Nul*.

Again, *wet wood cone*—50 strokes of air left in 3′—then let out well—but *Nul*.

Took away cone and box *c*, leaving only the air box cock. 30 strokes let out immediately—*Nul*.

6931. Box current of recent air has lost its power of making *Neg*., or of doing so with water in *c*—is this because some little matter in cock, as alkali or grease from the plug, forced onwards by the high pressure, has neutralized it? Must try a new cock.

6932. Tried to procure some effects by blowing from the mouth against an insulated cone, using a *box c* with water and oil of turpentine, and also a dry *box c* with silica; but could not succeed. Power of mouth and lungs not enough for these Electrometers.

6933. Required certain experiments as to states assumed by the bodies used above when excited by ordinary mutual friction. The substances used were a fur cuff of dyed Marten's skin; Flint glass; Shell lac; Sulphur; Feather: a wing feather plumage part; Quill; Brass; Rock crystal; Tin; Platina; Hand; Catskin.

6934. *Cuff of Marten's skin was Pos*., making Flint glass; Lac; Sulphur; feather; quill . . . . —Neg.

Feather-wing was Pos., making Flint glass; Lac—Negative.
Quill was Pos., making Glass, Lac . . . . —Neg.
Flannel Pos., making Rock crystal . . . . —Neg.
Glass Pos., making Brass . . . . —Neg.
Rock Crystal Pos., making Hand; tin; platina; brass—Neg.
Catskin Pos., making Flint glass . . . . . . —Neg.
Shell lac Pos., making brass . . . . —Neg.
See 6875 for other results, as
Sulphur is Pos., making Platina . . . . . . —Neg.
Sulphur is Neg., making Tin; copper; wood; flannel; canvas . . . . —Pos.

6935. Have been engaged in trying all the bodies in the column, each rubbed against *every other one*, and ascertaining their state. Any one is *Pos.* to all beneath, and *Neg.* to all above, when excited by mutual friction. The list is more correct than the results above (6934).

P
Catskin
Flannel
Ivory
Quill
Rock crystal
Flint glass
Cotton
Linen, canvas
White silk
Hand
Wood
Lac
Metals: Iron
Copper
Brass
Tin
Silver
Platina
Sulphur
N

6936. But there are some reservations to make. Thus of a *catskin*, some parts are Negative to other parts, and Negative to below Flint glass. The long hair part is most *Pos.*

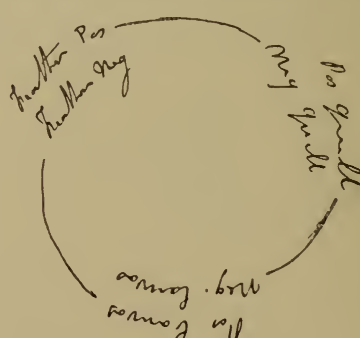
6937. *Ivory* and metals are curious. Though ivory is a very good insulator, yet it is very difficult to produce *mutual excitement* between it and the metals. With my present means the excitement was nearly *nul*, and very different to that with much lower bodies, as linen, cotton, wood, etc.

6938. *Flannel* is often uncertain. Of two pieces rubbed together, the one would always be Pos. and the other Neg.—and that which was thus *Neg.* would also be *Neg.* to Ivory, quill, rock-crystal, glass, etc. Much may depend upon how the flannel has been washed and cleansed from soap. Again, when two pieces of flannel are rubbed across each other, often both will become Neg.; and probably the Air has then become *Pos.* (6692).

6939. *Feather*, according as it was applied by friction to other bodies, would sometimes be above Rock crystal and yet again below white silk. A feather struck with its feather edge against dry linen (canvas) became strongly *Neg.*, and yet the same feather, drawn with a little pressure between the folds of the same canvas, was strongly *Pos.*; and this again and again. It is possible that a part of this effect may depend upon the different quality of the quill and the feather part; but as far as I examined the matter with the feather part alone, the same strange alteration was produced.

6940*. Feather and quill rubbed together: the feather was *Pos.*

* [6940]



and the quill Neg. Quill and canvas rubbed: the quill was *P.* and canvas *N.* Canvas and feather rubbed together: canvas was P. and feather Neg. Under these circumstances, these three things form a circle. But careful examination probably destroy it.

6 JANY. 1843.

6941. Resumed expts. with Air box. It still contains the potassa fusa which has been closed up in it, and the exit cock has been freshly cleaned with caustic potassa. All the rest is as before (6931).

6942. Used the last box *c* and canvas in it, which was moist (6930). 30 strokes and the air out directly against brass cone attached to the box; but was *Nul.*

6943. Arranged the insulated cone and the box *c* and metal pipe (6926). 30 strokes of air—out at once—*Nul.*

6944. Changed the issue cock for the former one (in good state), and used the box *c* and pipe (6926) and insulated cone of brass; and it became a little *Pos.*

6945. Then attached the cone to the box *c* and air box; and now *Nul.*

6946. The air itself, whether used instantly or left to dry, is always Nul. in its effects? So appears not to be friction of *air alone*, and hence a reason why not friction of *steam alone* in the boiler case.

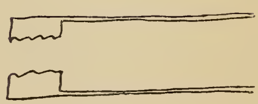
6947. Put some water distill. in box *c*—*Nul.* Again, with 35 strokes—*Nul.*

6948. A little oil of turpentine added to the damp canvas in box *c*, and now box left powerfully *Pos.* Good.

6949. Cleaned out box *c* by wiping—then a little *Pos.*; smell of turpentine. So this Nul source of air easily acts with Oil of turpentine mist and globules.

6950. Difficult to make distill. water act by air alone—time too short and pressure too small.

6951. Used this Nul source of air with powders. Dried the box *c* and put flowers of sulphur in. No excitement; on examination, found all the sulphur caught in the eddy of the box. So took it off and used a longer, narrower one, where the stream more direct—and now the sulphur was thrown out and the cone and box made *Pos.* Good.



6952. *Rosin* in box *c*. Blew out visibly, but cone nearly *nul*; again–nearly *Nul*; not clear whether *Pos.* or *Neg.*

6953. *Silica* in box *c*. 30 strokes–left cone well *Pos.* as before. Again the same distinct result. The cone is brass.

6954. Blew *silica* out of a jet (6926) against a tin plate set up 6 inches off. It made the air box *Pos.* and also the tin plate a little so; for it had rubbed against the Plate and not been discharged by it. So repeated the expt. and covered the plate with a wet sheet of paper, to catch, stop and discharge the silica, and now it became *Neg.* So Silica rubbing against metal becomes *Neg.* and makes it *Pos.* This not according to the table and yet a very constant effect.

6955. Used a new box *c* and the old ivory tube (6900), which I have tried to clean by spirit, etc. Insulated cone and 30 strokes of air out at once–cone a very little *Pos.*

6956. Same box *c* and the attached cone–effect *Nul.*

6957. Same box with old ivory tube and independant cone (6955), as just now, and now *Nul.* Quite as dry air.

6958. *Ice* corner opposite ivory tube–30 strokes of air–Nul. Now put canvass and oil of turpentine in box *c*, and opposed the ice–the *ice* left nul. Now put a little water into box *c* with oil of turpentine and now Ice well *Pos.*

6959. Henry's boiler–attached chamber *c* with oil turpentine and water in the canvass and brass cone to one of the stop cocks and insulated the whole. Got up steam; very poor pressure and issue, but still the Electrometer easily opened out *Pos.* A large Marcet's globe does very well.

16 FEBY. 1843.

6960. Very cold to-day and yesterday, and I have been trying to excite substances against ice. The ice was very dry and the other things well cooled, and all insulation was good, but I could obtain no indications.

6961. *Ice and flannel.* Flannel not stick to the ice at all, so all solid and dry and cold. Yet no indications either in ice or flannel.

6962. *Ice and Shell lac*–no indications.

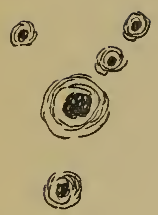
6963. *Ice and metal*–no indications.

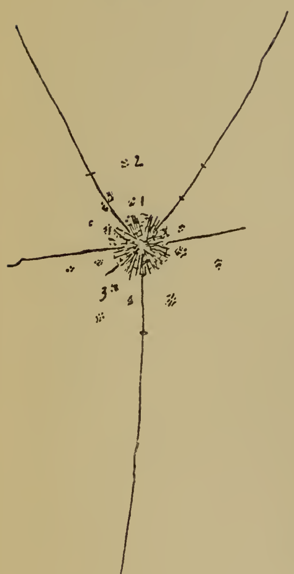
6964. Yet *shell lac*, *flannel* and *metal* easily excited each other well at these same low temperatures.

6965. Very curious that ice should be more difficult to excite than metal.

6966. Ice is a pretty good conductor of static electy. of intensity and at once discharges the electrometer; but still, it should have excited against shell lac, flannel, etc., if not against metal. Was the ice of common water—not distilled water.

12 FEBY. 1844.





6967. Mr Bachhoffner has sent me a large Leyden jar from the Polytechnic Institution. It was one of a battery; was charged in the battery, but when discharge was made and the wire intended to be burnt was burnt, at the same time this jar was broken through, by the Electricity passing through the glass from coating to coating, just midway between the upper and lower coating. The appearances were as follows. Electricity had passed through in five places, as in the sketch on the other side[1]. At each of these places the tin foil was raised and blown into a hole both on the inside and outside, being forced up from the glass in the form of a cone, the holes being about the size represented[2] and the cones as represented by the fainter lines. When the tin foil was stripped off the outside, five cracks were found, proceeding from one center, of the shape, size and disposition represented beneath[3]. The center corresponded to the larger hole in the foil and the places of the four smaller holes are indicated by short cross lines in the direction of the cracks. As to the center, there the main and breaking discharge had taken place, and at it the glass was pulverized nearly; still, the lines of the fractures were in innumerable radii and circles, as occurs when a bullet is thrown at plate glass. The size of the place is about that represented. This I believe to have been the first and great discharge—but then at the other four places, and just under the elevations of foil, appear edges of metal upon the crack, shewing that discharges have passed there also. Now the glass is no further broken here, and think these four discharges must have been posterior to the first and to the existence of the crack—probably supplementary or after discharges which take place through close crack[s] almost as easily as through metal. At the center of the large scar or fracture was a small round hole, about the size of a coarse human hair, and the sides of this were fuzed after the manner of a fulgorite. There are certain other small cracks across the larger ones, about $\frac{1}{20}$ of

[1] i.e. in the margin.

[2] The sketch is reduced to $\frac{3}{4}$ scale.

[3] i.e. in the sketch; reduced to $\frac{3}{4}$ scale.

inch long, and there are two to each secondary discharge, one on each side of the metallic line of edges—they appear to have been formed by these secondary discharges. Besides these there are certain places, as at 1, 2, 3, where a remarkable disintegration of glass has commenced and not gone through, i.e. the glass is broken with radial and curved fractures, as if a hole like the large one had begun to form there, but had not gone on to completion. These do not appear to go through the glass, but must be examd. in sun light. There are slight traces of seven or eight such places around the great central place, and as yet I see no signs of them elsewhere in the jar. Looking on them as flat cones, their base is on the outer surface of the glass and their apex in the body and near the middle of the thickness of the glass.

6968. On stripping off the foil from within, I found the gum or paste quite wet—this moisture must help to blow up the tin foil at the time of an explosion.

6969. There are several more places such as 1, 2 and 3, and I have mapped their places. Those in black ink are on the outer surface of the jar going inwards, and those marked red are on the inner or negative surface of the jar coming towards the outside[1].

6970. The character of the main fracture or center is the same on the inside as the outside.

6971. The metallic edges at the four places of secondary discharge occur at the inside also of the jar, at the same spots, but I cannot trace them *in* the crack, i.e. on the crack surface. They are radiate from the crack on the surface outwards, and have the general character of metallic deflagrations. That at the center was probably quite burnt by the force of discharge, and indeed the minute hole there and part around has a yellow brown colour.

MAR. 9, 1844.

6972. I sent Airy a letter and sketch for a proposed registering atmospherical Electrometer, depending on the attraction of discs. He thought the mechanical power would be small, so I have obtained a few numbers by expt. on a rough practical scale to shew the amount of power.

[1] There is, of course, no distinction between the red and black marks in the reproduction on p. 144.

*a**, a disc formed of a hoop, having paper stretched over its lower edges, and that covered with tin foil; a wire *b*, fastened to the upper edge by a little bracing, makes the disc quite flat; *c*, *c*, *c*, silk threads; *d*, a wire link hanging from one arm of a balance; *k*, a fine springing wire connecting *b* with *d*, and thoroughly *uninsulating* the disc *a*; *e*, a flat board covered with tin foil, standing on the insulating pillar *f*, connected by wire *g* to an electrical machine, by another wire to the ball *h*; *i* is another ball, uninsulated and adjustible in distance with ball *h*. The machine would of course charge *e* until a spark would pass at *h*, *i*, and also attract *a* with a force measurable with the balance.

The disc *a* was 24 inches in diameter.

The board *e* 24 × 36 inches square.

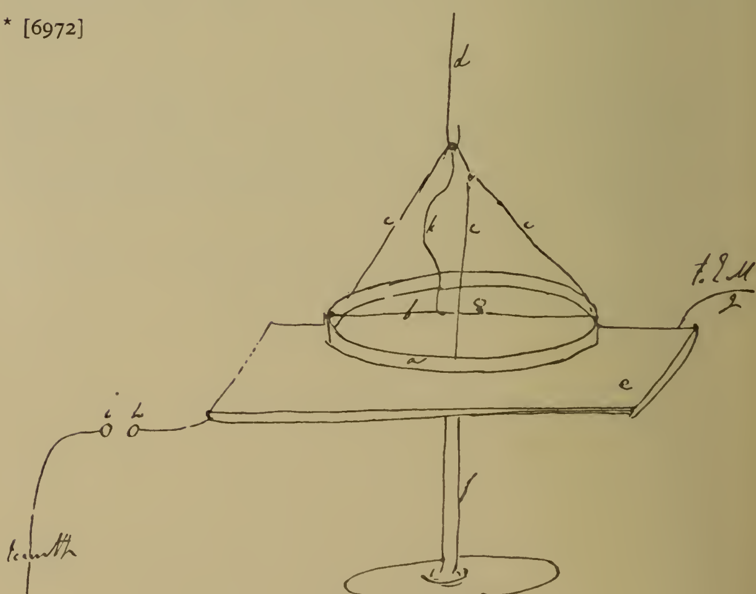
The balls *h*, *i* about $\frac{3}{4}$ of inch in diameter.

S shall be the distance between *h* and *i*.

t the distance between *a* and *e*; and the power expressed in grains.

S in part of inch	*t* in inches	pull in grains
$\frac{1}{8}$	$3\frac{4}{8}$	50
”	$2\frac{7}{8}$	75
”	$2\frac{4}{8}$	100
”	$2\frac{2}{8}$	125
”	$1\frac{7}{8}$	150
”	$1\frac{6}{8}$	200
”	$1\frac{3}{16}$	250
”	$1\frac{3}{8}$	300
”	$1\frac{3}{16}$	400

* [6972]



$\frac{1}{8}$	1	500
,,	$\frac{7}{8}$	600
,,	$\frac{6}{8}$	700
$\frac{1}{16}$	$\frac{13}{16}$	100
$\frac{1}{16}$	$\frac{12}{16}$	150
$\frac{1}{16}$	1	50

With S $\frac{1}{16}$, our large gold leaf electrometer opened about 6 inches.

23 MAY 1844.

6973*. *Condensation of Gases*. Made a cyanogen retort of green bottle glass. It was above 12 inches long in the glass; of an inch in external diameter; of an inch in thickness of glass. The two bends were not very good; one was a little puckered. Brass caps were cemented on by common cap cement and a little tow over the joint; a small stop cock was used and washers of leather just wetted through by soft cement at the fuzing point.

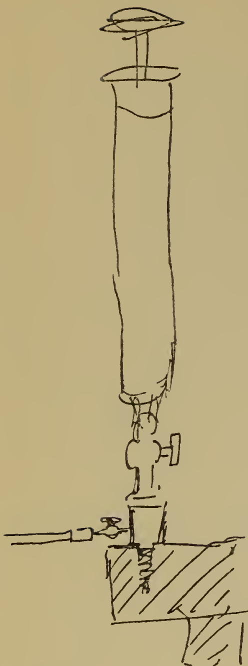
6974. Made cyanogen in the retort until it issued at the stop cock; then closed the cock and continued to heat the cyanide, cooling the bend *a* by a little water; liquid cyanogen collected there and all stood the pressure well. Kept the cyanogen for 24 hours–it returned to the solid residue, but was easily driven over into the bend by a little heat.

6975†. Made a compressed air Guage as in former times; placed it in a green glass tube closed at one end and furnished with a cap and stop cock at the other. The chamber or outer tube was 8 inches long; 0·25 of inch in external diameter; thickness of glass was 0·025 of an inch or $\frac{1}{40}$.

6976. Attached this guage and vessel to the cyanogen retort by its stop cock and leather washer in soft cement; opened the cock and the guage instantly shewed a pressure of four atmospheres just. It stood well and there was no leak; so glass, cock, junction, washer, etc. will easily stand this. Shut the cock and loôsened the guage aperture; immediately the guage mercury went to Zero. Screwed up again and re-opened the stop cock; the pressure was now only 3·6 atmospheres; temp. 60°. This difference was due

* [6973]


† [6975]

to the removal of the common air from the guage case; in the first instance it was left in.

6977. Fixed up the silk valved syringe. This is a condensing syringe, an inch in diameter, having only silk valves below and above; and I require to know how they will stand pressure. Attached the Guage tube by a little stop cock to it, as in figure. Then forced in air. Easily obtained a pressure of four atmospheres, 6, 8, and at last reached 10 atmospheres of pressure. This pressure was held well by the silk valve of the syringe—by soft cement—leather washers—by cap and red cement joint—by the glass tube of this thickness—and when the little stop cock was closed and the guage vessel removed, it was held by the little stop cock alone. Nothing started or gave way.

6978. Had another syringe of same diameter, but with metal valves. Aimed at 8 atmospheres pressure with it, but before that the Valves leaked back and they would not hold the pressure.

6979*. Made a green glass tube for gas, of this shape, etc.

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6980. Teased with leakage; traced it at last to a bad little stop cock. Tried all the other things—all tight.

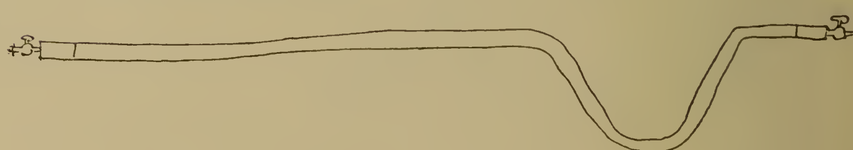
6981. Whilst a tube as above was on and under a pressure of about 6 atmospheres, broke it off close to the stop cock of the syringe. It broke easy and clean and there was no splitting up of the whole nor any projection of the mass. All good.

6982. When cyanogen had returned to its residue in the retort, tried the pressure—was the same as if separate and in liquid state.

6983. Worked with Addams' Carbonic acid liquid—procured solid—long filaments of frost form on it in a deep little basin and these are very electric. The whole piece of Carbonic acid is electric and is not discharged by the hand or by metal. It is a non-conductor. It is probably made electric as it rushes through the nozzle of the apparatus, like water, etc. with steam. Its state is   .

6984. Put C.A. solid and Ether into the little cooling dish as intended to be. Kept for 15′ with solid C.A. present, so at extreme cold. Mercury in it continued frozen for 21′, so at 50° below 0 for that time.

* [6979]



6985. Glass tube, easily cooled and introduced into it, stood well.
6986. Solid C. A. in a spoon in Nitrous gas—condensed part of the atmosphere—when spoon touched the liquid Nitrous acid, it froze it into a solid lump, pale green yellow, which in the air sent off yellow Nitrous acid gas.

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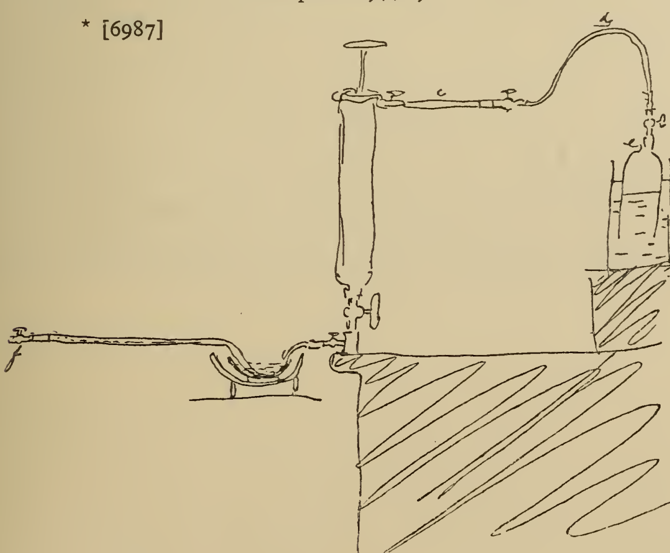
6987*. Worked with the syringe and green glass tube of 23 May, page 1485[1] ( ). *c* is a glass tube, with pieces of chloride of calcium to dry the gas drawn in by a flexible tube *d* from the gas jar *e* over water. At the bend in the tube arranged two little Dresden cups, one in the other, to make the cold bath of C. A. and ether.
6988. Addams' Apparatus was in excellent order and gave plenty of Solid C. A. After using it in lecture I proceeded to make the following expts.
6989. *Olefiant gas* was first put into jar *e*; then some of it slowly pumped through the apparatus until the gas issued freely from the terminal cock *f*; that was then shut, the cold mixture applied to the dipping bend carefully, and when the tube was fairly immersed and cooled, pressure was applied by the syringe. Soon the gas went down into a liquid, and thus another was added to the list, and the efficacy of the plan illustrated. It was a clear colourless transparent fluid. It did not crystallize at the temperature of this bath—Solid C. A. being in excess in the ether. When obtained, the cock was shut between the Syringe and the condensing tube and thus the liquor left to itself. Being then allow[ed] to acquire the temperature of the C. A. bath, the pressure stood

[1] i.e. pars. 6977–9.

* [6987]



at 3·7 atmospheres. Of course C. A. in the tube would have had a pressure of 1 atmosphere, or perhaps rather less. See below.

6990. Next took *Carbonic Oxide.*

Raised the pressure up to 5·2 atmospheres in C. A. bath, but could not condense the gas. Now the Syringe leaked and I could not get a higher pressure. Must have a smaller and better syringe and try again.

6991. *Nitrogen.*

Compressed up to 10 atmospheres but did not go. The syringe leaking and could not hold this pressure for a constancy. Evidently will not go down easily—but persevere. The glass and joints of cement stood well; all seems right except the syringe and perhaps the washers. Use washers of lead.

6992. *Hydrogen.*

Compressed up to 10 atmospheres, but would not go—the difficulty the same as with Nitrogen.

6993. *Carbonic acid.*

Easily condensed—its condensation readily indicated by circumstance that, when gas thrown in by syringe and guage raised, it instantly ran back again to a certain point. Also as the C. A. condensed on the inside, the Solid C. A. in the ether bath rapidly disappeared, the heat (latent) being transferred from the C. A. within to the C. A. without.

6994. When condensed, the C. A. could be obtained either as a liquid or a solid. It went down as a solid, but on shutting the cocks and removing the C. A. bath the tube became warmer, and at a certain temperature, unknown at present, it became a liquid. This could be easily frozen into a solid again, and when this was done, the C. A. did not expand or change in bulk much, but crystallized and froze into a clear, transparent, colourless, solid body, the C. A.

6995. When the cooling bath contained plenty of Solid C. A. and the tube was left in it, the guage within not only went back to Zero, shewing that the C. A. within did not exert more pressure than *one* atmosphere, but it retreated even to a distance equal to 4 or 5 divisions of the guage, the 12 divisions becoming 16; hence there was a partial vacuum in the tube, or rather a pressure of 0·75 of an atmosphere. This was very natural, for of course the evaporating C. A. in the bath would cool itself to a temperature below that required for a pressure of one atmosphere (just as water or ether will do so), and as there was no gas within the tube but C. Acid, it would take the same tension of vapour, or nearly so, as that outside. By [illegible] the gas from the cold bath one might make it far colder. Perhaps do this with some gases, as Hydrogen, etc., or in freezing some bodies.

6996. The bath was removed and the tube allowed to warm in the air until the C. A. within was partly liquefied, and then the pressure on the guage observed. This was nearly 6 atmospheres, or a little more. Hence solid C. A. can give an atmosphere exerting that pressure. It remained at that point until all the solid was melted, and then the pressure rose.

6997. *Oxygen.*

Pressure to 10 atmospheres; but did not go down.

6998. *Sulphurous acid* in a little tube (liquid). Cooled it in the C. A. bath; at last it froze into a crystalline, transparent, colourless solid, not differing much in bulk from liquid. When partly frozen, it seemed to form good crystals—looking like glacial Acetic acid. When taken out of cold bath, it quickly began to melt, as if its freezing point was not far from the temperature of the bath itself, and much lower than *C. Acid.*

6999. *Yellow condensed Nitrous acid* in a closed tube was cooled in the bath. First the yellow colour of the atmosphere disappeared; then the liquid got paler and paler and was at last very little coloured, and then it froze into a white crystalline solid, not so clear as Sulc. Acid, but as if crystals were finer and more needle like, so that the mass was rather opaque or translucent. It easily freezes, the temperature probably not being very low.

7000. Have used the same tube for all the attemp[t]s on the condensation of Olefiant gas, Hydrogen, Oxygen, etc. etc. as yet, and it stands well.

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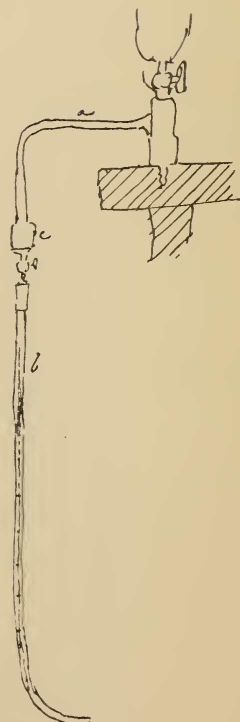
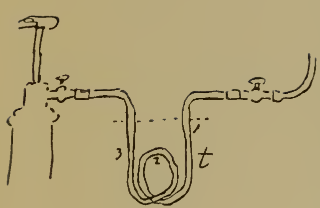
7001. Worked with 1 inch syringe (of 25th May), but used a different condensing tube, thus: *a*, a small metal tube continuing the syringe down to *c*; *b* is the condensing tube, hermetically closed and bent below, capped above, as before, and having a



little stop cock screwing into *c*. Used leaden washers every w[h]ere and they stood well. Then applied the E. C. A. bath at lower end of the tube *b*.

7002. To empty the air or a previous gas out of *b*; this was done: Nitrous oxide, for instance, was forced by the syringe through the apparatus, the cock at *c* being a little open until all was full but the tube *b*; the cock was then closed—10 atmospheres pressed into the tube; the whole left a while to mix—the cock at *c* loosed until guage (in the tube *b*) stood at one atmosphere—then closed again—10 atmospheres put on as before—and this was repeated a third time, so that very little air could remain. When a fluid was condensed in bottom of *b*, then it was easy to expel any trace of air by letting a little of the fluid escape as vapour.



7003. A new expedient for drying the gas was used. A glass tube the $\frac{1}{3}$ of an inch in diameter was bent into a coil *t*, and by caps attached to the inlet of the syringe and the end of the flexible tube proceeding to the gas jar or gazometer; then the coil of *t* was put into a glass and kept at 0° by a frigorific mixture. On after examination, it was found that all the water that could be taken away by this degree of cold was deposited in the anterior part of the tube between 1 and 2 as ice; none appeared at 3.

7004. *Nitrous oxide*. This gas (as was to be expected from the old experiments) condensed and formed a liquid. It did not freeze by any cold of the E. C. A. bath. On leaving it to obtain the temperature of the bath (after shutting the cock at the top of the condensing tube), the pressure was then observed. The guage had 10 divisions and the mercury stood at 3 divisions (indicating 3·3 atmospheres), but on opening the stop cock a little the guage rose. When at 6 divisions it was reclosed, and then, being left a while, the mercury returned towards the fifth division. Being opened again, so as to stand at the seventh division, it was reclosed, and when the fluid had re-attained the temperature of the E. C. A. bath, the guage returned and rested at the sixth division, i.e. the air of ten volumes was compressed into 6 volumes. Hence the pressure of the vapour of liquid nitrous oxide at the temperature of the E. C. A. bath is 1·66 atmospheres only.

7005. I covered over the E. C. A. bath with a cover of tin foil, so as to obstruct the access of air and cause the bath to have the temperature belonging to it at the true pressure of 1 atmosphere. This, by making the bath a little warmer, also caused the mercury in the guage to move, shewing increased pressure within; but the difference was not more than a $\frac{1}{10}$ of a degree of the guage used.

7006. Though a pressure of 1·66 atmospheres was that exerted by the fluid nitrous oxide, yet it required far more than that to bring down the gas into the fluid state; and the guage often shewed 1 atmosphere[1] on pumping in, but then this soon fell as the heat evolved by condensation was transferred through the glass and removed by the bath on the outside. It was beautiful to observe that as the gas became liquid within, so the solid C.A. in the ether on the outside became gas and rapidly disappeared. It served for one indication that condensation within was going on.

7007. But when fluid was condensed and left to cool, still the pressure remained high, or at 3·3 atmospheres. This shews, either that air was in the gas which accumulated as the condensation went on, or that nitrogen or some other substance than nitrous oxide, and of a higher pressure, was there. This could of course be separated by letting it off afterwards; as was done.

7008. Made a new experiment with the same gas. Liquid came down easily when guage kept at 1 division or 10 atmospheres—loosed up to 6 divisions—closed the cock—the guage returned to 4 divisions—repeated this two or three times, and at last the mercury in the guage stood at $6\frac{1}{4}$ divisions, with a good E. C. A. bath and plenty of fluid nitrous oxide in the tube. Repeated this two or three times more. Settled now at $6\frac{1}{4}$ divisions, or 1·6 atmospheres at temp. of E. C. A. bath.

7009. Took off the condensing tube with its cock, etc. Opened the cock full. Liquid boiled—fell in temperature until it was so cool as to sustain only a pressure of 1 atmosphere, at which the guage stood. At this time it must have been much colder than solid Carbonic acid, but it would not freeze itself. So not solid at very low temperature. On closing the stop cock and putting the end of the tube into the E. C. A. bath, the latter made the nitrous oxide boil vigorously and the pressure rose to $6\frac{1}{4}$ divisions as before, or 1·6 atmospheres.

[1] ? 1 *division*, indicating 10 atmospheres.

7010. I put the closed end of the tube containing the nitrous oxide against a piece of ice. The liquid boiled and the ice froze—the guage rose to ½ a division or 20 atmospheres, yet the ice was hard and dry and not at 32°. The liquid then disappeared; but whether I had not enough to supply the capacity of the tube with gas, or whether a leak had come on, I did not ascertain. At all events, the pressure at 32° is more than 20 atmospheres.

7011. Also the cement of the cap joints stands 20 atmospheres, and the tube also.

My former result is 50 atmospheres at 45° F.

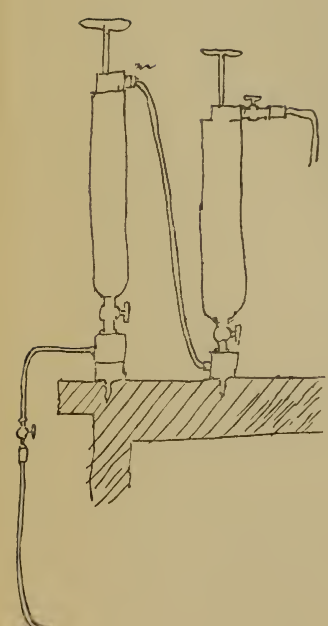
7012. *Nitric oxide or Nitrous Gas.*

Whilst all the parts of the apparatus were filled with nitrous oxide, I transferred the end of the flexible draught pipe to a jar of well washed Nitric oxide, and then introduced that gas into the apparatus (7001). This was done very well—no signs of the formation of nitrous acid appeared any where nor was the syringe or its valve in any way affected.

7013. I raised the pressure to 20 atmospheres, and the E. C. A. bath was good, but I could not obtain any signs of condensation. The valve of the syringe however began to leak, and I must repeat this with two pumps. The cement, the tube, and all other things held well.

7014. *Coal Gas.*

Succeeded the *Nitrous Gas* by coal gas, so as safely to remove the former. Then put on pressure with coal gas, but obtained no effects with pressure of 10 atmospheres. Repeat.



7015. Have had an iron syringe of ½ inch bore made for higher pressures. Mounted it in conjunction with the brass syringe of 1 inch diameter, connecting the two by a pewter tube and screws. Found afterwards that the screw at *m* was not tight and so only an imperfect effect was obtained.

7016. *Nitrogen.*

Pressure raised to 20 atmospheres, but no signs of going in a good E. C. A. bath.

7017. *Oxygen.* No.

7018. *Hydrogen.* No. This gas shews its leaking powers very much at these high pressures. Put collar of soft cement in at *m* and found excellent effects from it.

7019. *Carbonic oxide.* No; even when guage mercury stood at $\frac{2}{5}$ of a division or 25 atmospheres.

7020*. Now laid aside the pumps to have better unions and connecting pipe, and worked with open receiver, made out of bent tubes, introduced into the E. C. A. bath; and sent in certain gases from little generating vessels.

7021. *Chlorine.*

Easily condensed into a liquid—so easily that there was no *smell* of chlorine at the open mouth of the receiver. But there was no signs of freezing. So this body requires a very low temperature for its solidification.

7022. *Hydriodic acid.*

Generated from Phosphorus and Iodine. Easily condensed. Was a little coloured by a trace of free iodine. So easily condensed that very little fume appeared at the mouth of the receiver. It was easily *frozen* by E. C. A. bath and did not then fume. It was a brown (from free iodine) crystalline substance. On lifting it out of the bath and warming the tube with the finger, the solid melted, formed a pink fluid (from the iodine), and quickly after gave off fumes of hydriodic acid from the mouth of the receiver, as if it were then at 1 atmosphere of pressure.

7023. Hence probably its solid has less pressure of vapour than 1 atmosphere. Its liquefying point seems to be not much below its boiling point at pressure of 1 atmosphere, and both of these are probably not far distant from 0°; perhaps above it.

7024. *Muriatic acid.*

Gave no signs of going down at pressure of 1 atmosphere, i.e. in an open tube receiver at temp. of E. C. A. bath.

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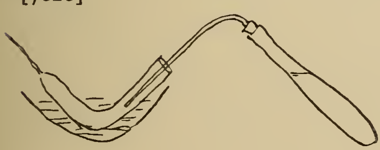
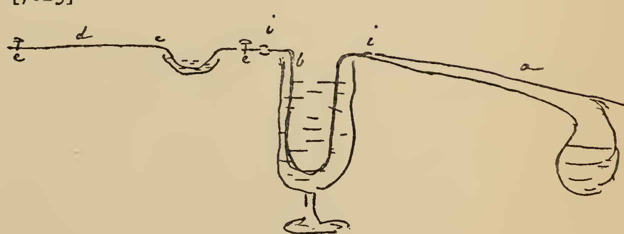
7025†. Worked without the pumps on Gases which could not be introduced by its means. *a*, a retort for evolution of silicated fluoric gas or other gases; *b*, a bent tube, dipping into a mixture of ice and salt, to condense and retain water; *c*, the condensing tube with its E. C. A. bath; a pressure guage being in the part *d*; *e*, *e*, stop cocks; *i*, *i*, caoutchouc tubes to connect the parts of the apparatus. In this form the end stop cock was left open, and

* [7020]



† [7025]



only the influence of the cold used to condense the gas sent through.

7026. *Silicated fluoric gas* was evolved from the retort and passed through the apparatus, but I could get no signs of condensation. All the glass parts of the apparatus looked clean and dry inside and unaffected, and it seemed as if the gas might be passed through the pump. On taking down the apparatus, the caoutchouc connectors seemed partly affected and I doubt whether the pump may be used.

7027. May perhaps use a small retort and put a pressure of three or four atmospheres on it.

7028. *Sulphuretted hydrogen.*

Used the same form of apparatus for this gas (7025), evolving the gas from Sulphuret of Antimony and Muriatic acid. The gas easily condensed, so that none issued from open stop cock, and the liquid quickly filled up the bore of the tube in the bend. It was colourless, transparent, not solidifying—and consequently a liquid when its own vapour has a pressure of one atmosphere; but at how much lower pressure or temperature, cannot say yet.

7029. The two stop cocks were closed and the condensing tube and its contents disengaged from the rest of the apparatus. It was then, at the bent part, put into baths of different temperatures and the pressure ascertained.

In bath of salt and ice, kept moving,	i.e. at 0° F,	the pressure was	6·9	atmospheres.
,, ,, ice and water,	i.e. at 32° F	,, ,,	9·0	,,

With one guage of 45 parts.

7030. With another guage of 10 parts, the pressure in a bath of 0° was only 5·55 atmospheres. In this case, all common air was well blown out of the tube; in the former case, I am not so sure. Must try again.

7031. *Ammonia.*

Used the same form of apparatus (7025). The gas very readily condensed, and plenty was obtained. It was a colourless fluid, clear, not freezing at the temp. of the E. C. A. bath or pressure of one atmosphere. Must go below that.

At 0° the pressure was 2·56 atmospheres—average of 4 observations; the last and best expt. was 2·43 atms.
32° „ „ 4·22 „ —average of 5 different observations.
60° „ „ 6·82 „

Gas let off—all well.

The observations above were made, letting off gas each time at the stop cock, so as to clear out all common air and get a true result.

7032. *Hydriodic acid.*

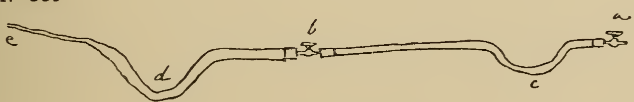
Used the same apparatus (7025). Obtained the hydriodic acid by putting into the retort *a* Phosphorus and iodine, then adding water (a little) and applying heat carefully. Almost every thing except hydriodic acid was stopped before the bent tube *b*—or else in it.

7033. The hydriodic acid condensed readily, freezing so as to block up the passage, and as this might embarras the passage of air out of the retort, the bath of E. C. A. was lowered from the tube and so applied that the contents might be liquid. No hydriodic acid passed out at the furthest stop cock and no fumes there whilst the condensing tube was kept cooled. When enough of the body was condensed, the cock near the bend was shut, and also the other cock, and the apparatus detached. Then a second tube, as in the figure*, attached to it, this tube being drawn out to a fine bore at *e*. The cock *a* was shut—*b* open—and *e* open—and then *c* allowed to rise in temperature until hydriodic acid flowed freely from the aperture *e*. Then *b* was shut for a moment—*e* sealed hermetically by a flame—afterwards *b* opened and *d* cooled in the E. C. A. bath. Immediately the hydriodic acid distilled over from *c* to *d*, leaving a little impurity of iodine at *c*—and lastly, closing the cock *b*, and unscrewing the tube *c* from it, a fine pure specimen of hydriodic acid was obtained.

7034. Whilst the acid was in *c* and *c* in the E. C. A. bath, the acid was solid. Lowering the bath, the acid soon liquefied, and it was not difficult to have liquid and solid together in *c*, and yet gas passing over them and escaping at the open terminal cock. So that the solidifying point—the liquefying or boiling point at *one* atmosphere cannot be very far apart.

7035. Solid hydriodic acid is a clear transparent colourless

* [7033]



substance having fissures or cracks in it something like a piece of ice, and appears to crystallize beautifully.

At 0° F. its pressure was 2·9 atmospheres.
32° ,, ,, 3·97—average of 3 obs.—last observation was 4·2 at.
60° ,, ,, 5·86—average of 2 obs.

7036. The hydriodic acid soon began to escape by the stop cocks, issuing out at the ends and passing the plugs. I found that it soon acted on the cement of the caps and also on the tallow of the plugs, and so got round the latter. It also soon acted on the brass, producing an iodide of copper and zinc, etc.; on the cement it acted, dissolving it and making a black acid mixture, fluid under pressure, but which, on relieving pressure, gave off pure hydriodic acid and left a black charry mass in small quantity.

7037. It also quickly spoils the guages by acting on the mercury and also on the Brunswick black of the graduation marks.

7038*. *Cyanogen.*

Easily condensed at common pressures, sending it in from a tube retort *a*, attached at once to the tube *b* by a screw connector.

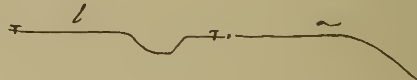
7039. Easily obtained either as solid or liquid—colourless, transparent, crystalline, etc.

7040. When there was mixture of solid and liquid in the tube, the pressure of the vapour was rather less than 1 atmosphere. The temperature at that point I do not yet know.

At temp. of 0° was *liquid* and pressure of 1·55 at.—mean of 4 obs.
,, 32° ,, ,, ,, 2·2 ,, —3 ,,
,, 60° ,, ,, ,, 3·85 ,, —3 ,,

7041. Ether
Alcohol
Sulphuret of Carbon } do not freeze in E. C. A. bath.

* [7038]



7042*. Put up two pumps (7015), with the cooling tube in ice and salt (7003), and used vertical condensing tube (7001). All was in good order except the valve at bottom of half inch syringe, which did not hold, but the valve in the piston held and also the stop cock *b*. The inch syringe was good and also its pipe connection with the second syringe.

7043. Had three steel stays for half inch syringe to keep it steady and prevent working of the leaden collars at its bottom. Used Leaden and wax leather collars; found both tight. Oil pumped through the pistons into the pipe *p* and a little once entered the cock *b* and tube—must be avoided. Had good E. C. A. baths for all the following results.

7044. Oxygen would not condense at 27 atmospheres.

7045. Nitrogen would not condense at 27 atmospheres.

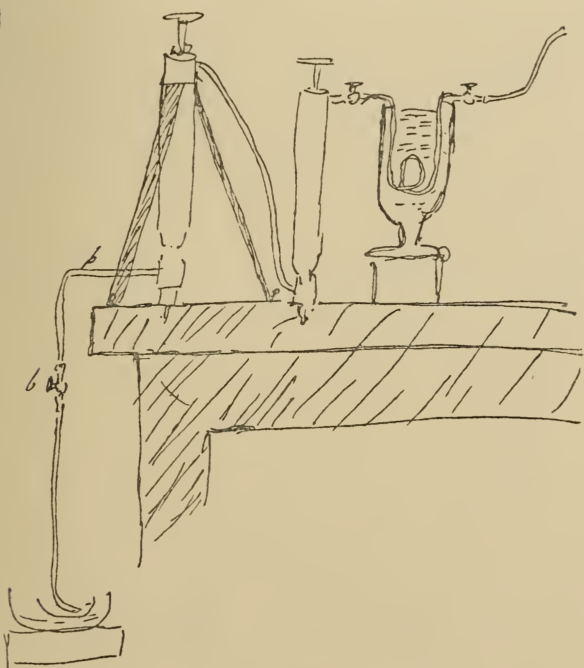
7046. Hydrogen would not condense at 27 atmospheres.

7047. Nitrous Gas would not condense at 27 or 28 atmospheres.

7048. Carbonic oxide would not condense at 27 or 28 atmospheres.

7049. Olefiant gas. Condensed—liquid—would not freeze. Pressure was then 9·6 atmospheres and kept there. As there might be some air in tube (7002), opened the cock to sweep it out until guage about 4 atmospheres, then shut it. Liquid boiled within and froze something round it from the E. C. A. bath on the outside, shewing the extreme cold produced within—greater than that of the E. C. A. bath. Cock closed and liquid Olefiant left to gain temperature of E. C. A. bath; pressure rose to 8·7 atmospheres. Again opened the cock and lowered pressure—shut and

* [7042]



left it: rose to 8 atmospheres. Think that is about right for the present case, but in the former case the pressure was only 3·7 atmospheres (6989). Have three or four times thought there were two or more bodies in Olefiant gas. Must work this matter out. Variations of pressure at temp. of E.C.A. bath, when liquid in the tube, are very great.

7050. Probably do something by ice and salt bath and *dissolving liquids* in the condensing tube, as oil turpentine, alcohol, etc., using pressure also—try.

7051. Coal gas. Not condense at 27 or 28 atmospheres. Remember that the pressure at which a gas first condenses seems much greater than that the liquid can sustain. This is perhaps because of air in the gas, or in the condensing tube, and on the whole a condensing tube through which there is free way for first filling of it with gas is better than one closed hermetically at one end.

7052*. *Phosphuretted hydrogen.* A retort evolving phos. hy. from solution of potash and Phosphorous was connected by caoutchouc connectors with a cooling tube in a mixture of ice and salt, and that with a condensing tube in a bath of E.C.A. Phosphuretted hydrogen was sent through until it issued with its ordinary characters at the final stop cock, but none condensed in the E.C.A. bath; all well arranged, but this gas will not go down at pressure of one atmosphere. Might try it without much difficulty with pressure by use of the half inch syringe.

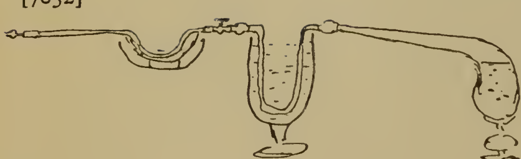
7053. *Arsenuretted hydrogen.*

Arranged in the same way, except that a glass tube from end of condensing tube convey the passing Gas into Jars over a water trough. The gas obtained from Zinc and white arsenic by dil. S.A. No signs of condensation. Hence doubt the statement in Graham—but as there might be hydrogen enough to carry off the volatile Ars. hydrogen, must procure the gas from an alloy of Zinc and Arsenic.

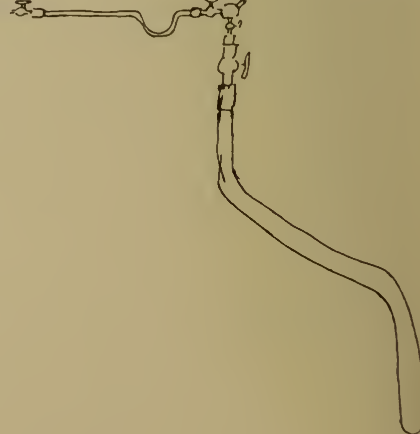
7054†. *Muriatic acid gas.*

As it does not seem to go down at pressure of 1 atmosphere

* [7052]



† [7054]



(7024), made a large tube retort, put in pieces of Mur. ammonia and oil of vitriol, and when they were mixed, connected this retort by a right angled connector with the condensing tube. When pure M.A. Gas issued from end of condensing tube, shut that cock and let the pressure rise. Liquid Mur. acid condensed in the bent part but some of the mixed materials were also carried over. The pressure was 1·6 atmospheres at the E.C.A. bath temperature. Took of[f] the generating vessel and then by an excellent E.C.A. bath open to air and with plenty of carbonic acid in it, the pressure was 1·33 atmospheres.

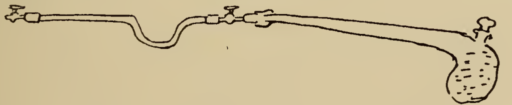
7055. Put the bent part into a bath of ice and salt assumed to be at 0° and moved it well about. The pressure was then from 10 to 12 atmospheres. Leaking *through* the cock came on, as if M.A. easily affected and loosed the oil or dissolved in it and passed; and as this would lower the temperature within, probably 12 or 12½ atmospheres is not too high a pressure for temp. of 0°.

7056*. *Fluoboron gas* (7287). Evolved from fuzed Boracic acid and fluor spar by oil of vitriol and heat. The retort connected by a caoutchouc connector with the condensing tube and a pressure of one atmosphere only employed. No signs of condensing by E.C.A. bath. Plenty of good gas went through.

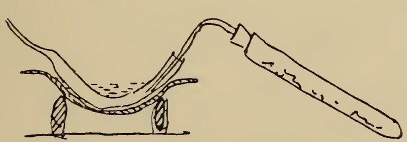
7057. *Fluosilicon Gas.* Arranged as Muriatic acid Gas (7054) except that the generating tube was of Green glass and as wide as my little finger in internal diameter. Had plenty gas through the apparatus and shut final cock until pressure was 4 atmospheres —still fluo silicon would not condense. So more difficult than M.A.

7058†. Mixed some bromine and phosphorus in a tube. Had an explosion and burnt my fingers. Saved enough bromide of phosphorus to prepare *hydrobromic acid* by heating it with a little water. Sent the gas acid into a little open receiver in an E.C.A.—it condensed—a little bromine and impurity passed over with it—but the pure, colourless, limpid and very liquid hydrobromic acid could be decanted off from this matter into a clean part of the condensing tube and then shewed its proper characters. No signs of solidification. Its pressure of course less than 1 atmosphere at temp. of E.C.A. bath. When taken out of the bath, the fluid quickly disappeared, producing hydro bromic gas.

* [7056]



† [7058]



7059. Better make Bromide of Phosphorous by putting bromine at *a* and phosphorous at *b* and letting vapour of bromine pass over and combine with phosphorous—or better still thus*, and then can attach *c* to a condensing tube, and by a little water in at *d* can evolve hydrobromic acid and get its pressure at E. C. A., at 0°, etc. etc.

Is more condensible than M. Acid.

7060. Now experimented with E. C. A. bath under air pump receiver. On taking off pressure of atmosphere, the bath began to boil, shewing the evaporation of the C. Acid, and it must have become *very cold.*

7061. One good effect is the exclusion of the air and its moisture; there was much less water frost about it.

7062. Found it easy to keep the bath in this very cold state a quarter of an hour or more, even when quantities not more than I had used up to this time.

7063. The ether remains as fluid as ever at this low temperature.

7064. Sulphuret of carbon in a little tube into this cold bath still as fluid as ever.

7065. Alcohol absolute into this very cold bath. Did not solidify or become turbid, but it thickened considerably and in that respect looked like a colourless limpid oil. When the little tube was inclined to and fro the alcohol poured from side to side with difficulty.

7066. As the Oleft. liquid in evaporating produces far more cold than the E. C. A. bath, so it might be used as a bath for the condensation of still more refractory gases, assisted by pressure.

15 JUNE 1844.

7067. Prepared some *fluosilicon* gas over mercury in different Jars. Put up into it the following different substances: Copper; Zinc; Iron; Caoutchouc; Cap cement; soft cement; Olive oil. In two hours none sensibly acted upon. On the 17th, or 48 hours after, no action on any—except perhaps the oil a very little. This gas may be used with the compressing syringes.

7068. On the 15th prepared also *fluoboron gas* and put up the same substances. In the case of oil, there was some action immediately, especially with a little agitation. The soft cement also

* [7059]

was apparently acted on. On the 17th, or in 48 hours, the oil was much acted on—also the soft and cap cement and the india rubber; the three latter being affected on the surface. Perhaps this gas might be used in a syringe quickly, but that is doubtful because of the ready action on the oil. The metals had not been acted on.

10 JULY 1844.

7069. At Hampstead. Arranged a silvered glass concave mirror, $11\frac{1}{2}$ inches in diameter, with a frame to sustain balls like peas in the focus, that they might be submitted to solar radiation of an intense kind.

7070. When a ball of chalk fixed on a platina wire was in the focus, it was most beautifully luminous, so as, when looked at from the garden against the bright sky, to shine as a little sun. It was very quickly heated so as to crack: become caustic lime and become white hot on the side towards the reflected rays, and being applied to paper, instantly burnt it. After the first few heatings, 8 or 10 seconds in the focus was enough to make the ball red hot and able to fire paper.

7071. Whilst the ball *remained in the focus*, the light therefore intense.

7072. Also the rays of heat from it were strong to the finger held near; and if a piece of crown glass was between the finger and the ball, the rays passed abundant through it; and a double concave lens being held about 5 inches from it, heat was found in the secondary focus. Thus the rays of heat from it have the character of the sun's heat rays in passing through and being refracted by glass. The lens was about 2 inches in diameter.

7073. The rays from the ball in focus easily affected paper with chloride of silver made on it—as was to be expected.

7074. The ball heated in focus—and brought out of focus—radiated much heat to the finger.

7075. The ball heated in focus and brought out of focus near to finger, with crown glass intervening—finger not feel heat so soon, but did after a few moments, but then the glass had become hot—so that rays to the finger probably principally if not entirely from the hot glass.

7076. No length of heating seemed to make the ball's rays more able to pass through glass, i.e. continued heating did not appear to dispossess the lime of terrestrial heat and fill it with solar heat. Solar rays in conversion into sensible heat give no power of re-originating rays having solar properties more than common heat would do.

7077. Crown glass converts many solar rays into sensible heat. A piece of crown glass held in the focus, so that rays may pass into and across it, becomes *visible*, shewing thus that all light rays do not go through; and becomes very hot, shewing that many heat rays are stopped.

7078. The back of the lime ball in the focus did not radiate heat rays concentratible by a lens, as the front or incandescent side did (   ).

7079. *Retort deposit carbon.* A ball of this substance in the focus rapidly heated and gradually burnt away—only slowly. It, when thus heated, emitted rays of heat more freely than chalk (due to its emissive power), but it was otherwise as the lime, and presented no new phenomena.

7080. *Phosphate of lime.* A ball made out of a cupel: heated as the rest, but it emitted very little heat as compared to the plumbago.

7081. *Little glass globe* alone: nothing particular.

7082. Do. filled with powdered fluor: very luminous whilst in the focus but nothing particular when brought out of it.

7083. Tried to obtain effect on chloride of silver in a dark box by taking balls suddenly out of the focus and introducing them, but obtained no evidence of the emission under such circumstances of chemically acting rays.

7084. So no appearance thus far of any communication by sun's rays of a power of radiating solar rays to such bodies as these employed; no retention of peculiar or solar character. Still not decisive. The inquiry wants a good Daguerre or Talbot apparatus and preparation.

22 AUG. 1844.

Condensation of gases again.

7085*. Have arranged to compress gases by pumps into tubes cooled in E. C. Acid baths placed in vacuo so as to apply the lowest possible cold to them. The pumps and cooling tube were as before (7042, 7015, 7003); but the pipe carrying the gas from the ½ inch pump was of brass, small, in three lengths, screwing together at *a*, *b* and *c* by male and female screws with leaden washers between. The form was as in the next page[1]: from *a* to *b* was about 4 feet; from *b* to *c* 4 or 5 feet, and from *c* to *d* about 6 inches. This latter piece *c*, *d* was true on the outside and passed through a collar of leather in the upper plate of an air pump jar, and was attached at its end to the condensing tube. By this means the latter could be raised or lowered at pleasure out of or into the E. C. A. bath placed beneath in the vacuum of the air pump, and the length and flexibility of the brass tube allowed of this motion without any derangement or loosening of the joints. The condensing tube was, as before, of green bottle glass, bent as in the figure and fastened at *g* and *h* to caps as before described. The cap *g* screwed on to a cock, and that on to the end of the pipe *d*, all by leaden washers. The cap *h* had a blank screw plug fitted to it, also with a lead washer. The guage to measure the compression was in the tube from *e* to *f* and easily visible.

7086. I found all the parts of the apparatus to work excellently well together; in that respect the apparatus did all that I expected from it.

7087. In operating, the gas was pumped through as before, but the Plug *h* was left a little open and glass blown through until all the apparatus was full; then that was screwed up and the operation proceeded with, and in this way several gases were

[1] i.e. as shown in the diagram.

* [7085]

submitted to a high pressure at the temperature of a bath of E. C. A. in a vacuum of $28\frac{1}{2}$ inches.
7088. First for this temperature. I have had an Alcohol thermometer made, the bore of which was previously examined. The graduation begins at about 65° and then descends to 192° below 0°. 60°, 32° and 0° were correctly ascertained and the degrees below were accurately marked off of the same dimension (i.e. capacity) as these.
7089. A good Ether Carb. Acid bath was prepared (I had a magnificent charge of liquid C. A. from Mr Addams) which well covered the bulb of this thermometer. The thermometer gradually fell until it stood at nearly 100° below 0° F. The whole was then put into the air pump jar and the air removed. Of course the bath mixture boiled and the temperature fell. The barometer of the air pump was raised to 28·3 inches; that outside being at 29·9. In this state of things the thermometer fell to 160° below 0°. Then let in air again, took off the bell glass, added a little ether and then some more solid carbonic acid (though plenty was still in the bath), so as to bring the bath up to its air temperature, and the thermometer gradually rose to 106° below 0° and there stood very steadily.
7090. So the E. C. A. bath in air may be taken at 106° below 0°; the same bath in vacuo at 160° below 0°, and the latter as 54° colder than the former. It was from this difference that I hoped for some effects of condensation not obtainable without.

Now experimented with different gases.

7091. *Oxygen.*

The arrangement acted well—all quite tight—leaden washers and joint stand well. There was plenty of C. A. in the bath the whole time. The air pump barometer was at 28 inches and the pressure in the condensing tube was 25 atmospheres, *but* there were no signs of condensation. The condensing tube continues much clearer on the outside, and is more easily examined as to its contents at the cooled part than if in the air, because there is no water in the surrounding atmosphere to condense on it.

7092. *Hydrogen.*

The air pump barometer was at 28 inches and the E. C. A. bath good. I could not raise the pressure guage above 18 atmospheres, for after that the leakage either by the piston or through the valve or elsewhere counteracted my efforts. At that it would *not* condense.

7093. *Nitrogen.*

Made from Air by burning plenty of Phosphorus in it and leaving the gas over water many hours. E.C.A. bath good and air pump barometer at $28\frac{1}{2}$ inches. Now here I was able to raise the pressure to 56 atmospheres, and in this there was no mistake, and yet the gas would *not* condense.

7094. The pumps and valves held wonderfully well, and the difference between this experiment and the one with hydrogen which immediately preceeded it I can only refer to the difference of leakage in consequence of the physical qualities of the gases. Nitrogen leaks the least by far, or has most difficulty in passing through minute apertures. Oxygen leaks more easily and Hydrogen far more easily. These effects fully agree with my old results.

7095. They should be compared with Graham's diffusion expts. etc. etc. There seems to be a little anomaly somewhere among them, if my first experiments at low pressures be right.

7096. *Nitrous Gas.*

The apparatus was first filled with hydrogen, then Nitrous gas was sent through, and at the close of the experiment hydrogen was used to displace the Nitrous gas. In this way the pumps acted freely—were not affected—and all went on well. The bath was good; the vacuum barometer at 28 inches; the pressure on the N. Gas as nearly as possible 50 atmospheres. But no signs of condensation.

7097. *Carbonic oxide.*

Well freed from Carbonic acid by standing over lime in water for 48 hours. E.C.A. in good state. Vacuum air pump at 28·5 inches. Pressure on the guage in condensing vessel was 40 atmospheres, but *no condensation.*

7098. The lower valve of the Iron half inch pump now began to fail, and the piston, when depressed, would slowly rise again.

7099. *Coal gas.*

From London supply. E.C.A. good. Air pump barometer at

28·5 inches. Pressure on the gas 32 atmospheres. *No condensation.* Iron pump becoming worse.

Ceased for the present the pump experiments.

7100. *Arseniuretted hydrogen.*

Have made alloy of equal weights Arsenic and Zinc, and acting on this by strong Mur. Acid, obtained Arseniuretted hydrogen. The retort cooling tube and condensing tube, as also surplus tube, was like that used before for Phos. and Ars. hydrogen (7052, 7053). The gas easily condensed; a few bubbles continued to travel on to the trough, but these were probably due to a little free hydrogen, for the Arseniuretted hydrogen thus obtained has not a pressure of one atmosphere in the E. C. A. bath (See Graham's Elements, 1842, p. 631; also Dumas and Soubeiran, Annales de Chimie et de Physique, xliii). Fluid did not solidify.

7101. The bend containing liquid was put successively and alternately into 2 baths, one of ice and salt well mixed, the other of Ice and water, and several observations made agreeing well together and giving, as the average result, a pressure of 6·2 atmospheres for Ice and salt or 0°; and 8·7 atmospheres for Ice or 32°.

7102. The following substances in tubes were submitted to the cold of the E. C. A. bath both in air and in vacuo. The fluid *chloride of carbon*, and hydro chloride of carbon or chloric ether: both solidified in the E. C. A. in air, or at 106° below 0°. *Caoutchoucine* and also *camphine or pure oil of turpentine* did not solidify, even in the E. C. A. in vacuo, or at 160° below 0°.

7103. The charge of Solid Carb. Acid from Mr Addams was excellent and in the evening I had not used it all out.

7104. May seal up some gases by use of E. C. A. bath. Sealed up a little chlorine.

7105. *Alcohol thickens* in E. C. A. bath in air; so also does *Caoutchoucine* and *oil of turpentine.*

26 AUG. 1844.

7106. *Sulphurous acid.*

Worked with it to get certain pressures and succeeded very well.

At 0° F.	was	pressure of	0·91 ?	atmospheres.
10°	,,	,,	1·00	,,
32°	,,	,,	1·60	,,

At	43°·5 F.	was pressure of	2·00	atmospheres.
	60°	,,	,,	2·76	,,
	90°	,,	,,	4·30	,,
	115°	,,	,,	6·25	,,

The five first results are the average of several good experiments. The two last are not so certain—for them the whole apparatus was immersed in baths of these temperatures.

7107. Sulphurous acid dissolved the resinous cement of the Brunswick black used to mark the guage.

27 AUG. 1844.

7108. Worked on *Cyanogen* to get certain pressures; succeeded very well.

At	0° F.	was pressure of	1·66	atmospheres.
	18°	,,	,,	2·00?	,,
	32°	,,	,,	2·70	,,
	40°·5	,,	,,	3·00	,,
	45°	,,	,,	3·33	,,
	60°	,,	,,	4·17	,,
	58°	,,	,,	4·00	,,
	72°	,,	,,	5·00	,,

7109. After many results the numbers in ice and water retnd. to 2·7 atmospheres and the guage was in excellent order. The results are good. Put the cyanogen tube aside.

28 AUG. 1844.

7110. Found the *Cyanogen tube* to-day, after 30 hours, just as yesterday—no leakage of the cocks or plugs, or apparent action externally on the cement.

29 AUG. 1844.

7111. The *cyanogen* tube as sound as ever.

Proceeded to ascertain what volume of liquid there was and what volume of gas it would produce. The temperature of the apparatus, liquid and mercurial trough and place was 63° F. The Barometric pressure 30·2 inches.

7112. The extent of the liquid in the tube was observed against a guage or scale. Then a small bent conducting tube was screwed

on to the stop cock end of the tube and the gas let out over mercury. It was all well received, and when at atmospheric pressure, equalled 6·6 cubic inches. When out, the tube was dry and free and the air pressure guage had returned to 10 divisions, or its first point, shewing all was right.

7113. Then distilled water was introduced and its quantity adjusted until it as nearly as possible equalled the original volume of fluid cyanogen. This quantity of water weighed 3·8 grains.

7114. As the cyanogen atmosphere was at first under a pressure of   atmospheres (at 63° F.), and when the vessel was opened fell to 1 atmosphere, it was necessary to know the capacity of the vessel. It was found to be 0·2 of a cubic inch.

7115. The tube in which this cyanogen was kept was one bent as above, of green glass, capped at each end, the short end closed by a blank screw and leaden washer and the long end by a stop cock; but immediately that this tube was separated from the generating vessel, a brass connector was screwed on to the outer end of the stop cock and a blank screw into the brass connector—all with leaden washers. It was this dumb cap which kept all so tight, for I find that all these cocks have a tendency to leak slightly round the plugs—not out by the side of the plug, but round it and out at the end of the cock. So when this cap was taken off to-day, indications of a leak instantly appeared. Hence must always close up with a blank end.

7116. On taking the apparatus to pieces, I found that the ring of cement in contact with the atmosphere of cyanogen had absorbed a little and rendered it soft; but this extended only a little way in and did no harm. It caused that part to swell inwards and soften.

7117. The bulk of 3·8 grains of water is 0·01505 of a cubic inch, and this is the volume of the liquid cyanogen.

7118. The pressure of cyanogen at 63° is about 4·36 atmospheres; hence 4·36 − 1 or 3·36 times the capacity of the condensing tube, or 0·672 of a cubic inch, is the quantity of gas which would be sent into the mercury jar from the mere expansion of the compressed atmosphere of cyanogen. This, subtracted from 6·6 cubic inches, leaves 5·928 cubic inches as the gas from the liquid.

Now 0·01505 : 5·928 : : 1 to 393·9 nearly.

So 1 volume of liquid cyanogen at the temperature of 63° expands into 393·9 volumes of cyanogen gas at the same temperature and at a barometric pressure of 30·2 inches.

7119. If 100 cubic inches of Cyanogen weigh 55·5 grains (Brande), then 393·9 cubic inches will weigh 218·6 grains, and this is the weight of a cubic inch of liquid cyanogen. Hence its specific Gravity is to water as 0·866 nearly to 1·000 at or about 63°.

7120*. *Ammonia.*

Worked on *Ammonia* to obtain its pressures. 200 grains of finely powdered Mur. Amm. and 150 grains of recently heated quick lime, also in fine powder, were well mixed in a mortar and put into a green glass tube retort. This then had a cap cemented on to it ( ), and was connected by a stop cock to a bent green glass cooler tube furnished with caps at each end. This cooler tube was immersed in ice and water. Its further extremity was connected with a condensing tube and guage like that just described for cyanogen ( ). Heat of a spirit lamp was then applied to the generating retort and the terminal screw of the condensing tube left a little open. Ammoniacal gas was blown through, and when issuing freely, the terminal screw was closed up tightly. After that the guage was watched and heat applied so that the mercury should indicate pressure outwards. Then a bath of ice and salt was applied at the bend of the condensing tube, the heat continued, and gradually ammonia beautifully appeared in the liquid state there. When enough for my experiments was collected, the cock *c* was closed; the condensing tube disjoined there from the rest of the apparatus; the blank cover or cap (7115) screwed on; the cock itself screwed up tight and all made quite home. Now the ammonia was safe and acted well on the guage.

7121. Proceeded to obtain the relative pressure and temperature at certain points. Whilst the temperatures were below 60°, I immersed only the bend containing the fluid in the respective baths of temperature; but above 60°, as also for Cyanogen, etc., I used a bath in a jar, so that the whole apparatus was immersed; otherwise the liquid would have distilled.

* [7120]

At 0° F.	the pressure	was	2·63	atmospheres.
8°	"	"	3·00	"
21°	"	"	3·75	"
26°	"	"	4·00	"
32°	"	"	4·69	"
38°	"	"	5·00	"
45°	"	"	5·55	"
50°·1	"	"	6·00	"
60°	"	"	6·82	"
61°	"	"	7·00	"
68°·5	"	"	8·00	"
74°·7	"	"	9·00	"
82°	"	"	10·00	"

The degree at 32° and at 0° was verified after all the other experiments, of which the above are the average, was made. They were exceedingly constant.

30 AUG. 1844.

7122. The *Ammonia* (7120) had kept well unto this morning. The Guage and all in order. The cock, when shut, had been screwed up tight, and that was good, for on taking off the blank cover there was no signs of leakage.

7123. Proceeded to obtain the volume of gas from a measured volume of liquid. The thermometer was at 60° F.; the Barometer at 30·2 inches.

7124. Proceeded as before, successfully, but the mercury in the guage returned to 17 divisions instead of 15, indicating that a little ammoniacal vapour had been gathered up by the air within the guage, probably as the mercury travelled back and exposed parts of the glass to it which had been in the ammon. 24 hours.

7125. The whole volume of ammoniacal gas received was 12·4 cubic inches.

7126. The volume of liquid ammonia (7112, 7113) converted into gas equalled the volume of 2·9 grains of distilled water.

7127. The capacity of the condensing tube (7114) was 0·15 of a cubic inch.

7128. The weight of this condensing tube without its caps was only 86·3 grains and it had stood well—a good tube.

7129. The ammonia (liquid) had a little colour this morning, and though it evaporated away quite readily and dry, it left a slight stain. When all was out, the guage mercury stood at 17 parts, as said (7124); but on shutting the stop cock it in 4 or 5 minutes moved to 15 parts, shewing a little evolution of ammonia from the cement or metal surfaces. On examination, the cement was found apparently untouched, being hard and unchanged in form at the exposed surface.

7130. The bulk of 2·9 grains of distilled water is 0·011487 of a cubic inch, and this is the volume of the liquid ammonia.

7131. The pressure of Ammonia at 60° is 6·82 atmospheres; therefore 5·82 atmospheres of compressed gas escaped. Now the capacity of the tube was 0·15 of a cubic inch, and this minus 0·011487 of c.i. is 0·138513; and this multiplied by 5·82 atmospheres = 0·80614566 c. inches as the expansion of the compressed gaseous ammonia; leaving 11·6 cubic inches nearly as the gas of 0·011487 c.i. of liquid ammonia at 60° F. and Bar. 30·2.

7132. Now 0·011487 : 11·6 : : 1 is to 1009·8 nearly. So 1 volume of liquid ammonia at 60° expands into 1009·8 volumes of ammoniacal gas at the temperature of 60° and Barometrical pressure of 30·2 inches. If 100 inches of ammonia gas weigh 18·28 grains (Brande), then 1009·8 cubic inches will weigh 184·6 grains; and this is the weight of a cubic inch of liquid ammonia. Hence its S.G. is to water as 0·731 to 1 at 60°.

7133*. (7120) full size[1] of the ammonia glass tube.

31 AUG. 1844.

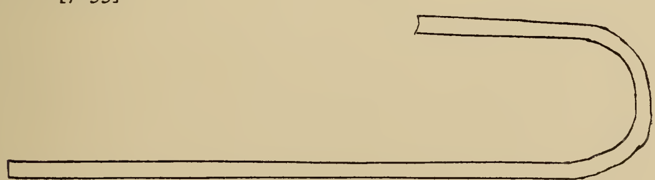
7134. *Chlorine.*

Endeavoured to get the pressures of chlorine, working with the same form of apparatus as that employed for ammonia (7120). Succeeded in getting the liquid chlorine in the condensing tube, but it instantly rendered the mercury in the pressure guage useless, though I had divided it into several portions with air between, hoping to protect the last by the first.

7135. Then made a guage having for its indicating liquid *proto chloride of arsenic*. Found that this dissolved chlorine and that

[1] Diagram reduced to $\frac{1}{2}$ scale.

* [7133]



chlorine so got through to the measured air within and altered its volume to an unknown degree. This therefore failed. Besides, there was very much action on the cement, caps, etc. etc., and certainly this form of apparatus will not do.

7136. *Cyanogen.*

Repeated the expts. with cyanogen, using the same apparatus except that, instead of a screw and cap at the shorter end of the condensing tube, it was drawn out fine, and when cyanogen was issuing freely through, it was hermetically sealed. This form did exceedingly well and there was of course fewer joints to make tight. The screwing up of the stop cock plug screw is a very important point in keeping specimens either for freezing or for pressure experiments.

See results of pressure, next page[1].

7137. *Cyanogen.*

At 0° F.	the	pressure was	1·7	atmospheres (good).
7°	"	"	2·0?	"
20°	"	"	2·45	"
32°	"	"	2·98	"
34°·5	"	"	3·0	"
45°	"	"	3·63	"
55°·3	"	"	4·0	"
60°	"	"	4·444	"
71°	"	"	5	"
84°	"	"	6	"
94°·5	"	"	7	"

These expts. were better than the former, and at the last the guage was found to return correctly to the given indications at 0°, 32°, 60°, etc.

7138. *Sulphurous acid.*

Worked with it in a tube like the last (7136) and a guage with divisions on both sides the 1 atmosphere point.

At 0° F.	the	pressure was	0·653	atmospheres.
12°	"	"	1·0	"
20°	"	"	1·32	"
32°	"	"	1·62	"
43°·5	"	"	2·0	"

[1] i.e. par. 7137.

At 45° F. the pressure was 2·08 atmospheres.

60°	,,	,,	2·75	,,
66°·7	,,	,,	3·0	,,
84°	,,	,,	4·0	,,

7139. This tube and the cyanogen left closed up to remain until Monday. Used Indian ink on the guage this time and also kept a copy on paper for the outside.

SEPTR. 2, 1844.

7140. *Suls. acid.*

Re-examd. the tube of Suls. Acid from Saturday (7138). The liquid acid was all there, but coloured, for it had acted on the Brunswick black graduation marks, washing out some of the colour, and the marks had softened and run down, but the indian ink marks stood well and served well. After observing the results below, the gas and liquid was let off to see whether the air in the air pressure guage returned to its correct point of 10 parts. As the pressure fell, the brunswick black matter here and there frothed from its giving up the Suls. acid: shews the mutual solubility of the Suls. acid and the B. Black medium. The guage returned to 10·5 parts, shewing a little creeping in of Suls. Acid gas to the air, namely a $\frac{1}{21}$ part. This is taken into account in the numbers below.

7141. At 0° F. the pressure was 0·7 atmospheres; but this is too great, for guage did not receed freely at this point.

10°·9	,,	,,	1·0	atmospheres
32°	,,	,,	1·72	,,
63°	,,	,,	3·00	,,
100°·3	,,	,,	5·25	,,

7142. *Cyanogen.*

This tube was in good order (7136) and apparently all the cyanogen there. The Brunswick black marks had run but the Indian ink marks stood. On letting out the cyanogen at last, all evaporated clear and the guage mercury receeded to 18 parts instead of 16, so that a little cyanogen had entered into the air, $\frac{1}{9}$—by the motion of the mercury and otherwise. Account of this increase was taken in the following numbers, which are set down in the order in which they were obtained, so that the irregularities might be compared with the ascending to or descending from a higher temperature. Those above 65° were obtained by baths covering the whole of the tube.

7143. At 18° F. the pressure was 2·43 atmospheres.

12°	”	”	2·25	”
27°	”	”	2·96	”
32°	”	”	3·33	”
84°	”	”	6·75	”
64°	”	”	5·47	”
0°	”	”	1·69	”
100°	”	”	7·5	”
93°	”	”	6·77	”
84°	”	”	6	”
78°	”	”	5·63	”
67°	”	”	5	”
58°	”	”	4·5	”
36°	”	”	3·38	”
61°	”	”	4·5	”
78°	”	”	5·63	”
83°·5	”	”	6	”
92°	”	”	6·77	”

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7144. Have tried Sulc. acid as the liquid in a guage for the pressures of chlorine and think it will answer well—for it holds well in a guage tube and it does not dissolve chlorine.

7145. Might obtain high pressure by having two condensed gases divided by mercury in different parts of a closed tube; and then mingling them, should have the sum of two pressures.

7146. Or if air were compressed to 45 or 50 atmospheres, and then liquid C. A. from another part of the vessel mingled with it, should have pressure of nearly a hundred atmospheres in the vessel, or on a mercury piston or division in it (7148).

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7147. On examining the different data for pressure of gases, have drawn up the following numbers for Suls. acid, Cyanogen and ammonia, in which the *ticked* numbers are the results of the best experiments and the others are interpolated.

Sulphurous Acid °F.	Atmosph.	Cyanogen °F.	atmosph.	Ammonia °F.	atmosph.
√0°	0·66	√0°	1·7	√0°	2·63
				√7·75	3
		8·6	2		
√12°	1				
				17·1	3·5
				√21	3·75
		22·9	2·5		
				25·1	4
				√26	4·06
27°·5	1·5				
√32°	1·65	√32	2·87	√32	4·46
				32·5	4·5
		35·2	3		
				39·3	5
41°·6	2				
√45°	2·125	√45	3·45	√45	5·45
				45·6	5·5
		46	3·5		
				51	6
53°·6	2·5				
		√55·7	4		
				56·2	6·5
√60°	2·8	√60	4·25	√60	6·9
				√60·9	7
		64·4	4·5		
√64°·6	3				
				65·1	7·5
				√68·8	8
		√72·2	5		
				72·3	8·5
74°·1	3·5				
				75·8	9
		79	5·5	79	9·5
				√82	10
82°·9	4				
		√85	6		
√90°	4·45				
		√90·3	6·5		
90°·7	4·5				
		√92	6·68		
		√95	7		
97°·7	5				
		99·1	7·5		
√100°	5·18	√100	7·61		
104°·1	5·5				
109°·7	6				
√114°·9	6·5				

7148. As to obtaining high pressures (7145). Suppose a vessel or tube containing liquid or solid C. A. in an E. C. A. bath. Then press in air till at 50 atmospheres. After that shut the vessel and let it acquire common temperatures, and a pressure of 100 atmospheres might be obtained, which might be directed on to a part of the vessel separated by mercury, and containing the gas to be condensed, which at the same time could be cooled by the E. C. A. to 100° below 0° F.

7149. I lately saw a tree in Greenwich Park which has been struck by lightning. It presents a particular effect which also occurred in the tree at Old Buckingham, and another that I saw in Kensington Gardens. The trunk of the tree is scored by the lightning, an irregular line of an inch or two in width appearing where the bark is torn off, and sometimes, as at Old Buckingham, the wood also torn out. This line marks where a flash of intense force has passed, but on tracing it upwards, as soon as it gets above the lower branches, it diminishes and at last disappears, and it cannot be traced to any particular branch and so along it to the end. The effects shew that it is not the result of lightning which has gone as one flash undivided into a tree and striking one branch as it has struck the one stem, but that it is the result of the electricity gathered by the many branches and their innumerable terminations, which, when collected on the stem into one portion, appears there with a power of tearing that it had not above; and very probably it was no flash in the branches though it was a flash on the trunk. All this is in perfect harmony with the conditions of the parts, for the terminal twigs and leaves and outer branches are numerous, pointed, wetted all over thoroughly and therefore good gatherers and conductors; but the trunk would be single and above all much drier, and the surface with excess of moisture, i.e. its wetted surface, very small by comparison with that of the leaves. It is very probable that the discharge over the surface of the trunk is along some run of water from the upper branches, for that would account for the marked selection of a course and also the explosive power there rending and tearing.

7150. The flash therefore probably enters the tree rather as a brush than otherwise, and in these cases becomes a flash again within the tree and low down, as at the top of the trunk.

7151. The danger of standing under trees is well known and agrees with all these effects and observations.

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7152. Procured more Carbonic acid from Mr Addams and went to work on various gases. The two pressure pumps were in order and mounted (7085); also the air pump, etc. for the E. C. A. in vacuo (7085).

7153. *Hydrogen.*

Again pumped in hydrogen as before (7092), and attained to about 25 atmospheres in the cold of E. C. A. in Vacuo; but there were no signs of condenzation. The hydrogen leaked again with great facility (7092).

7154. *Fluo silicon.*

Fluo silicic gas was made into two mercurial receivers and then pumped in by the two pumps (7085, 7067). The pumps did very well for a time, and long enough to condense the gas, for it passed into the state of a very limpid liquid at the extreme cold of E. C. A. in Vacuo. It did not freeze.

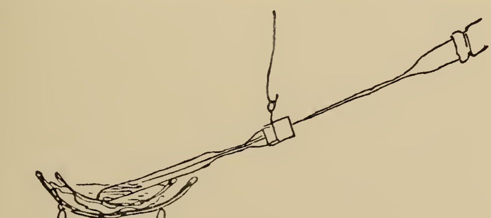
7155. The pressure at which it condensed was about 9 or 9·25 atmospheres at this low temperature. As more gas was thrown forward pressure rose to 17·6 atmospheres, but then the liquid in the condensing tube was as high and level with the surface of the E. C. A. bath, so that in fact the gas thrown in was not cooled to the temperature of the bath, and hence part of the increased pressure, if not all (7179).

7156. The tube was put aside to wait whilst other condensation[s] were carried on, and after about 2 hours looked at. Then all the liquid was gone and the guage indicated 33·3 atmospheres. Leakage had taken place as the temperature and the pressure rose and there were appearances of it on the metal, etc. (7179).

7157*. *Ammonia.*

Endeavoured to seal up Ammonia in a tube, thus. The gas was generated from quick lime and Mur. Ammonia; then passed through a tube cooled to 0° F.; and then conducted by a fine tube into a tube condenser, capped at its open extremity and drawn out narrow but strong a little below the cap. The closed end of the condenser tube was immersed in an E. C. A. bath in Air.

* [7157]



The Ammonia soon condensed into a liquid. The fine conveying tube was then withdrawn; a Stop cock screwed into the cap of the receiver; a piece of turmeric paper held against the opening of the cock and the receiver lifted out of the bath. As soon as the warmth of the air raised vapour enough to fill the receiver with ammonia, which was easily judged of by the effect on the turmeric paper, the cock was shut and *then* the receiver dipped in the E.C.A. bath again. The pressure was of course quickly reduced below 1 atmosphere, and a spirit lamp being carefully applied to the contracted part of the tube, the glass softened, collapsed, closed, and was thus drawn off and hermetically sealed, very. The glass tube was of course green bottle glass. The ammonia tube was then removed and set on one side in the common air.

7158. *Sulphuretted Hydrogen.*

Obtained from Sulphuret of Antimony and Muriatic acid, cooled, etc. as before; was also sealed up very well in the same way (7157).

7159. Endeavoured to seal up some *Sulphuretted hydrogen* in a longer tube with a mercurial condensing guage in it, so as to get the pressures. This was done in a similar manner (7157), but a little derangement of the guage took place in lowering the pressure within when sealing up the tube; so that the air under the mercury in the guage expanded so far as to bring the mercury to the top of the guage tube, and some of the air escaped. On allowing the pressure within to rise, the mercury entered the tube, took its place and worked well—but the volume of air beneath it at 1 atmosphere was of course not known (7173).

7160. *Arseniuretted Hydrogen.*

Obtained from alloy of equal weights of Arsenic and Zinc, acted on by a mixture of 1 vol. Oil of Vitriol and 4 vols. of water.

This gas being cooled and condensed was easily sealed up in a tube (7157).

7161. Another portion was also sealed up in a tube, with a guage containing mercury as its indicating fluid (7159, 7172). Both were sealed up very well.

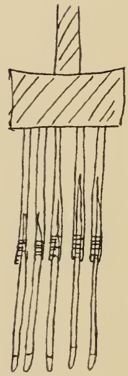
7162. *Hydrobromic acid.*

Per bromide of phosphorous was made by use of the double vessel before thought of (7059), but it is the per bromide that must be used. The proto bromide is a liquid and often appears when per bromide might be supposed to be there—more bromine added carefully to this makes it the yellow crystalline per bromide. No explosion; all went on well as to formation, etc.

7163. This per bromide, moistened and distilled, gave hydro bromic acid which, cooled first and then sent into a condensing tube (6979, 7058), at first refused to condense, probably from little air present increasing tension; but at last fairly condensed and was sealed up. It was kept for an hour or two but at last burst (7036).

7164. As to trying it with guage, I doubt whether the little portion of bromine carried over would not destroy the working of the mercury.

7165. Had now Chlorine, Ammonia, Sulphuretted Hydrogen, Arseniuretted hydrogen and Hydrochromic acid condensed in little tubes, and proceeded to try whether they would freeze in E. C. A. in Vacuo. Attached the tube to five arms, and these to a sliding wire going through the top plate of the air pump jar, and thus could easily introduce them into the E. C. A. in vacuo. In doing this, the hydro bromic tube burst, shewing that it had a high pressure at common temperatures. Of the four remaining, Chlorine and Arseniuretted Hydrogen would not freeze; the other two did. So



7166. *Chlorine*: not solidify at 160° below 0° (7089).

7167. *Arseniuretted hydrogen*: not solidify at 160° below 0°.

7168. *Ammonia*: freezes in E. C. A. in Vacuo and is then a white translucent crystalline body. When taken out of the bath and allow[ed] to melt, the solid part was always below and appeared to be heavier than the fluid. This was apparent also by the contraction of the ammonia and consequent depression of its surface as it froze. It was rather long in melting and when put into the bath again instantly froze, shewing that its freezing point was a good deal higher than the temperature of the bath. No attempts to freeze the Ammonia in an E. C. A. bath in air succeeded—not cold enough.

7169. *Sulphuretted hydrogen.*

This body acquired the solid state, but evidently was more difficult to freeze than Ammonia and melted more quickly. The solid was heavier than the liquid; crystalline; white and translucent. Melted and solidified it several times.

In these experiments the Barometer was 29·8 inches. The air pump barometer 28·7 inches. The E. C. A. bath good and abundance of solid C. A. in it the whole time. The temperature must have been quite as low as before (7089), or 160° below 0°.

7170. These four Specimens may be thus known:

Ammonia is in the longest tube.

Sul. Hydrogen is in the next longest tube.

Chlorine is in the shortest but one and is yellow.

Arseniuretted hydrogen is in the shortest tube.

7171. *Chlorine* becomes somewhat paler as it is cooled, but still keeps a fine clear yellow colour. It is not in that respect like the Nitrous acids, etc.

7172. *Arseniuretted hydrogen.*

Proceeded to obtain pressures with the Arseniuretted hydrogen tube (7161, 7101). It was now about 6 hours since the Gas was condensed and the guage tube is very dim and dark, as if a film of Arsenic were condensed on it. The varnish marks also have spread but the Indian ink marks remain. The mercury probably moves stifly but I must take it as it is. The tube was well sealed and the volume of air not altered. Its volume was 24 parts. The following pressures were obtained in the order given—the bath including the lower end and about $\frac{1}{4}$ of the whole tube.

0° F.	8·00 atmospheres
10°	8·72
30°	10·13
40°	10·90
50°	12·00
55°	13·11
60°	13·70
30°	10·66
40°	12·00
6°	8·42
10°	8·72
20°	9·23

The previous experiments (7100, 7101) gave

0° F.	6·2
32°	8·7

7173. *Sulphuretted hydrogen*—pressures.

As said (7159), this air guage has an unknown quantity of air. Made however experiments with the tube first at temperatures from 0° to 60°, in baths and Jars of ice, salt, water, etc. etc.; and after that with an E. C. A. bath in air—intending at last to ascertain the volume of air in the guage and so deduce the pressures. The following experiments are very consistent and are in the order of observation[1] (Guage 17 parts. 7177).

60° F.	1·45 *parts*	or 11·7 atmospheres
50°	1·62	10·5
45°	1·70	10
40°	1·87	9·09
35°	2·00	8·5
30°	2·10	8·09
√ 0°	3·16	5·38
10°	2·80	6·07
√20°	2·46	6·9
√30°	2·14	7·94
35°	2·05	8·3
√40°	1·83	9·3
45°	1·72	9·88
√50°	1·66	10·24
55°	1·55	10·97
√60°	1·45	11·7
65°	1·37	12·4
70°	1·33	12·8

7174. The next experiments were made in a very abundant bath of E. C. A. in air, in which the Alcohol thermometer (7088) and the tubes both of Sulphuretted hydrogen and arseniuretted hydrogen were dipped at their lower extremity so as well to cover much more than the part containing the fluid. The bath sank to 100° below 0°, and as it rose observations were made, the bath being continually stirred to keep it and its included matters at the same temperature. Only two observations were obtained with the

7175. *Arseniuretted hydrogen*—and these doubtful from the dark state of the tube

−96° F. (below 0°)	2 atmospheres
−79°	2·09

These of course could not be true, since at 96° below 0° this

[1] The ticks are in pencil in the MS.

substance is condensible at less than 1 atmosphere; but it may depend upon the evolution of hydrogen altering the pressure in the tube. Or from the sticking of the mercury the Air may have passed by it out of the guage. Of course the other numbers with Arseniuretted hydrogen are of no value. Yet they are so regular that if the present amount of air were known they would be good.

7176. *Sulphuretted hydrogen* in E. C. A. bath (see 7177 for guage 17 parts).

			Probably too little[1]
−98° (below 0° F.)	18·24 parts	0·932 atmospheres	1·33
−93°	17·33	0·98	1·40
−79°	13·33	1·27	1·77
−74°	12·43	1·37	1·81
−70°	11·33	1·5	1·94
−63°	9·33	1·82	2·35
−55°	7·83	2·17	2·68
−50°	7·10	2·41	2·97
−45°	6·22	2·73	3·37
−40°	5·33	3·2	3·8
−35°	5·00	3·4	4·05
−30°	4·33	3·93	4·7
−25°	3·90	4·36	5·0
−20°	3·63	4·68	5·4
−15°	3·23	5·26	6·0
−10°	3	5·66	6·32
− 5°	2·66	6·40	7·2
0°	2·53	6·72	7·5

7177. Now cooled the fluid end again in an E. C. A. bath, and when pressure reduced as much as could be, opened the end of the tube so as to let the gas escape and the guage take its place at a pressure of 1 atmosphere. The number of parts occupied by the air in it was found to be as nearly as possible 17. Hence may make a general estimate of the pressures and add it above.

7178. The mercury in this guage looked bright and kept well to the end—but probably green glass guages better than those with Glass containing both alkali and oxide of lead.

[1] In the manuscript the fourth column of figures and the words "Probably too little" (apparently referring to the third column) at its head are in pencil. See par. 7185.

7179. Examined the *Fluo Silicon* tube of yesterday (7154, 7156). Still pressure high, i.e. at about 30 atmospheres; temp. 60° F. On putting the bend into Ice and Salt at 0°, fluid condensed within, which rose again in vapour at common temperatures, leaving a little wetness and also a little roughness, as if some flocculent silica adhered to the inside of the tube. On cooling it to 32°, some liquid condensed also, but more at 0°. The liquid was very limpid and etherial—easily boiled by warmth of finger. The guage appeared scarcely to move for these changes, but as the mercury was at 0·75 parts in a guage of 22 parts, perhaps only little motion could be expected. Must compare this with the former data. Remember Cagniard de la Tour's expts.

7180. The escape of the liquid from the tube had been effected by the plug of the cock and out at the plug end (7156), not along or by the plug through the cock. This cock was not screwed up at the time (7122). There a little lump of hard silica, etc. was formed. The cement had not suffered in the least.

7181. *Arseniuretted hydrogen.*

This tube is as dirty on the green glass as on the white flint glass of the guage—and green glass guage probably do no good here. The deposit is probably solid hydruret of Arsenic, and some of it is rolling about in adhering flocculi in the clear fluid. The mercury seems clean but the inside of the mercury guage black and dirty.

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7182. *Arseniuretted hydrogen* (7101, 7161, 7172, 7175).

The dirtiness of this tube (7181) must interfere with the motion of the mercury and hence its pressure doubtful. The pressures on 2 different occasions were

0° F.	. 6·2 atmospheres	and	0° F.	. 8·0 atmosph.
32°	. . 8·7 ,,		30°	. . 10·13 ,,

7183. Assuming for the moment that the average numbers of (7172) are as follows and nearly correct:

0° F.	8·0 atmospheres
6	8·42
10	8·72 twice
20	9·23
30	10·40
40	11·45
50	12·00
55	13·11
60	13·70

then the following may be drawn up as a series with interpolations of which many of the terms will accord well with the experiments; but they leave the former two low results out altogether[1]:

√ 0° F.	8·0 atmospheres
5°	8·34
√10°	8·70
15°	9·10
√20°	9·42
25°	9·86
30°	10·32
35°	10·80
√40°	11·32
45°	11·86
50°	12·42
√55°	13·01
√60°	13·73

7184. The observations with *Arsen. hy.* at exceedingly low temperatures (7175) must be wrong, because it has not a pressure of 1 atmosphere in the E. C. A. bath in air. Perhaps hydrogen is gradually set at liberty and Arsenic deposited.

7185. For *sulphuretted hydrogen* (7173, 7176):

see the curves as laid down on paper. Probably a change of the air in the guage by passing the mercury.

[1] The ticks are in pencil in the MS.

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Barometer at 29·47 at ½ p. 7 o'clk. A.M.

7186. Worked with the two condensing pumps and the apparatus for the E. C. A. in vacuo (7085), so as to apply the cold of the latter if needed.

7187. *Nitrous oxide.*

Used the syphon shaped tube (7085) and the E. C. A. in air; the air guage was one with 30 parts of air. Gas easily condensed at 6 or 7 atmospheres, but on continuing to pump, soon reached pressure of 14 or 15 atmospheres; but then the liquid in the tube had increased until it stood above the level of the E. C. A. bath and consequently was of a higher temperature and tension there. This was without the action of the exhausting air pump.

7188. Then exhausted to see if we could freeze the Ns. oxide, but at this time the cement of the cap gave way a little so as to cause a leak. Went on exhausting; and two things happened. First the Ns. oxide froze and became a *beautiful clear crystalline solid*; which, on lifting it up above the bath, liquefied again; on recooling it, it froze again, and this was repeated several times.

7189. The next point was the discharge of the mercury entirely out of the Guage, to effect which the pressure within the condensing tube must have been less than $\frac{8}{10}$ of an atmosphere—how much less I cannot say. Hence Ns. oxide at the temp. of E. C. A. in Vacuo has less pressure than one atmosphere. Also solid Nitrous oxide has a less pressure than one atmosphere.

7190. Used another condensing tube and guage. Guage had an air capacity of 22 parts. Condensation of the Nitrous oxide easily went on at pressure of 6 or 7 atmospheres in E. C. A. bath in *air*. Then let off a part of the liquid in vapour to clear out all but vapour of condensed Nitrous oxide, and finally closed up the tube by a cover to the cock (7115) and screwing up the cock plug screw (7122).

7191. With this tube, its place of condensation being in an E. C. A. bath in air, the pressure was 4·4 atmospheres. On adding a little more ether to the bath (which warmed it a little), the pressure rose to 4·9 atmospheres nearly; but the addition of plenty of solid C. Acid made it again 4·4 atmospheres, which must be nearly true. On moving the tube about in the bath the pressure diminished to 3·7 atmospheres, because of the evaporation of the C. A. cooling the bath below its boiling point.

7192. Had a case before like this—that is, of carbonic acid (6995) which, inside a tube, was reduced below the pressure of one atmosphere by the cooling of the E. C. A. bath in air at pressure of one atmosphere.

7193. In a good bath of ice and salt, the pressure was 22 atmospheres and there was liquid left. In a bath of powdd. ice and water the pressure rose to 31 atmospheres, but there was no liquid left. When put into ice and salt, plenty of liquid came down, and from the appearance I should judge that at 32 it was not far from Cagniard de la Tour's state. Perhaps about 40° F. it might come on.

7194. Being put into an excellent and fresh bath of salt and ice, the guage indicated a pressure of 20 atmospheres just, and this I think is nearer than the former; for I obtained the result again and again. So the pressure at 0° F. may be taken as 20 atmospheres.

7195. As to Cagniard de la Tour's state, if that came on at a temperature of 40° or 50° and with a pressure perhaps of 40 or 50 atmospheres, then there is no reason why a pressure of 80 or 100 atmospheres should not halve the bulk of the vapour; and if this were so, then the bulk would probably be less than the original bulk of the liquid. In that case, the strange result would occur or might occur that a certain pressure and heat would make the body smaller than the same pressure and cold.

7196. What considerations of physical constitution would this lead to?

7197. *Olefiant gas.*

This gas has stood over lime and water for two days. The tube was like the last. The guage had 30 parts of air. The gas easily condensed in an E. C. A. bath *in Air*. The first pressure was 10·7 atmospheres, but on letting out vapour, etc. until $\frac{1}{3}$ of the liquid was gone and then closing all well up, the pressure was 5 atmospheres nearly in a good E. C. A. bath in air. This I consider a good result.

7198. Allowing this liquid to evaporate, I made a second condensation: had as before a higher pressure at first, but on letting out some vapour and closing up, the rest then gave, as before, a pressure of 6 atmospheres nearly in the E. C. A. bath *in air*.

7199. On putting this tube into a bath of ice and salt, all the liquid disappeared; hence could not get the pressure. In the E. C. A. bath the liquid condensed again beautifully. The Cagniard de la Tour temperature is probably less for this fluid than for Nitrous oxide.

7200. On submitting this fluid to the low temperature of the E. C. A. bath *in Vacuo*, it would not freeze; but whether it was not cold enough or wanted touching (7210), I cannot say.

7201. On setting this tube aside, the fluid disappeared, i.e. reimmersion in ice and salt or in E. C. A. would not bring any back; but the guage shewed a high pressure long after. Olefiant vapour or liquid is most probably condensed by the cement of the caps and permeates it. The chemical nature of the two bodies would indicate that, at the same pressure and temperature, the gas would condense not on the clean glass, but on the wax and resin.

7202. When the tube was opened, the air guage accurately returned to its place again, i.e. to 30 parts.

7203. *Muriatic acid gas*. Bar. now 29·43 at ½ past 10 o'clk.

Used a similar tube to the former; the air guage had 24 parts. The Gas was generated from Mur. Ammonia and sulphuric acid in a little retort connected by a caoutchouc tube with the condensing tube. When the gas flowed freely through the whole apparatus, the plug (   ) at the further end of the condensing tube was closed and then the tube arranged at the air pump so that it could dip into the E. C. A. bath prepared for it. There was no condensation at first, i.e. whilst pressure of air was on the E. C. A. bath, but on proceeding to remove that pressure, and when the air pump barometer had risen to 22 inches (7217), suddenly the Muriatic acid fumes issuing from the generating retort disappeared and the liquid was condensed in the tube. At that moment the M. A. G. condensed at a pressure of one atmosphere or a little more for the [illegible] of the caoutchouc joint tube.

7204. The air pump was then worked and the E. C. A. bath in it made as cold as possible, but the *M. A. did not freeze* (7210).

7205. The liquid was then secured, after some vapour had been allowed to escape. Its pressure in the E. C. A. bath in air was 2·285 atmospheres; in a second experiment after some time, it was 2·18 atmospheres (see 7285).

7206. In ice and salt baths the pressure, from many observations, was 17·2 atmospheres. In ice, as well as I could judge by this guage, 30 atmospheres.

0°	. . .	17·2 atmospheres
32°	. . .	30·0 ,,

Carbonic acid.

7207. Tube as the former. Guage of 30 parts. Easily condensed by pumps in an E. C. A. in air and of course froze at that temperature. When in order, it gave the following pressures. In an excellent E. C. A. bath *in air* the pressure was 2·5 atmospheres. Hence a satisfactory proof that the Carbonic acid in an E. C. A. bath is held by a solvent power or by affinity, for whilst at a certain temperature it has an elastic force of one atmosphere, the same Carbonic acid out of the ether has an elastic force of 2·5 atmospheres.

7208. In salt and ice, or at 0° (and a good bath), the pressure was 22·2 atmospheres. In ice and water pounded, etc., the pressure rose to 30 atmospheres, but at the same time the tube began to leak and I could not verify or complete the observation.

7209. Prepared a little receiver tube with a cap, and a screw plug to close the cap when required. Placed the end of the receiver tube in an E. C. A. bath and then sent in *Euchlorine*, prepared from Chlorate of potassa and sulphuric acid, by a small leading tube. The gas instantly condensed, first into an orange red liquid, and then this crystallized into a red mass having the colour and appearance of a little bichromate of potassa. The crystals were hard, brittle, translucent, clear, etc., and in this small tube, even when looking along it, there is no sensible appearance of colour as if much or any vapour rose from them. The tension must be small at this temperature. The tube was then closed up by the screw and thus the substance retained in the tube. As the tube became warm in the air, the crystals melted and formed an orange red fluid, and the yellow colour of the vapour above appeared.

7210. Some hours after, wishing to freeze this fluid again, I could not succeed in an excellent E. C. A. bath. The colour of the vapour disappeared and the fluid increased in quantity, but no cold would make it crystallize; not 110° below 0°. I shook it much, but it remained fluid. I opened the vessel; it still remained fluid; but when I touched it with a platina wire, it instantly solidified and became crystalline as before.

7211. Hence may not be sure that the bodies which did not freeze would not freeze if touched in a similar manner. Like acetic acid, phosphorus, etc. etc.

7212. Being solid, it was kept with a thermometer (7088) in an E. C. A. bath of which the temperature was allowed gradually to rise in the air. It liquefied at −75° F. or 75° below Zero. When partly melted the solid portion was heavier than the liquid.

Hydrobromic acid.

7213. Prepared some tubes (7157) and succeeded in sealing up two portions of hydrobromic acid in the liquid state. The liquid was very limpid and of a red colour, I believe from a little bromine which passed over; but no signs of this appeared in its vapour.

7214. This acid did not freeze in E. C. A. in Air, but did in E. C. A. in vacuo. The bath with the tube and the thermometer (7212) in it was allowed to rise in temperature, and at −124° F., or 124° below zero, the solid hydrobromic acid melted.

7215. *Sulphuretted hydrogen.*

Was frozen in a similar way and its melting point also ascertained. It assumed the liquid state at 122° below 0°.

7216. *Ammonia.*

Was also examined in a similar manner. It melted at 103° below 0° F. The solid Ammonia is heavier than the liquid and sank in it.

7217. *E. C. A. bath in Vacuo.*

I put a good E. C. A. bath with plenty of Carbonic acid in it and also a thermometer into an air pump receiver and exhausted, observing the barometer of the air pump and the temperature of the bath as the exhaustion proceeded. The following was the result[1].

Air pump bar. at	inches		Air pump bar. at	inches	
0	−103°	F. was the bath temperature	15	−115°	F. was the bath temperature
√1	−106°		16	−116°	
2	−108°		17	−117°	
3	−108°		18	−118°	
4	−109°		19	−119°	
5	−109°		√20	−121°	
6	−110°		21	−123°	
7	−111°		√22	−125°	
8	−111°		23	−	
9	−112°		√24	−131°	
√10	−112½°		25	−	
11	−113½°		√26	−139°	
12	−114°		√27	−146°	
13	−114½°		√28	−160°	
14	−115°		√28·2	−166°	

7TH NOVR. 1844.

7218. Examined the *Mur. Acid* condensing tube of Novr. 4 (7203). No liquid at common temperatures, nor in bath of 32°, but in bath of Ice and salt came down—yet smaller than formerly, as if a little leak or action.

7219. Now it had also acted on the Brunswick black of the graduation and made it run down, and the liquid acid was a little brownish from its having dissolved at the moment of condensation a portion of the same. Hence it is probable that the diminution is dependant on the absorption of part of the acid by the cement of the caps; for there is no appearance as yet of leakage on the outside.

7220. *Nitrous oxide.*

Examined the Nitrous oxide condensing tube of the 4th instant (7190). No liquid at common temperatures or at 32°, but plenty in bath at 0°—as much apparently as before; neither was there any action on or running down of the Brunswick black marks of graduation. This body does not dissolve in Nitrous oxide.

7221. *Hydrobromic acid.*

My specimens of hydrobromic acid are red from a little vapour of bromine. Placed the tube horizontal and cooled the narrow

[1] The ticks are in pencil in the MS.

part so as to condense and distil into it a little of the hydrobromic acid. It is quite *colourless*. Hence *Hydrobromic* acid, like *Muriatic* and *hydriodic* acids, *is colourless*.

8 NOVR. 1844.

7222. Cagniard de la Tour's expt.

The general effect considered—must be a general law.

In a long tube, and not heating the bottom, easily obtain progression from cold fluid to perfect vapour.

7223. But then how can the transition consistent with the law of continuity take place in such cases as Mercury, Zinc, Potassium, etc., which as liquids are opaque metals, as vapours transparent and sometimes colourless?

7224. As to hydrogen, probably the lowest temperature I can obtain may be *above* the temperature of the point of that conversion. If hydrogen a metal might else tell something.

7225. Perhaps also Nitrogen and oxygen have the same points, so low as to be below lowest attainable temperature.

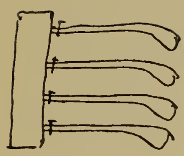
7226. Might probably get this state with Nitrous oxide or other gases if common temperature is high enough for it, by putting on pressure to a tube with its lower end in an E.C.A. bath. The proof would be obtained if the line of demarkation between the liquid and the gas were to disappear, and the transition from the vapour to the cold fluid below were gradually. This would represent the ether tube heated at the top of the liquid only, not at the bottom.

7227. As gas or vapour appears to be the simplest form of matter, for then volumes of equivalents are in simple ratios, so the law which binds them together in this state into one series ought to rule, either simply or in conjunction with some other law, in the Cagniard de la Tour state; and through it in the liquid and solid state also, supposing there be any case in which a solid can pass into the Cagniard de la Tour's state.

7228. As to the pressures of different bodies when at the C. de la Tour point, it probably does not depend so much on the volatility of a body as on the great volume of vapour it can form—hence pressure for water very high—for Ether, low by comparison. In that case, the liquids of gases giving *densest* vapours may be expected to shew it most easily.

7229. In obtaining high pressure.

Perhaps may compress Hydrogen, Nitrogen and oxygen by Mr Addams' pump of liquids, or by pumping in Mercury into a wrought iron barrel previously filled with hydrogen and connected with a glass tube cooled in an E. C. A. bath in Vacuo.



7230. Suppose two or three or more reservoirs of the liquids of condensed gases, as Nitrous oxide, Carbonic acid, Muriatic acid, Olefiant gas, etc. were connected with one common vessel. If one were opened, the pressure of its vapour would be diffuzed through the vessel; if a second were opened whose vapour had no power of dissolving in the first liquid, its force would be added to the first and a very high pressure be obtained within; and so on for the others. The probability is that they would dissolve each other more or less; thus, Mur. acid would probably dissolve carbonic acid and the reverse.

7231. Query what would happen with Strong solution of ammonia under restraint and heated.

7232. Would strong solution of Ammonia in a Cagniard de la Tour tube give me a gradual progression from top to bottom? It would probably shew the state as regarded the ammonia.

9TH NOVR. 1844.

7233. The results as to pressure are very unsteady. Those given by E. C. A. in air may well be so because that is an unsteady and varying temperature, unless the bath be covered over so as to have pressure of *C. A. only* on it. Must take this precaution. Will enter them together here so as to have easy reference to them.

7234. *Olefiant Gas* (7299).

	atmospheres	
In E. C. A. bath in air	3·7	(6989)
,, ,, ,,	8·7	(7049)
,, ,, ,,	5·0 }	(7197–7202)
,, ,, ,,	6·0 } . . .	

7235. *Nitrous oxide.*

In E. C. A. bath in vacuo	0·8 or less .	(7189)
,, ,, in air	4·4	(7191)
,, ,, ,,	1·66 . . .	(7004–9)
,, ,, ,,	19	(7243)
0° F.	{20 {22	(7193)
32° F.	31 and more .	(7193)

7236. *Muriatic acid*[1].

√	In E. C. A. bath at −125° F.	1·0 about . .	(7203)
√	E. C. A. bath in air	1·6	(7054)
	,, ,, ,,	1·33 . . .	(7054)
	,, ,, ,,	2·218[2] . . .	(7205)
	,, ,, ,,	2·285 . . .	(7205)
	0° F.	12	(7055)
√	,, ...	{15·2 . . . . . {17·2	(7244)} (7206)}
√	32°	{30 . . . . . {28·2	(7206)} (7244)}

7237. *Carbonic acid* (7300).

In E. C. A. bath in air	2·5	(7207)
0° F.	22·2	(7208)
32°	30 at least . .	(7208)
32°	36—old expts.	

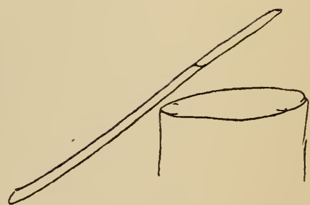
7238. Solid carbonic acid gives a pressure of 6 atmospheres (6996).

11 NOVR. 1844.

7239. Any gas which is likely to shew Cagniard de la Tour's state by E. C. A. bath and common temperature must be a gas that cannot be condensed at common temperatures (7226). Must look therefore at Nitrous oxide or Olefiant gas.

7240. At somewhat higher temperature perhaps Muriatic acid, Carbonic acid, Hydriodic acid (heavy).

7241. By having a long tube, three fourths filled with fluid, and heating only the top part, might easily have the progression from cold liquid to vapour, with Ether or any other fluid that can be used at all.



[1] The ticks are in pencil in the MS. [2] Par. 7205 gives 2·18.

7242. The following are some solids and liquids with the given Specific Gravities of their vapours. The heaviest are of course the most promising for Cagniard de la Tour's results[1].

	S.G.	
Hydrogen	1	
√Ammonia	8·5	
Water	9	
Nitrogen	14	
√Olefiant	14	
Oxygen	16	
√Sul. hydrogen	17	
√Mur. acid	18·5	
√Carbonic acid	22	
√Nitrous oxide	22	
Alcohol	23	. . . . *119 atmospheres, 405° F.*
√Cyanogen	26	
Caoutchoucine	28?	
Etherine	28	
√Sulphurous acid	32	
√Chlorine	36	
Ether	37	. . *pressure 38 atmospheres, 320° F.*
Sulphuret carbon	38	
Bi-carb. hydrogen (solid at 28°)	39	
√Hydrobromic acid	39·5	
√Arsend. hydrogen	41	
√Euchlorine	44	
Nitrous acid	46	
Hydro chlo. carbon	50	
Phosgene gas	50	
√Fluo silicon	52	
√Hydriodic acid	63·5	
Naphthaline	64	
Camphine	68	
Camphor	76	
Bromine	78	
Proto. chlo. carbon	84	
Proto. chlo. Arsenic	92?	
Sesqui chlo. carbon	120	
Iodine	126	

Ether at C. de la Tour's point	4275
Ether at common temp.	8550
Water at common temp.	11810.

[1] The ticks and the words in italics are in pencil in the MS.

7243. Reviewed the *Nitrous oxide* tube of the 4th instant (7190, 7220), now 14 days old. It was just as on the 7th instant (7220), perfectly clear and clean, and neither the bitumen of the guage graduation nor the cement touched. There was no fluid—nor at 32°; but at 0° it came down well, and the pressure was then such that the mercury stood exactly at 1 part. The guage was perfectly clean, and also the mercury, and on letting out the gas and reducing the pressure to 1 atmosphere, the mercury returned to 22 parts very nearly—21·4 perhaps. As the air at the greatest compression was cooled to 0° nearly, for the end of the guage was in the cooled part, I now cooled the whole of the guage to 0° and found the bulk decreased to 19 parts nearly; so that, in fact, the pressure of Nitrous oxide would by this good and constant experiment appear to be about 19 atmospheres at 0° F.; and I think this is not far from the truth.

7244. Reviewed the *Mur. acid tube* of 4th Novr. (7203, 7218). The Mercury was at high pressure, but there was no liquid and the bitumen of the graduation had softened and run down. Neither was there any liquid produced at 32° or at 0°. I let out the gaseous acid and took the tube to pieces: I found the cement had been much softened and affected; blackened and injured in part within. As the pressure went down the soft black bitumen, etc. boiled and frothed up, and became afterwards brittle. So liquid Mur. acid dissolves it. On taking out the guage the mercury returned to 25 parts; this probably due to M.A. vapour leaving the surface of the glass after the mercury had returned over it, as the pressure was removed. Assuming that the 24 parts of air originally in would have become 21·3 parts at 0°, then the pressures at 7206 ought to have a correction as follows:

0°, instead of 17·2 atmospheres should be about 15·2 atmospheres
32°, instead of 30 atmospheres „ „ 28·2 „

23 NOVR. 1844.

7245. *Air poles.* Matteucci says they cannot repeat my experiments on air poles, etc., 460 of Experimental Researches, etc. Tried to-day with the Electrical machine the experiment, Par. 462,


of a piece of turmeric paper. Easily evolved Alkali at the points and so verified my own results.

7246. Worked to-day with *Sulphurous acid* to obtain correct pressures. Used a horizontal tube and Guage and the close end of the guage was furthest from the cold. Distilled condensed sulphurous acid into it, secured all well and obtained the following pressures, which I think are good—at all events up to 70° F. The following is also the order in which they were obtained, so that the results both in rising and falling might [illegible]. Guage of 22 parts.

32° F.	1·52 atmospheres
23	1·222
19	1·1
14	1·0
26	1·33
32	1·5
49	2
56	2·444
65	2·75
73·5	3·143
80	3·44—condensing elsewhere in tube, so too little.
64	2·857
60	2·68
55	2·54
48	2·2
33	1·57
32	1·51
40	1·88
23	1·222
14	1·0
32	1·5

Bunsen's numbers converted differ very much. Bib. Univ., 1839, xxiii, 185.

14° F.	1·02 atmosph.
23	1·45
32	1·94
41	2·5
50	3·13
59	3·84
68	4·64
77	5·5

I have no doubt these are better than those of 7147, except perhaps the highest.

25 NOVR. 1843[1].

7247. *Cyanogen.*


Worked with it and a horizontal tube to get pressures, and had closed end of the guage away from the bath. The barometer was at 30·1 inches. The guage was clean and good, and had 22

[1] Presumably 1844 is intended.

parts. After the experiments were finished the tube was opened and gas let off, when the guage returned accurately to the 22 parts; nor was there any division or sticking of the mercury. When the cyanogen was let off, it would *not freeze itself* when so cold as to have vapour of only 1 atmosphere of tension.

7248. Cyanogen continued. The observations were taken in the order set down[1].

0° F.	1·158 atmospheres
10	1·538
√20	1·885
30	2·291
√32	2·365
46·5	3·142
√52	3·353
√63	3·928
70	4·400
√79	5·000
√74	4·731
√69·6	4·400
√64	4·000
55	3·666
√48	3·260
√44	3·013
√50	3·343
√44	3·034
√38·5	2·690
√32	2·391
√27	2·200
√20	1·903
√11·5	1·571
0	1·158

Bunsen's numbers are as follows nearly.
Bib. Univ., 1839, xxiii, 185.

−4° F.	1·05 atmosph. 30 inches
5	1·44
14	1·85
23	2·27
32	2·72
41	3·2
50	3·78
59	4·37
68	4·986

I believe these numbers are very good except the higher degrees.

7249. *Ammonia.*

Worked with it and a new tube and guage. Barometer as before. Guage had 30 parts. After having ascended to warmer temperatures, and then descending to cooler, the mercury divided a little, as if ammonia adhered to inner surface of glass tube, and then as pressure diminished expanded into vapour; it was not much. On returning to still lower temperatures, expansion of the

[1] The ticks are in pencil in the MS.

air on the inclosed side of the guage was to be expected from the same cause, and in fact, on letting down the pressure at last, the mercury returned to 34 parts instead of 30 parts; and probably a correction, uncertain in amount, ought to be applied to the four last observations, for they are subject more or less to this effect. Hence the first observations are the best. When the *ammonia* was let off, it did not freeze itself—hence not cold enough for that at pressure of one atmosphere.

0° F.	1·935 atmospheres		Bunsen's numbers, Bib. Univ., 1839, xxiii, 185.	
10	2·727			
20	3·333		23° F.	3·99 atmosph. of 30 in.
32	4·285	rather doubtful	32	4·74
44	5·357	Do.	41	5·22
49	6·000		50	6·53
56	6·593		59	7·58
41	5·263		68	8·75
33	4·615			
44	5·263			
52	6·000			
61	6·976			
67	7·500			
65	7·500			
55	6·383			
51	6·000			
40	4·838			
18	3·000			

26 NOVR. 1844.

7250. From the best consideration that I can give, the following are good numbers for the bodies beneath. They accord closely with the best experiments, and though they differ much in some cases from Bunsen's, I put trust in them at present.

Sulphurous acid

	Atmosph. 30 inches
0° F.	0·725
5	0·79
10	0·92
√14	1
15	1·03
√19	1·12
20	1·16
√23	1·225
25	1·3
√26	1·33
30	1·45
31·5	1·5
√32	1·53
√33	1·57
35	1·61
40	1·78
45	1·94
46·5	2
√48	2·06
50	2·14
55	2·36
√56	2·42
58	2·5
60	2·6
√64	2·76
65	2·83
68	3
70	3·1
√73·5	3·28
75	3·4
76·8	3·5
80	3·7
85	4
√90	4·35
93	4·5
95	4·7
98	5
√100	5·16
104	5·5
105	5·56
110	6

7246
(7106, 38, 41, 47)

Cyanogen

0° F.	1·25
5	1·38
8·5	1·5
√10	1·53
11·5	1·6
15	1·72
√20	1·89
22·8	2
25	2·09
√27	2·2
30	2·3
√32	2·37
34·5	2·5
35	2·54
√38·5	2·72
40	2·78
44	2·97
44·5	3
45	3·03
√48	3·17
√50	3·28
√52	3·36
54·3	3·5
55	3·53
60	3·84
√63	4
64	4·07
65	4·15
√70	4·5
√74	4·79
75	4·87
77	5
√79	5·16
80	5·25
83	5·5
85	5·7
88·3	6
90	6·17
√93·5	6·5
√95	6·64
98·4	7
100	7·2
√103	7·5
105	7·72

7248
(7038, 108, 37, 42)

Ammonia

√ 0° F.	2·48
0·5	2·5
5	2·75
√ 9·3	3
10	3·05
15	3·33
√18	3·5
20	3·65
√21	3·72
25	3·96
25·8	4
√26	4·04
30	4·3
√32	4·44
√33	4·5
35	4·66
39·5	5
40	5·04
√41	5·1
√44	5·36
√45	5·45
45·8	5·5
√49	5·83
50	5·93
√51·4	6
√52	6·1
√55	6·38
√56·5	6·5
√60	6·9
√61·3	7
65	7·42
√65·6	7·5
√67	7·63
69·4	8
70	8·1
73	8·5
75	8·78
76·8	9
80	9·5
√83	10
85	10·3

(7031, 121)

7251. *Phosphuretted Hydrogen.*

Made it abundantly by sol. Caustic potassa and Phosphorus. The whole of the Phosphorus disappeared, and to the last bubble the gas was spontaneously inflammable and apparently very good. Had 14 or 15 vessels full, and of these 11 were jars, rather large. Easy to get this kind of gas abundantly.

7252. Tried it in the pumps, passing it in after carbonic acid. Gave a pressure of 20 atmospheres at a temperature of 0°, but it would not condense.

7253. In this case, dried the gas by passing it through a tube (convoluted) in ice and water. This left much water in, that was brought down in part of tube at 0°. Must dry at 0°, and perhaps better to put in some dryer as well.

7254. *Carbonic acid.* Would not go down at pressure of 20, and by jerks of 30 atmospheres, at a temperature of 0°; so could not condense it this way.

27 DECR. 1844.

7255. Have been to day to Mr Addams to try strength of some of the glass tubes and joints, etc. This was done by screwing them on to a pipe connected with a water force pump of which the piston was solid; and the arm of this pump was long and divided and could have weights hung on to it. It served the purpose of a valve. The tubes were in all cases filled with water. The piston had a diameter of $\frac{53}{100}$ of an inch; its area therefore was 0·2206 of a square inch.

The length of the lever from the fulcrum to the piston was 4 inches, and from the fulcrum to the suspension of the weight pan 40; that is, the power of the lever was 10 to 1.

The weight of the lever, piston etc. was 7 lb. 6 oz., and its center of gravity 17 inches from the fulcrum, so that this weight was acting with a leverage of 17 inches compared to 4 inches on the piston = 31 lb. 5 oz. directly on the piston, or 142 lbs. on a square inch = to 9·5 atmospheres of 15 lbs. each.

7256. So without any other weight than the handle there is always on the tubes connected with it a pressure of 9·5 atmospheres.

7257. As to the added weights, 1 lb. added = 10 lb. on the piston, and this 10 lb., divided by 0·2206 or the area of the piston, gives 45·33 lbs. as the pressure upon a square inch, which is equal to 3·022 atmospheres of 15 lbs. each. For each pound therefore sustained there, a pressure of 3·022 atmospheres may be counted.

7258. I have taken no account of the friction of the piston in the leathers, but it cannot be a serious portion, for the piston worked freely and water leaked by it at the higher pressures.

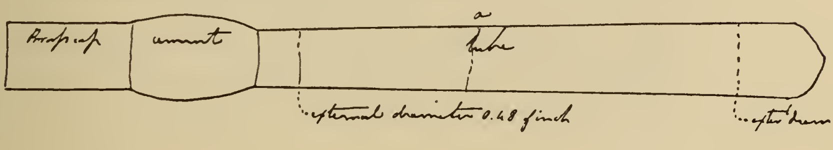
7259*. This tube bore pressures gradually increased up to $6\frac{1}{2}$ lb. suspended on the end of the lever. This is equal to 19·643 atmosphere, which with the addition of 9·5 atmospheres for weight of lever, gives 25·143 atmospheres as the bursting force. The part between the line *a* and the cement and cap was not broken. The part beyond was producing only fine pieces. The glass was not quite equal in thickness all round, but its average thickness at *a* was only 0·035 of an inch.

Hence such a generating vessel might have been used up to 10 or 15 atmospheres or even 20 upon a case of emergency.

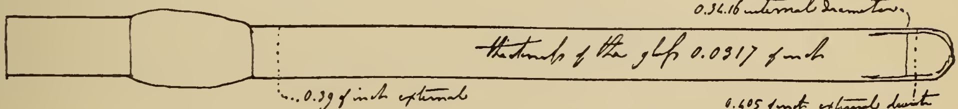
7260†. This tube did not burst with 12 lbs. on the lever, but was then taken down and preserved; nor did the cement in any part give way, either here or above. The tube has sustained a pressure of 45·764 atmospheres.

7261‡. This tube bore increasing pressure until there was 11 lb.

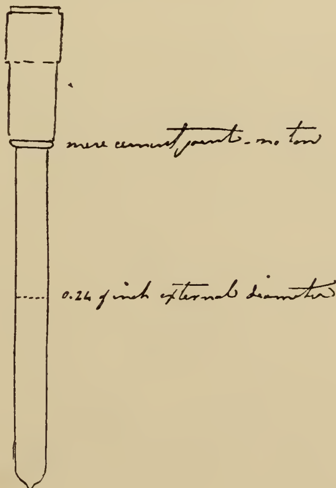
* [7259]



† [7260]



‡ [7261]



on the lever and then considerable leakage appeared; but on examining the tube, I found it was the plug of the small stop cock; and when that was tightened, then the leakage disappeared. Increased the weight until it was 19 lb., at which the tube burst. As before, about $\frac{2}{5}$ of it on the cap side remained sound; the other part towards the end broke into only four pieces. The cement was perfectly sound from first to last. The thickness of the glass was in this tube from 0·02 to 0·0175 of an inch—not more. The pressure nearly 67 atmospheres.

7262*. The last tube proved was one which had been used in the condensation of Olefiant gas, etc., and was in the state in which it was left from the experiments many weeks ago. The glass was thicker than in the one above. The weight was increased up to 30 lbs.; and then even up to 36 lbs.; still nothing gave way, except that all the cocks leaked at the plugs. I then took it off the apparatus and preserved it as a specimen.

The pressure which this tube has borne is equal to 118·3 atmospheres.

7263. The diameter, internal, of the tube may be assumed at about 0·18 of inch; its length is 10 inches. This gives the internal pressure as equal to 10035 lbs. when the above pressure was on it.

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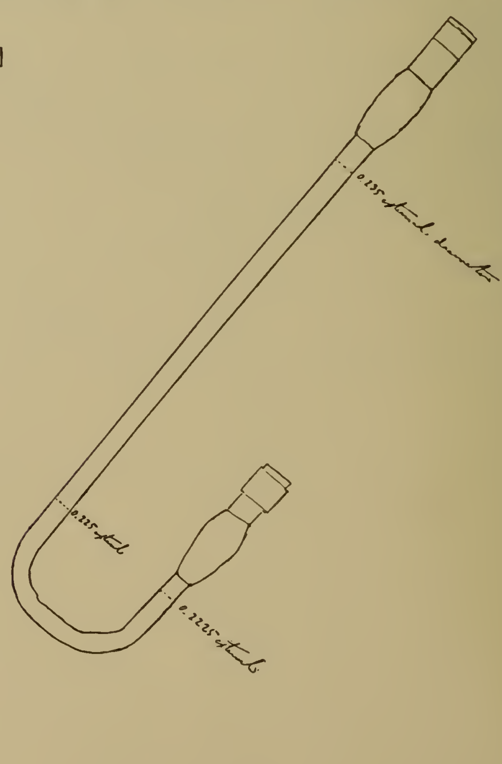
7264. *Carbonic acid.* A green glass tube, closed at one end and capped at the other, had some pieces of solid C. A. put into it, and then the cap was closed by a blank screw. On letting the

* [7262]



temperature rise, the solid C.A. melted and the fluid C.A. was retained well and conveniently in the tube.

7265. On putting the end of the tube into an E.C.A. bath, the liquid C.A. could be frozen at pleasure, and as easily melted again. When both solid and liquid C.A. were in the tube, the solid sank in the fluid, being *heaviest*. The melting point of Solid C.A. was very nearly 70° below 0° F. The fluid froze very easily at −80° F. I could not freeze or keep solid at any temperature higher than −70° F.

7266. *Hydriodic acid*. Was prepared and sealed up in a tube (   ). When Frozen and then liquefied, the solid acid seemed to sink in the fluid, being *heavier*. The melting temperature of the solid acid was at 55° or 60° below 0° F.

7267. *Cyanogen*. This gas or liquid freezes at about 30° below 0° nearly. The solid liquefies at that temperature. The pressure is at that time below 1 atmosphere; also for this reason liquid Cyanogen opened to the air does not by evaporation freeze itself. It was doubtful whether the solid or liquid body was heaviest, but probably the former.

7268. *Sulphurous acid*. This substance does not solidify at −60°, or in best E.C.A. bath in air; but did become solid in a bath cooled in vacuo. The solid acid is a white crystalline body, and when partly liquefied the solid sinks freely in the liquid, being heavier. It is liquid at −100° and will not then freeze; it is solid at −115° F. By repeated trials the liquefying point came out between −100° and −105° F.

7269. *Nitrous acid*.

A portion of pale yellow liquid nitrous acid was frozen in the E.C.A. bath. It became paler and of a greenish colour, appearing as a greenish yellow crystalline solid. When melting, the solid was heavier than the liquid. Its freezing point was found to be rather high, in fact near 0°; and I found that I could freeze it in a bath of Ice and salt.

7270. Some *green acid* (green from the presence of a little water in it) was cooled; it requires a much lower temperature to freeze it than the former. Its colour diminishes and the whole freezes into a pale bluish body, but there is then evidently two substances, probably dry acid, which has frozen out first, and then a hydrated acid which freezes at a lower temperature. At −30°, part was liquid and part solid; hence it appears that the true hydrate requires at least 30° below 0° to solidify it.

7271. *Ammonia.*

When liquid Ammonia was in an E.C.A. bath in air and air drawn up over it into the nostrils, there was abundant indication of ammonia vapour by the smell. Even if solid, it would most probably have a strong smell, just as the Carbonate has.

7272. *Sulphuret of Carbon* and liquid C.A. put together in the air do not mix well. The C.Acid does not wet with the liquid as it does with Ether. But the latter is made very cold and will not burn or freeze (7374, 7419).

7273. Put a glass containing some *dry solid C.A.* under the receiver of the air pump, to see how far the exhaustion could then be carried. Brought the barometer of the pump up to 29 inches, the Barometer outside being at 30 inches.

7274. *Carbonic acid.*

Sent C.A. from Syringe pumps into a glass tube cooled in an E.C.A. bath in air. Easily condensed the gas, but found the cement of the caps leaked and therefore could not proceed to ascertain pressure at various temperatures. The Guage had 81 parts and the mercury stood at 14 parts nearly when the liquid was in the E.C.A. in air, giving the pressure then as 5·8 atmospheres.

7275. *Phosphuretted hydrogen* (7251–3).

The gas made 29 Nov. (7251) still seems good and rich in phosphorus, and though it has deposited some films in the water, the gas itself, burnt from a jet, appears to be excellent phosphuretted hydrogen. It will not take fire spontaneously. Pumped it in (after the Carbonic acid), cooling the gas in its passage by sending it through a long thin spiral glass tube immersed in a bath at 0°. The pressure guage used had 91 parts.

7276. This gas condensed at a high pressure and produced a clear transparent liquid body. For this it required the E.C.A. bath cooled in vacuo; and this temperature did not freeze the fluid.

7277. There was leakage through the metal of the cap, so that I could get no precise results of pressure. But whilst the tube was attached to the condsg. pumps, the pressure guage was at

25 parts or 3·24 atmospheres. When the piston was suddenly depressed, the guage rose to 3 parts or 30 atmospheres but instantly, because of the leakage, fell back to 25 parts.

7278. When the condensing tube was taken off from the pumps, the liquid before in the tube gradually disappeared (because of the leakage); but on putting the bent part into a bath of E.C.A. in air, liquid instantly appeared and the guage was then 65 parts, so that the pressure was 1·23 atmospheres (I have some doubts about these numbers). But is clear that there was then a phos. hydrogen in the tube condensible by that temperature and pressure, and there must have been another Phos. hydrogen or pure hydrogen to account for the higher pressures before hand. Repeat.

7279. *Sulphuretted hydrogen.* Bar. 30 inches. (7312).

Made the gas first (from M.A. and Sulrt. Antimony) over water into bottles, to deposit impurity, and kept it 12 hours. Then passed it, at common pressures, first through a long thin spiral condenser at 0° and afterwards into the condensing tube cooled in the E.C.A. bath; using a horizontal tube with the guage end away from the cold bath. The fluid Sulphuretted hydrogen condensed beautifully and was perfectly clean and clear. The guage had 81 parts. The following estimate of pressure was obtained in the following order:

0°	15·4 parts or	5·26 atmospheres
15°	10·6	7·64
20°	9·8	8·265
30°	7·5	10·8
40°	6·7	12·09
50°	6·03	13·43
60°	5·48	14·78
50°	6·0	13·5
40°	6·66	12·16
32°	7·6	10·66
0°	14·6	5·54

7280. *Muriatic acid.*

Condensed in a tube, being sent in from a dry generator. All was in good condition. The Guage had 42 parts. The following are results:

0°	2·7 parts or	15·55	
10°	2·3	18·26	
20°	2·0	21·00	
24°	1·8	23·33	
28°	1·71 or 1·72	24·56 or 24·42—only a trace of liquid left,	
18°	2·06	20·4	so perhaps doubtful.
0°	2·7	15·55	
E.C.A. in air	12·2	3·44	

7281. *Arseniuretted hydrogen.*

Well prepared and condensed, having passed through a long thin cond. at 0° before it entered the final condensing tube, which was , and had a guage turned from the cold with the mercury at 48 parts. The mercury moved well and freely.

3°	8·75 parts	5·486 atmospheres
10°	7·125	6·73
20°	6·3	7·62
32°	5·3	9·06
40°	4·6	10·43
50°	4·1	11·7
60°	3·65	13·15
60°	3·6	13·33
50°	4·0	12
40°	4·6	10·43
32°	5·28	9·09
20°	6·27	7·65
0°	9·3	5·16
−76°	48	1
−64°	34	1·12
−52°	26	1·85
−23°	14	3·43
−36°	18	2·66
−24½°	14	3·43
−53°	26	1·85
− 5°	9·8	4·9

Even next day (Jany. 1), the tube contained clean good liquid arsend. hydrogen; and when opened, the mercury returned to 48 parts on the guage.

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7282. *Fluoboron.*

Refuzed boracic acid and pulverised it finely. Examined Fluor spar for Carbonate of lime; pulverized it and heated it. Boiled the Sulc. acid and got off much water. Then with these made mixture for Fluoboron, a soft paste. It was excellent—gave plenty of good gas and did not boil up in the retort. It was sent into a condensing tube having the bend immersed, first in an E. C. A. bath in air, and afterwards in an E. C. A. bath which had been reduced in temperature by rarefaction in air pump jar. But there were no signs of condensation.

Must try it with pressure, as I can easily do now.

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7283. *Arseniuretted hydrogen.*

Have considered the data (7281) and the former data (7172, 7182, 7101, 7161, 7172, 7175). Those 7281 were with a horizontal tube, guage always at common temperatures and good clear liquid arsend. hydrogen and tube. When opened the next day, all was good and the guage mercury returned to its right place.

The numbers in the margin[1] are I think a good approximation, and those ✲ marked are close to the best experimental results, and are the data on which the curve and other numbers are constructed[2].

√ ✲	−75°	0·94	√ ✲	− 5°	4·74
√	−70°	1·08	√ ✲	0°	5·21
	−65°	1·23	√ ✲	3°	5·56
√ ✲	−64°	1·26		5°	5·71
√	−60°	1·40	√ ✲	10°	6·24
	−55°	1·59		15°	6·80
√ ✲	−52°	1·73	√ ✲	20°	7·39
√	−50°	1·80		25°	8·01
	−45°	2·03	√	30°	8·66
√	−40°	2·28	√ ✲	32°	8·95
√ ✲	−36°	2·50		35°	9·34
	−35°	2·55	√ ✲	40°	10·05
√	−30°	2·84		45°	10·79
	−25°	3·16	√ ✲	50°	11·56
√ ✲	−23°	3·32		55°	12·36
√	−20°	3·51	√ ✲	60°	13·19
	−15°	3·89		65°	14·02
√	−10°	4·30			

[1] i.e. in the table below.
[2] The ticks are in pencil in the MS.

7284. *Mur. acid* (7293).

Considered the last results and have much confidence in them on laying down the curve and numbers; for these data were with a horizontal tube and guage at constant temperature (7280), whilst the former results (7205) were with this tube , and so the guage cooled. This particularly affects the result at – 103° F. (7205). The result at 50° is from my former first paper. Those marked ✲ are the experimental numbers and agree well.

✲	–125°	1 atmospheres
✲	–103°	3·35
✲	0°	15·4
	5°	16·6
✲	10°	18
	15°	19·6
✲	18°	20·7
✲	20°	21·5
✲	24°	23·26
	25°	23·7
✲	28°	25·2
	30°	26·2
	35°	29·0
	40°	32·2
	45°	35·8
✠	50°	39·8

7285. If at (7205) the end of the guage which was in the part of the bent condensing tube immersed in the E. C. A. bath in air (and which, being moved about, would make the E. C. A. bath considerably below – 103° F.), be considered as at – 100° F., then the contraction of the air by cold would be about $\frac{10}{33}$ or 0·304 of the whole; and this, instead of making the pressure for the E. C. A. bath 2·2, would make it 3·16. The observation of –125° is independant of the guage, because at that temp. the gas condensed under atmospheric pressure, or at least very little more.

7286. *Sulphuretted hydrogen* (7029, 7159, 73, 76, 7279). On the following page[1] are a set of numbers for sulphuretted hydrogen, but they are exceptionable. From –100° to 0, the experimental numbers were too low because the closed end of the guage was in the part of the tube in the E. C. A. bath (7284), but in the table of curves they have been corrected and the cor-

[1] i.e. in the table following.

rected numbers are used in the next page. From 0° to 60° there are two sets of numbers, differing considerably. I have used those of (7279) chiefly, which give the highest numbers; but the data ought to be brought better together. The numbers at −100° are probably too high, for Sul. Hydrogen condenses in an E. C. A. bath at common pressures. The number of 17 atmospheres at 50° given in the old paper on the gases is too high.

−100°	1·3	−15°	4·75
−95°	1·36	−10°	5·15
−90°	1·42	− 5°	5·58
−85°	1·50	0°	6·04
−80°	1·60	5°	6·53
−75°	1·72	10°	7·05
−70°	1·86	15°	7·60
−65°	2·03	20°	8·29
−60°	2·22	25°	9·02
−55°	2·42	30°	9·79
−55°[1]	2·64	35°	10·60
−45°	2·88	40°	11·45
−40°	3·14	45°	12·34
−35°	3·42	50°	13·37
−30°	3·72	55°	14·34
−25°	4·04	60°	15·45
−20°	4·38		

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Barometer at 30 inches.

7287. *Fluoboron* (7056)*.

Arranged a horizontal tube and attached to it, by a piece of brass tube very slightly bent, a generator containing well prepared fluor spar and boracic acid in powder mixed with well boiled sulphuric acid. Heated to evolve gas and allowed it to pass through the apparatus, then closed the extreme stop cock and allowed the pressure to accumulate. Used first an E. C. A. bath in air at the flexure to condense, but this did not bring down the gas into liquid, so cooled a bath in vacuo and applied it, and then the gas condensed.

7288. Fluoboron was then a very limpid, colourless, transparent fluid, apparently light, presenting no signs of crystallization or solidification though the temperature must have been very low;

[1] ? in error for 50°.

* [7287] 

probably −140° F. The guage in the tube was new and divided into 30 parts and played well. The gas condensed when the pressure was about 3 atmospheres. Afterwards obtained the following numbers well.

	parts	
−100°	6·5	4·61 atmospheres
−82°	4	7·5
−72°	3·25	9·23
−67°	3	10·00
−65°	2·6	11·54
−66°	3	10·00
−62°	2·6	11·54

Afterwards, when the gas was allowed to escape, the tube became perfectly dry and clean and the guage mercury returned to 30 parts.

7289. Could not go to higher pressures, for at −62° there was just a trace only of liquid left; at −66°, abundance of liquid; at −65°, plenty; at −62°, very little.

7290. *Phosphuretted Hydrogen.*

Repeated the former experiment (7275) with part of the gas then left unused (7275, 7251) and which still seemed very good. The guage had a division into 68 parts. The phenomena were as before. The gas would not condense at pressures of 20 and 25 atmospheres in an E. C. A. bath in air, but on the application of an E. C. A. bath cooled in vacuo, it condensed, and a clear, limpid, light, colourless liquid appeared, shewing no signs of freezing at the lowest temperature.

7291. By pumping, the guage was often at 2·5 and 3·5 parts, or at 27·2 and 19·4 atmospheres. The liquid remained at the 19·4 atmospheres, which increased very little at the 27·2 atmospheres, as if there were two gases mixed, the one incondensible and the other condensible. Whether this be because pure hydrogen is mixed with the phos. hydrogen, or that there are two phosphuretted hydrogen[s], I cannot say; but at all events, there is a condensible Phosphuretted hydrogen.

7292. There was a slight leak in the cement of one of the caps, which made the sinking from the higher pressure to the lower; it was small. The condensation into liquid was good. When the gas was let off the liquid rose rapidly in vapour and the guage mercury returned well to its place.

7293. *Muriatic acid* (7284, 7302).

Obtained a good specimen of liquid Mur. acid in a good horizontal tube, using a generator with pressure from evolution as before (7280, 7287). The guage also was very good and was divided into 64 parts. Pressures were obtained in an E. C. A. bath temperature, but the thermometer (   ) used to give its temperature is slow in taking that temperature, and though I stirred constantly and allowed all the time I could, still, if the bath was rising in temperature, the tube and liquid would be before the thermometer, and therefore the pressure indicated by the guage would belong to a temperature somewhat higher than that shewn by the thermometer[1].

		Guage 64 parts.
E.C.A. in Air *106*	46 parts	1·4 atmospheres
−105°	42	1·52
−100°	36	1·777
−92°	28	2·29
−83°	22	2·9
−77°	19	3·37
−67°	15	4·266
−53°	11	5·82
−42°	9	7·11
−33°	7·5	8·533
−22°	6	10·66
− 5°	4·6	13·91
0°	4·3	14·9
10°	3·5	18·28
18°	3·0	21·33
25°	2·7	23·7
31°	2·5	25·6
36°	2·33	27·46
liquid yet 42°	2·18	29·36
32°	2·45	26·12
27°	2·66	24·06
good exp. 19°	3	21·33
good exp. 0°	4·45	14·38

7294. I could not go safely above 42°, because the liquid was then nearly all gone. When experiments were over and vapour allowed to escape, the guage mercury returned to 64 parts.

[1] The italic entries are in pencil in the MS.

7295. The thermometer for E.C.A. bath is of Alcohol. That used for temperatures above 0° is of mercury and much quicker in its taking the bath temperature.

7296. *Nitrous oxide* (7235, 7243, 7303). *Guage* 68 *parts*.

Condensed this gas by the pumps in a tube of this form, as before described. Easily filled the part in the bath with liquid. Had a good guage of 68 parts, and obtained pressures as below.

−80°	11·4 parts	5·965 atmospheres
−67°	10·6	6·415
−55°	9·2	7·391
−45°	8·75	7·771
−32°	6	11·33
−40°	7·25	9·38
−59°	9·4	7·234
−53°	8·6	7·9
−48°	8	8·5
−40°	7	9·714
−32°	6	11·333
−20°	4·75	14·31
−12°	4·25	16
− 8°	3·9	17·43
− 0°	3·65	18·63
0	3·62	18·78
10	2·8	24·28
18	2·7	25·18
23	2·45	27·75
31	2·25	30·22
42	2	34
46	1·8	37·77
51	1·72	39·53

7297. Between −80° and −0°, there was so much liquid as to be in the curved tube nearly at the level of the fluid in the E.C.A. bath, and as I feared that it had not been cold enough at the upper level, I now let out about one third of the liquid and then repeated the following:

−15°	4·6	14·78 atmospheres
−54°	8·5	8
−48°	8·0	8·5

7298. The results shew that I had had good observations in the upper series. At the highest temperature, or 51°, there was plenty of liquid to assure the result; but the diminution of it on falling from 42° to 46°, and from that to 51°, was very striking. More vapour went up than was simply proportionate to the added force in atmospheres. When liquid let off guage returned accurately to 68 parts.

Bar. 30 *inches.*

7299. *Olefiant gas* (7234, 7304). *Guage* 68 *parts.*

Condensed as the last was (7296) by pumps in a tube having a guage of 68 parts, in excellent order. Condensation very excellent; tube full to the level of the E. C. A. bath. The pressure was 2·9 parts or 23·48 atmospheres in this E. C. A. bath, but that was because the surface of fluid was level with or even above the cooling fluid, so warmer. Proceeded to obtain the pressures with this quantity of fluid at 0° and higher degrees, when the fluid rapidly diminishes; and afterwards let off about $\frac{2}{5}$ and then obtained pressures safely and accurately in an E. C. A. bath.

0°	2·6 parts	26·15 atmospheres	
12°	2·0	34	
17°	1·9	35·79	
20°	1·8	37·77	
27°	1·7	40	
30°	1·67	40·72	
8°	2·2	30·9	when cooled here from 30° it was
0°	2·55	26·66	beautiful to see how [illegible].

Now less liquid and the E. C. A. bath and alcohol thermometer.

13°·5	2·125	32
− 5°	2·7	25·177
−25°	3·4	20
−48°	4·28	15·88
−73°	5·28	12·88
−89°	6·1	11·14
−102°	7·1	9·577
−101°	7	9·714
−93°	6·7	10·15
−70°	6	11·33
−57°	5·3	12·83
−42°	4·4	14·54
−40°	4·3	15·81
−37°	4·18	16·268
−25°	3·55	19·15
−14°	3	22·66
−11°	2·9	23·48
− 9°	2·8	24·286
− 5°	2·66	25·56
0°	2·45	27·75
2°	2·4	28·33

FOLIO VOLUME V
OF MANUSCRIPT

10 JANY. 1845.

7300*. *Carbonic acid* (7237, 7254, 7274). Bar. 30 inches. Guage 113 *parts.*

Prepared a tube as in the figure, sealed at one end, capped at the other. Cut long pieces of Carbonic acid and dropped them in until the tube was loosely full to the cap. Closed the cap by a plug screwed in and let the tube warm in the air—it was then full of liquid up to *a*. Cooled the end a little and then slowly opened the plug and let carbonic acid vapour out; of course the liquid within was cooled and froze; at last quickly open[ed] out the plug end—dropped in a guage—replaced the stopper—screwed all up and then let it acquire a temperature of about 0°. Then there was liquid in the tube to about *b*. Not so much as I wished, but very good for a first trial of the arrangement. Guage had 113 parts. Then worked in a bath of Ice, salt, water, etc., and mercury thermometer.

0°	5·5 parts	20·545 atmospheres
10°	4·45	25·393
16°	4	28·25
20°	3·8	29·73
28°	3·4	33·235
32°	3·25	34·77
10°	4·5	25·11

Began with E.C.A. bath and alcohol thermometer—obtained two numbers—

−8°	6·5	17·33
−4°	6·0	18·833

but now no more acid in Addams vessel and so I was obliged to cease working. So left the tube closed up for further observation tomorrow.

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7301. Looked at the C.A. tube of yesterday (7300); there was no liquid and the pressure was 38 atmospheres—but on putting the end into a mixture at 0°, plenty of fluid condensed and the pressure

* [7300]

was then 5·25 parts or 21·52 atmospheres. The experiment was made in haste or else I believe it would have come to yesterday's result. Set it aside again.

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7302. *Mur. acid* (7284, 7293).

Have worked up the numbers of 7293 and compared their curve with that of (7284). See diagram. The results were very good and the following is the table of vapour force. The dotted √ numbers are the results of experiments and are in very close accordance with the table and the interpolated numbers.

√ −105°	1·58 atmospheres	√ −33°	8·53 atmospheres
√ −100°	1·80	−30°	9·22
− 95°	2·07	−25°	10·05
√ − 92°	2·28	√ −22°	10·66
− 90°	2·38	−20°	10·92
− 85°	2·73	−15°	11·84
√ − 83°	2·90	−10°	12·82
− 80°	3·12	√ − 5°	13·88
√ − 77°	3·37	√ 0°	15·04
− 75°	3·55	5°	16·32
− 70°	4·02	10°	17·74
√ − 67°	4·266	15°	19·32
− 65°	4·53	20°	21·09
− 60°	5·08	√ 25°	23·08
− 55°	5·67	√ 27°	23·80
√ − 53°	5·83	30°	25·32
− 50°	6·30	√ 31°	25·76
− 45°	6·97	32°	26·20
√ − 42°	7·40	35°	27·84
− 40°	7·68	40°	30·67
− 35°	8·43		

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7303. *Nitrous oxide* (7296, 7297, 7234[1]).

The numbers for nitrous oxide (7296, 7) have also been laid down as a curve and others interpolated, and the accompanying series is the result, in which I place much confidence. Those marked √ are the direct results of experiments or very near indeed to them. At the temp. of −80° F. (7296), there was evidently too much liquid in the tube, so that the upper portions had not been

[1] ? par. 7235.

cooled to $-80°$ and therefore the pressure is too high. At $51°$ there appears to have been too little liquid. It is to be remembered that any water which would pass with the gas at the temperature at or below $0°$ would at this point be exerting all its solvent power on only a little liquid, and therefore tend to diminish the elasticity of the vapour below what it ought to be.

I should like to repeat from $-50°$ to $-106°$ or lower.

	−67° F.	6·2 atmospheres	√	0° F.	18·92 atmospheres
√	−65°	6·4		5°	20·50
	−60°	6·95		10°	22·18
√	−59°	7·02		15°	23·96
√	−55°	7·55	√	18°	25·1
√	−53°	7·75		20°	25·84
	−50°	8·2	√	23°	27
√	−48°	8·48		25°	27·82
	−45°	8·9		30°	29·90
√	−40°	9·68	√	31°	30·2
	−35°	10·54	√	32°	30·95
√	−32°	11·11		35°	32·08
	−30°	11·48		40°	34·36
	−25°	12·50		45°	36·74
	−20°	13·60	√	46°	37·15
√	−15°	14·78		50°	39·22
√	−12°	15·58	√	51°	39·55
	−10°	16·06			
	− 5°	17·44			

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7304. *Olefiant gas* (7299, 7234, 7313, 7316).

Numbers laid out as before—and those marked thus [√] may be considered as experimental numbers.

Should like to correct these numbers by a second set of experiments, especially for the colder degrees.

Found on the results of (7299) that the mercurial thermometer shewed its slowness in taking temperature of bath—for as we descended in temperature, the temp. was given by it too high for the real temperature and therefore for the pressure, and as one ascended in temperature, it was given too low for both. Hence the curve comes between the series of numbers.

°F.	atmospheres	°F.	atmospheres
−105°	8·85	−20°	21·23
√ −102°	9·08	−15°	22·50
√ −101°	9·18	√−14°	22·70
−100°	9·30	√−11°	23·30
− 95°	9·77	−10°	23·89
√ − 93°	9·96	√− 9°	24·20
− 90°	10·26	√− 5°	25·41
− 85°	10·78	√ 0°	27·18
− 80°	11·33	√ 2°	28·00
− 75°	11·91	5°	29·30
√− 73°	12·18	√ 8°	30·74
− 70°	12·52	10°	31·70
− 65°	13·17	12°	32·72
− 60°	13·86	15°	34·20
− 55°	14·59	√ 17°	35·20
− 50°	15·36	20°	36·80
√− 48°	15·70	25°	39·60
− 45°	16·18	√ 27°	40·70
− 40°	17·05	30°	42·50
− 35°	17·98	35°	
− 30°	18·98	40°	
− 25°	20·06	45°	

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7305. A small tube about $\frac{1}{5}$ of inch internal diameter had its capacity measured—3 feet of it held just 2 cubic [inches]. Powdered chlorate of potassa was then put into it—18 inches in length held 150 grains, or 3 feet 300 grains. 300 grains will give about 300 cubic inches of oxygen. So probably a mixture of dry chlorate of potassa and oxide of manganese, being put into a tube for 1 foot in length, would fill that and 2 feet more of vacant tube with oxygen at 50 atmospheres. This will do.

Thermometers.

7306. Have examd. the thermometers. The Mercurial thermometer used is ready in its taking temperature and comes in ice and water correctly to 32°. The large spirit thermometer (7088) is very slow in taking final temperature (7295), and I have little doubt that all the tension[s] taken as the bath rose in heat (7302, 7303, 7304) are those of higher temperatures than those indicated by the thermometers and set down in the results. It comes right to the mark on the glass stem at 32°, but it shifts on its attached scale, and when the scale is resting on the stem, it so settles that the spirit, which should indicate 32°, shews 34°.

7307. Have found a small Alcohol thermometer going down to −130° or −140°. It takes temperature far quicker than the former and is better in that respect. The spirit in the stem settles 1° above the mark for 32° on the stem. The instrument is stiff on the stem and, being adjusted to allow for the above, is a very good instrument. I purpose using it tomorrow.

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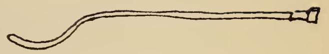
7308. *Carbonic acid* (7314). Bar. 30·35 inches.



Used a second tube like that before described (7300), and introduced Carbonic acid and a guage as before—all in good order, after one slight derangement of the guage which was rectified. The guage was one of 72 parts. Put this and the former tube (7300) (the ends) into a mixture of ice and salt until they had acquired the same temperature. Being then examined by their respective guages, they gave accurately the same pressure of 26 atmospheres and a fraction. Very Good result, shewing that the old tube, now 11 days old, keep constant and also its guage.

Now employed this tube (7308) to obtain pressures, and two thermometers, one mercurial (7306) and the other the small Alcohol one (7307). The results were as follows:

		Guage 72 parts	
− 3°	3·4 parts or	21·17 atmospheres; multiplied by 1·012 to make them atmospheres at 30 inches:	21·42
0°	3·3	21·82	22·08
+ 5°	3·0 or 2·99	24 or 24·08	24·29
10°	2·68	26·87	27·19
15°	2·54	28·34	28·68
18°	2·40	30	30·36
23°	2·20	32·72	33·11
32°	1·90	37·9	38·35
37°	1·75 perhaps or 41·1 at.	exploded—eyes escaped—had glasses on.	

7309. As the other tube (7300) was in accordance and had plenty of fluid at 0° and lower temperatures, resumed working with it and an Ether Carb. Acid bath—but now had the former large alcohol thermometer (7088, 7306) and endeavd. to guard against its slowness in acquiring temperature. Had also a different guage and one of 113 parts which has been in the tube 11 days. After the experiments, the tube was opened and the guage returned to 111·5 parts, being a diminution of 1·5 parts, but the Barometer is now 30·35 inches, whereas it was at 30 inches when the guage was inclosed. Hence it remains unchanged as to the air within. So if I yet consider the guage as 113 parts, it will correct pressure within the C.A. tube to atmospheres of 30 inches. I have much confidence in the first seven results, and also I think the rest are likely to be good, for I took pains to get the true temperature of the bath.

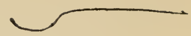
-9°	5·8 parts	19·48 at.
$-20^\circ{\cdot}5$	7·0	16·14
-56°	15·0	7·53
-75°	23·0	4·91
-101°	68	1·66
-111°	99	1·14
-107°	79·6	1·42
-102°	63·5	1·78
-95°	49	2·306
-83°	31	3·64
-73°	22·5	5·02
-53°	14	8·07
-41°	11	10·27
-34°	9 about	12·55
-23°	7·5	15·06
-21°	7	16·14
-15°	6·4	17·65
-9°	5·7	19·80
-4°	5·25	21·52
-6°	5·55	20·36
-3°	5·2	21·73
$+4^\circ$	4·52	25

See (7314) for results.

7310. As to the tube that burst—the piece left in the cap had an external diameter of 0·2416 of an inch and an internal diameter of 0·1833 of an inch—half the difference, or ·02916, is the thickness of the glass there. By the appearance of the fragments, I think this must have been the thinnest part of the tube. Another part had external diameter ·2708, internal diameter ·2041, thickness ·03335 of an inch—more than former.

7311. Consider therefore that this tube of 0·1833 of inch internal diameter and 0·02916 of inch in thickness burst with pressure of

41 atmospheres. It was of green bottle glass and had bends in it thus.



7312. *Sulphuretted hydrogen* (7279, 7315).

Condensed this gas in a tube of this kind, by an E.C.A. bath cooled under the air pump just as on the last occasion (7279), and used the gas left from that time, which had been kept in stoppered bottles. The gas condensed well—the only inconvenience was from its freezing and partially blocking up the way. The guage had 61 parts at the present bar. pressure of 30·35 inches, so that the atmospheres below are too heavy.

The results down to −2° were ascertained by an E.C.A. bath and the large Alcohol thermometer (7088, 7306). The results from 0° and upwards were in a salt and ice bath, etc., with the same thermometer. The slip of the thermometer scale is here guarded against. At 52° the liquid sulphuretted hydrogen was all gone. When the tube was opened the guage came back to 61 parts.

		atmospheres	multiplied by 1·012 (7308) gives	at. at 30 inches
− 94°	52 parts	1·17		1·18
−106°	63 (below 1 at.)	0·968		0·979
−100°	61	1		1·012
− 90°	54	1·018		1·03
− 83°	49	1·24		1·254
− 74°	41	1·51		1·528
− 68°	37	1·65		1·67
− 62°	33	1·848		1·87
− 59°	31	1·97		1·993
− 58°	31	1·97		1·993
− 48°	25	2·44		2·469
− 40°	21	2·90		2·934
− 29°	17	3·59		3·633
− 24°	15	4·06		4·108
− 16°	13·6	4·48		4·53
− 8°	12	5·08		5·14
− 2°	10·8	5·65		5·717
− 20°	15	4·06		4·108
0°	10·9	5·6		5·66
8°·5	9·5	6·42		6·497
11°	9	6·77		6·85
17°	8·2	7·44		7·529
26°	6·5	9·38		9·49
34°	5·5	11·09		11·22
39°	5	12·20		12·34
48°	4·5	13·55		13·71
? 52°	4·25	14·35		14·52
27°	6·25	9·75		9·86
− 45°	24	2·54		2·57

7313. *Olefiant gas* (7299, 7304, 7234, 7313, 7316).

Condensed by the pumps, etc. as before (7299) in a tube of this form. The guage mercury stood at 64 parts with present pressure of 30·35 inches Bar.

Found that at −5° there was no liquid—had not looked at −7° and had no more C.A. so as to go back in temperature. Suspect −7° and am a little jealous of the result at −13°.

All these with the large Alcohol thermometer (7088).

Did not open the tube but put away to see what would happen with it and the cement—expect the cement will condense the Oleft. and perhaps soften and let it leak away.

Guage 64 parts at 30·35 Bar.

			multiplied by 1·012 for atmosphere of 30 inches.
−26°	3·5 parts	18·28	18·51
−11°	3·25	19·69	19·92
−29°·5	3·8	16·84	17·04
−52°	5·8	11·03	11·16
−70°	7·9	8·10	8·19
−93°	12	5·33	5·394
−99°	14	4·57	4·62
−96°	12·5	5·12	5·18
−83°	9·5	6·73	6·81
−61°	6·5	9·84	9·95
−61°	6·75 (good)	9·48	9·59
−52°	5·6	11·42	11·55
−41°	4·6	13·91	14·07
−38°	4·5	14·22	14·39
−37°·5	4·33	14·78	14·95
−25°	3·6	17·77	17·98
−23°	3·55	18·03	18·24
−21°	3·45	18·55	18·77
−14°	3·33	19·28	19·51 ?
−13°	3·30 ?	19·39 ?	19·62 ?
− 7°	3·28 ?	19·51 ?	19·74 ?
− 5°	3·28 ? no liquid	19·51 ?	

27 *Jany.* Looked at tube—all safe and pressure at 3·25 parts—very good—no apparent action as yet on the cement.

7314. *Carbonic acid* (7308, 9).

These numbers are deduced from the results at 7308, 9 and from their accordance with the experimental results give me great confidence. Those marked √ are close to the results of experiment.

Much care was taken in procuring the temperature of the bath. I believe they are the best yet obtained amongst these gases.

√[1]	√	−111°	1·14
√		−110°	1·17
√	√	−107°	1·36
		−105°	1·48
	√	−102°	1·68
	√	−101°	1·76
√		−100°	1·85
√	√	− 95°	2·28
√		− 90°	2·77
		− 85°	3·32
√	√	− 83°	3·60
√		− 80°	3·93
√	√	− 75°	4·60
	√	− 73°	4·93
√		− 70°	5·33
		− 65°	6·12
√		− 60°	6·97
√	√	− 56°	7·70
		− 55°	7·89
√		− 50°	8·88
		− 45°	9·94
√		− 40°	11·07
		− 35°	12·27
√	√	− 34°	12·50
√		−30°	13·54
		−25°	14·88
√	√	−23°	15·45
	√	−21°	16·00
	√	−20°·5	16·15
√		−20°	16·30
√	√	−15°	17·80
√		−10°	19·38
	√	− 9°	19·80
		− 5°	21·06
√	√	− 4°	21·48
	√	− 3°	21·80
√		0°	22·84
√	√	5°	24·75
√	√	10°	26·82
√	√	15°	29·09
	√	18°	30·65
√		20°	31·56
√	√	23°	33·15
		25°	34·26
√		30°	37·19
√	√	32°	38·50
		35°	40·35
√		40°	

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7315. *Sulphuretted Hydrogen.*

Arranged from the data of (7312). The lower temperatures are probably very good, but between 15° and 30° there appears to have been some error or change. If the thermometer scale had slipped down there on the glass stem and so given 3° lower temperature than it ought, i.e. if a sudden difference of 3° was thus introduced into the series of observations (which were made with one thermometer, the large alcohol) it would account for much of the irregularity, but not all.

Hence this curve is not so correct, in my mind, in form as it probably might be, or ought to be.

[1] In the MS. the ticks in the first column are in pencil; those in the second column are in ink.

F.		F.	
√[1] −105°	0·97 atmospheres	−25°	3·85 atmospheres
√ √ −100°	1·02	√ √ −24°	3·95
−95°	1·08	√ √ −20°	4·24
√ √ −94°	1·09	√ √ −16°	4·60
√ √ −90°	1·15	−15°	4·66
−85°	1·23	√ −10°	5·11
√ √ −83°	1·27	√ −8°	5·32
√ −80°	1·33	−5°	5·59
−75°	1·45	√ √ −2°	5·90
√ √ −74°	1·50	√ 0°	6·10
√ −70°	1·59	5°	6·64
√ √ −68°	1·67	√ 10°	7·21
−65°	1·75	15°	7·81
√ −62°	1·86	√ 20°	8·44
√ −60°	1·93	25°	9·14
√ √ −58°	2·00	√ √ 26°	9·30
−55°	2·13	√ 27°	9·48
√ −50°	2·35	√ 30°	9·94
√ −48°	2·47	35°	10·84
√ √ −45°	2·59	√ 40°	11·84
√ √ −40°	2·86	45°	12·94
−35°	3·16	√ √ 48°	13·70
√ −30°	3·49	√ 50°	14·14
√ −29°	3·60	√ √ 52°	14·60

7316. *Olefiant gas* (see 7313 for data).

The numbers below or rather above −15° are in error for want of fluid; there was none at −5° and perhaps not at −7°.

The pressures here are very different from the former (7299, 7304), being much lower, though consistent with themselves. I still suspect that there are different hydrocarbons produced—perhaps having the same composition.

Must therefore give the pressures as uncertain.

−100°	4·60	−75°	7·60
√ −99°	4·70	√ −70°	8·32
√ −96°	5·05	−65°	9·08
−95°	5·12	√ −61°	9·71
√ −93°	5·37	−60°	9·88
−90°	5·68	−55°	10·76
−85°	6·28	√ −52°	11·36
√ −83°	6·55	−50°	11·72
−80°	6·92	−45°	12·78

[1] See footnote on p. 227.

√ −41°	13·70	√ −29°·5	16·65
−40°	13·94	√ −25°	18·02
√ −38°	14·44	√ −23°	18·65
√ −37°·5	14·60	√ −21°	19·20
−35°	15·20	−20°	19·58
−30°	16·56		

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7317. *Oxygen.*

Worked in preparation for this gas, purposing to obtain it from a mixture of oxide of manganese and chlorate of potassa in a close tube.

7318. Manganese requires to be heated red hot and for some time, else part of the water (it is a hydrate) is retained by it. Also, when heated, if left exposed to air only for 10 minutes, it absorbs much water, i.e. for such experiments as mine. Must heat it and mix hot—not using paper but tin foil, and must put it into the tube whilst hot.

7319. When chlorate (previously fuzed) is mixed with such oxide of manganese, it requires more heat for the decomposition than if the water were left in. Water helps it much, just like carbonate of lime in the kiln.

7320. Even when there are equal parts by weight of chlorate and the manganese, still on heating in a tube the chlorate melts and tends to puff up and so to stop up the tube and make the action irregular.

7321. Oxide of copper does not do so well as oxide of manganese —it requires a higher heat.

7322. Two by weight of the heated oxide of Manganese and 1 by weight of the fuzed chlorate were powdered and mixed well and put into a narrow green glass tube by a funnel of tin foil. When in the tube, it occupied 9 inches in length, and its weight was 38 grains, so that 12·6 grains of chlorate were there. These 9 inches in length of the tube had a capacity of 0·2 of a cubical inch or *less*. Connected this with a refrigerating tube and pneumatic trough, and decomposed by spirit lamp. Could decompose it without displacement of the charge, but it melted at first and required time and care, and the tube was occasionally almost visibly red beneath. The quantity of gas which came off was 12·75 cubic inches. On reheating the tube to a higher degree, found no more chlorine came off. The oxygen smelt a little of chlorine. Only very little water was in the tube this time.

7323. So here 38 gr. of mixture or 12·6 gr. chlorate gave 12·75 c. inches of oxygen from a charging occupying tube space equal to 0·2 c. inches. The evolution would give 64 atmospheres in that space.

7324. As the pressure will rise in the tube the bubbles will be smaller—and this with care may allow perhaps 1 Chlorate to 1½ oxide of Manganese. Still, must use a very small condensing tube or else have too much gas space—also a larger generating tube.

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7325. *Oxygen* (7356, etc.).

Proceeded to experiment with oxygen. Had a generator, a tube* 12½ inches long and 0·23 c. inches in capacity, capped, etc. Had also a horizontal condensing tube†, small and strong, of the capacity of 0·13 of cubic inch, and in it a guage up to 103 parts. Made a mixture (with all the precautions (7317–22)) of 75 gr. chlorate potassa and 130 gr. of oxide of manganese—introduced 55 grs. of this into the generator, in which it occupied 9 inches in length; and having filled the generator and condensing tube with pure oxygen, screwed both together with a cock between and also closed the terminal cock, screwing up the plug and covering the end with a cap.

7326. Applied heat of a spirit lamp to the generator—decomposing that portion of chlorate first which was nearest the condenser. All went on very well as to decomposition, but when the pressure indicated was 16 atmospheres, found something leaked. Screwed up the cock plugs and the cocks, and tried again. At 20 or 25 atmospheres still leaked. On shutting the cock between the condenser and generator, the pressure fell faster than before in the condenser, and on opening the cock the pressure rose in the condenser. So it is in some part of the condenser arrangement that the leak takes place. The two caps on this tube are new and bored out of solid metal, but they have not been heated in wax. The leak may very likely be in one of them.

* [7325]

† [7325]

7327. Carried on the decomposition, which proceeded well to the end—and hastening it, I rose up to 26 atmospheres. Thinking that perhaps the Manganese might as a porous body absorb or combine with oxygen when cold, I heated the whole tube at once, but that caused no elevation of pressure. And I think that the decomposition is good and complete, but that the pressure was low because of loss by the leak.

7328. Could go no further and therefore took down the apparatus —intend then to reheat the generator and see whether it will give any more oxygen or whether really all has come away (   ). Did so and obtained 1·5 c. inches more.

55 grains of the mixture would give 20·1 cubic inches of gas, which minus 1·5 leaves 18·6 c.i., and if all this retained in the two tubes, the pressure would have been equal to 51·66 atmospheres: not enough. Not Enough: use 75 chlorate and 110 Manganese.

7329. *Nitrous oxide.*

Proceeded to condense Nitrous oxide and obtain its vapour pressure. Took all care to have the liquid in the tube thoroughly immersed in the cold bath. Had a new thermometer made with care and small in the bulb, to acquire temperature more readily—it appears to act well and be good. The following are the results as they were obtained in order. The barometer only 29·25 inches. *Guage* 60 *parts*= 58·5 parts at 30 inches.

	parts	atmosph. at 30 inches		parts	atmosph. at 30 inches
− 62°	6·4	9·14	−54°	7·15	8·18
− 90°	7·55	7·75	−52°	6·8	8·6
−100°	8·45	6·93	−49°	6·7	8·73
−102°	8·8	6·64	−38°	5·75	10·18
−105°	9·0	6·5	−37°	5·7	10·26
−106°	9·3	6·3	−26°	4·72	12·4
−105°			−25°	4·65	12·6
−102°	9·6	6·1	−24°	4·6	12·71
− 92°	9·3	6·3	−17°	4	14·62
− 83°	8·95	6·53	− 9°	3·5	16·71
− 82°	8·9	6·57	− 8°	3·45	17
− 74°	8·5	6·88	− 2°	3·1	18·86
− 72°	8·35	7	− 1°	3·1	18·86
− 70°	8·25	7·09	− 6°	3·4	17·2
− 57°	7·3	8·01	− 6°	3·4	17·2

	parts	atmosph. at 30 inches		parts	atmosph. at 30 inches
−40°	5·55	10·54	−18°	4·18	14
−36°	5·3	11·04	−17°	4	14·62
−59°	6·8	8·6	−10°	3·5	16·71
−56°	6·7	8·73	− 9°	3·48	16·81
−64°	7·25	8·07	− 9°	3·48	16·81
−61°	7·15	8·18	− 1°	3·18	18·4
−56°	6·9	8·48	0°	3·05	19·18
−52°	6·7	8·73	7°	2·7	21·66
−49°	6·5	9	10°	2·55	22·94
−47°	6·35	9·21	14°	2·4	24·38
−45°	6·2	9·43	18°	2·26	25·89
−35°	5·4	10·83	18°	2·25	26
−33°	5·3	11·04	22°	2·15	27·21
−32°	5·2	11·25	26°	1·95	30
−26°	4·6	12·71	21°	2·17	27
−24°	4·55	12·85	17°	2·32	25·21
−23°	4·5	13			

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Nitrous oxide.

7330. The tube (7329) which was put aside last night was this morning examined and found all safe. There was no liquid visible, and the guage was at 1·3 parts, the pressure therefore being 48 atmospheres at this temperature and with this imperfect atmosphere within. Guage 60 parts = 58·5 parts at 30 Bar.

7331. With a bath of ice and salt cooled the bend, which immediately made abundance of liquid appear, and then obtained the following pressures. The barometer is now 29·45 inches, but the guage was made 60 parts yesterday when the Barometer was 29·25 inches.

	parts	atmospheres of 30 inches Mercury
32°	1·6	36·56
25°	1·75	33·43
23°	1·8	32·5
16°	1·92	30·46
10°	2·14	27·33
7°	2·2	26·6
0°	2·4	24·37
0°	2·4	24·37

Nitrous oxide.

7332. Have been laying down the numbers of 7329, 7331 and applying a curve, and as before said (7008, 7009), there seems every reason to believe that two bodies are in the liquid, one much more elastic than the other, but each *soluble* in the other. The numbers of the curve are on the next page[1], and the following remarks occur. By connecting on the diagram the observations in the order in which they were made, it is easy to see that, when made with a falling temperature, the tension of the vapour is much higher for any given degree of temperature than when made with a rising temperature; the pressure for instance at −60° is much higher if the previous temperature has been −35° than it is if the previous temperature has been −95°—as if the higher temperature had volatilized the more volatile part, and which had not condensed thoroughly or been dissolved by the remaining liquor so perfectly as when, before observation at the same temperature, the whole had been cooled to −95° or the lowest degree.

7333.

	atmospheres (7329)		atmospheres (7329)	atmospheres (7331)
−105°	6·25	−35°	10·95	
−100°	6·39	−30°	11·8	
− 95°	6·54	−25°	12·75	
− 90°	6·70	−20°	13·8	
− 85°	6·87	−15°	14·95	
− 80°	7·05	−10°	16·20	
− 75°	7·25	− 5°	17·55	
− 70°	7·50	0°	19·05	24·40
− 65°	7·80	5°	20·70	26·08
− 60°	8·15	10°	22·50	27·84
− 55°	8·55	15°	24·45	29·68
− 50°	9·00	20°	26·55	31·62
− 45°	9·55	25°	28·85	33·66
− 40°	10·20	30°		35·82
		35°		38·10
		40°		

7334. Again, considering the form of the curve between −35° and +35°, the part between −35° and −105° is not consistent, for it shews far too great an elasticity of vapour, which instead of being at −105° above six atmospheres, ought not to be above

[1] i.e. in par. 7333.

one atmosphere if it were produced by a fluid like that which remains liquid in the tube at temperatures of -35° and above. There must therefore be in the tube two substances, the more volatile of which, rising first, gives such high pressures at low temperatures, and the less volatile of which produces the feeble pressures of the higher temperatures. It even seems probable that all or nearly all the more volatile part is in vapour at temperatures above -30° or -35° F. Hence a means of separating in some degree the two substances.

7335. These conclusions are strengthened by the results of (7331). When the observations were made with the liquid as first condensed in the E.C.A. bath, they gave the numbers and curve of (7329, 7333), etc., but on placing the tube away until next day, during which time it had sustained a higher temperature than any before that, and then by a bath of ice and salt (7331, 7333) observing a series of tensions between 0° and 35°, these came out from *four to five* atmospheres higher than the corresponding pressures for the same temperature on the preceeding day; from which it would appear that the more volatile portion, which had by the cold been condensed in and dissolved by the more fixed portion, had been gradually raised into vapour by the continued warm temperature and thus gave a series of higher pressures. Exactly the same effect being produced as happened in the first instance, namely, that on proceeding from high to lower temperatures, too high a pressure is indicated, and on proceeding from low to high temperatures, too low a pressure is shewn; i.e. as compared with the pressure that would be given by the vapour of this *mixed* fluid if sufficient time were allowed.

7336. There appears therefore to be another substance than Nitrous oxide in this liquid, and consequently in the gas from which it was obtained, and which probably has never yet been distinguished. It cannot be Nitrous gas, for there is no sign of reddening by oxygen; it cannot be any of the acids of nitrogen, for the gas has been made some time—made over water and kept in wet bottle. It may be nitrogen, which may be very soluble in Nitrous oxide when cold, but that is not very likely. It may be a new compound.

7337. It is very curious that Chemical analysis has not detected these impurities, so to speak, of Nitrous oxide and Olefiant gas, but has given and assumed that these bodies are perfectly uniform and pure.

7338. Perhaps the presence of a little nitrogen might explain the whole. Must purify by letting off the atmosphere in the tube when it is at -35°.

7339. The Nitrous oxide curve of (7296, 7297, 7243) is almost identical with the principal one now given (7333) up to temperatures of -30°, but below that temperature it leaves the former curve rapidly, indicating much lower temperatures. This would seem to shew that in that case there had been much less of the more volatile substance, Nitrogen or whatever it may be. But still the form of the curve and its crossing that of Carbonic acid would indicate that some remained.

7340. The effects here and in the other cases of irregularity, as Sulphuretted hydrogen, Mur. acid, Olefiant Gas, etc. may perhaps be due to some air. Work all this out by degrees. In that point of view, one might consider and compare what the nitrous oxide curve would become if it were assumed that air equal to 6 atmospheres were in the tube at the temperature of -100°. Whatever the body is, it is soluble in liquid nitrous oxide, more or less, and if it be air or nitrogen, must be equal to $\frac{1}{4}$ of the contents of the tube—for the pressure is only about 40 atmospheres at 45°, and as the 6 parts at -100° would be equal to 9 parts or 9 atmospheres at 45°, there remaining but 31 atmospheres for the Nitrous oxide, and no liquid was present in the tube at 45° F.

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7340[1]. *Olefiant gas.*

As to its impurities, my Oleft. gas could not contain Carbonic or Sulphurous acid ( ), for it stood long over lime and water, but as to Ether vapour, perhaps it might contain some, though I expect the water and lime took out the greater portion of it.

7341. Now as to the ability of water to remove ether. Put a given volume of air into a glass tube over water—let up a little ether—warmed and agitated the tube—the air increased in bulk about one

[1] 7340 is repeated in the MS.

third parts, i.e. three parts became four—agitated and washed with water—and the ether was quickly removed and I believe entirely, for the bulk was very nearly reduced to what it was at first, and the excess was, I believe, air from the water set free by the ether. The bulk was again marked and the tube transferred to a dish with alcohol and the alcohol allowed to displace the water. On agitating it with the remaining air, there was no diminution, as there would have been if ether vapour had been left by the water, but expansion from the addition of a little Alcohol vapour. Now, to take down the alcohol vapour, the tube was transferred over water, the alcohol allowed to flow out by inclining the tube, and water to enter—then agitated all together. Some air was evidently set free from the water and when the water had taken away all the alcohol and vapour, the air remaining was still larger than before—being however only a little larger than it was after the water washing after the Ether. Hence it would appear that the water washing had taken out all the ether, i.e. that not $\frac{1}{100}$ in volume of the aereal contents of the tube was ether after the water washing. Water therefore will sufficiently wash out ether—weak alcohol perhaps better.

7342. Had one of the bottles of Olefiant gas which however had not stood over lime water and lime for purification, and therefore contained sulphurous and carbonic acid. Put some of it in a tube with its bulk of water and agitated it; there was diminution to the extent of about $\frac{1}{10}$ of the bulk.

7343. Put Olefiant gas into a tube over water, added ether, agitated; the 2 vols. of Oleft. became nearly 3 vols.—there was $\frac{1}{3}$ inch of liquid ether below on the water in the tube. Poured out the excess of ether and washed the gas with water—the increase by ether vapour instantly disappeared and the volume of gas became less than at first, and more than was due to the $\frac{1}{10}$ of ether vapour previously in it. Found this was due to a solution of part of the olefiant gas in the former liquid ether and its removal.

7344. For filled a tube over water with the Olefiant gas—transferred it over ether—introduced a little and agitated—there was expansion at first by Ether vapour and probably not more than $\frac{3}{4}$ of the contents of the tube was then Olefiant, the rest being assumed to be ether. Continued to agitate and soon absorption began—went on agitating until at last only $\frac{1}{6}$ of the tube was occupied by a gas which ether would not dissolve, the rest having been dissolved. So taking the contents of the tube as 24 parts, about 18 parts of the original Olefiant gas was left in, and of these, 14 parts were soluble in Ether and 4 parts only insoluble.

7345. Made the same experiment with the same Olefiant gas and tube and strong alcohol, and with a similar result.

7346. A tube containing 77 parts of the Olefiant gas was thus treated with the Alcohol and left 19 parts of unabsorbed gas. So that $\frac{3}{4}$ were soluble in the Alcohol, $\frac{1}{4}$ not. This remaining gas, when transferred and washed over water and tested by a light, was not air, but a combustible gas, burning with a brightness more like that of light hydrocarbonate than of Olefiant gas.

7347. Some of the same Olefiant gas was in like manner agitated with oil of turpentine; $5\frac{1}{2}$ parts originally left 1 part uncondensed.

7348. Another Olefiant gas was tried in the same tube with the oil of turpentine—$5\frac{1}{2}$ parts left $\frac{6}{8}$ of a part or 44 parts left 6 parts. This shews a variation in the different Olefiant gases prepared at different times.

7349. Hence it appears that Ether may be washed pretty well out of Olefiant gas by water. That if alcohol be used, it must be weak—that Olefiant gas or the greater part of it is easily soluble in Alcohol, Ether, Oil of Turpentine, etc., and that perhaps light hydrocarbonate is left. At all events, that when Olefiant gas is cleaned from Sulphuric[1] and Carbonic acids and from Ether, it still is a variable mixture of substances.

7350. Put some Olefiant gas in preparation—first by shaking it well in a bottle with its bulk of water—and then by leaving it over lime and water—where it is to be for 48 hours, with occasional stirring.

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7351. *Olefiant gas.*

Having been making Olefiant gas to-day. The course as usual continued until the gas at last would hardly burn and very pale. Took seven different portions from first to last as specimens and then examined them—first as to the amount abstracted by

[1] ? Sulphurous.

agitation with a little water, which might be suls. acid and ether—second by agitation with lime and water, which might be principally carbonic acid and the rest of the Suls. acid—third with oil of turpentine, which freely abstracts the Olefiant and leaves an insoluble residue. 80 vols. of gas were taken each time and the results roughly ascertained.

7352. *First specimen*—water absorbed little or nothing—lime and water took little or nothing—Oil of turpentine took up 63 parts—and 17 parts were left insoluble in any of these menstrua.

Second specimen—water absorbed scarcely a trace—lime and water absorbed 3·5 or 4 parts—Oil of turpentine absorbed 68 parts—8 parts remained insoluble or $\frac{1}{9 \cdot 5}$.

Third specimen—water absorbed 2·5 parts—lime and water took 3 parts—Oil of turpentine took 67 parts—7·5 parts were left—$\frac{1}{9 \cdot 9}$.

Fourth. Water took 2·5 parts—lime and water 3·5 parts—oil of turpentine 66·5 parts—residue 7·5 parts, $\frac{1}{9 \cdot 9}$.

Fifth. Water took a trace—lime and water 3·8—Oil of turpentine 66·2—residue 10 parts, $\frac{1}{7 \cdot 6}$.

Sixth specimen. Water took 4 parts—lime and water 10 parts—oil of turpentine 47·3 parts—residue 18·7 or $\frac{1}{3 \cdot 6}$.

7353. *Seventh specimen.* All the portions had stood over water some hours and in this I could observe that above two fifths of the original volume had in this way disappeared. Of the residue, 80 parts were taken and from them water by agitation took 10 parts, lime and water took 27·5 parts—oil of turpentine only took 16·2 parts—and 26·3 parts were left unabsorbed, $\frac{2}{3}$. These being transferred over water and washed, etc. then burnt very like carbonic oxide or light hydrocarbonate.

7354. Hence it is very evident that the original gas varies much in the proportions of the bodies present in it during the performance of the preparation. That besides Sulphurous acid, Carbonic acid and Ether vapour, which may be separated by water and lime, there are two other substances from first to last, the one soluble, the other insoluble in Oil of turpentine. The true olefiant part is that which dissolves in the oil of turpentine and it constitutes about 83·75 per cent. of the whole mixture in the middle of the preparation, or 91·08 per cent. of the gas cleaned from Sulphurous and Carbonic acids and Ether. But as yet I do not know that this soluble part is one single substance or more than one. Must prepare it pure and examine it.

7355. The insoluble part appears from first to last. A portion examined on a former occasion appeared to have the characters of light hydro carbonate—the last portion examd. to-day looked rather like carbonic oxide. Probably both are present. Have kept the first and last Jars and must examine them in this respect.

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7356. *Oxygen.*

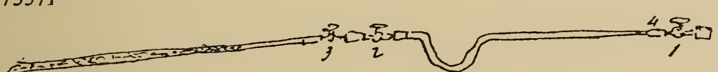
Have prepared the former condensing tube (7325) and also the former and another generating tube—putting on the caps afresh and examining all parts. Have mixed 100 of chlorate and 175 of oxide manganese, making 275 of mixture, and having filled the two generators with oxygen, then introduced this mixture till each was full to within 2½ inches of the cap. After this, put stop cock in to both, re-exhausted them by the air pump carefully, and refilled them with pure oxygen and closed them up tight. 150 grains of the mixture went into the two tubes, equal to 54 of chlorate, of which perhaps 33 grs. were in the larger generator and 21 grains in the smaller.

7357*. A guage was put into the condensing tube (of 101·5 parts —Bar. 30·2 inches) and then much oxygen was passed through the tube. Then the end cock was closed, covered and made fast, and the generator attached to the other end of the condenser tube, but with two stop cocks and a connection between, so that they could be separated without loosing the tension in either. The object was, if the first generator did not give gas enough, to take it off and put on the second.

7358. Applied heat as before (7326); all went on well to 8 or 10 atmospheres: by a little soap suds found a leak in the plug of cock 2. Shut cock 3, separated, replaced the cock 2 by another cock and went to work again. Now no leak up to 20 atmospheres.

7359. Applied an E.C.A. bath in air to the bend and continued to evolve oxygen (beginning at the part nearest to the condensing tube) and gradually raised the pressure until the guage was 1·75 parts or 58 atmospheres of 30·2 inches each, and then applied an

* [7357]



E.C.A. bath cooled in vacuo and probably at −140° F. or thereabouts—but there was no appearance of condensation.

7360. Proceeded to evolve the last portions of gas from this generator, and in doing so a leak or effect of a leak began to be visible by the return of the guage; this slowly increased, and as no more could be done with the apparatus, I put it under water to find the leaks and discerned *one* at the cement of the generator cap—a second at the cement of the furthest cap 4—both of which increased in leaking power by degrees, and a third minute leak at the plug of the cock 1. The other cement and the other cocks and joints stood perfectly well at this pressure.

7361. So can hardly carry this mode of condensation further, since two out of three cements began to leak—but it is important to have reached this point with oxygen, and it is very striking to observe that the glass generator stood this high pressure without blowing out, though parts at the bottom were visibly dull red hot, for in shading them with my hands I could perceive, as I moved the spirit lamp about, that that was the case. Must use forcing pumps.

7362. *Nitrous oxide.*

Have made some pure nitrate of ammonia from Nitric acid pure and Carbonate of ammonia, both free from Muriatic acid. Have decomposed this Nitrate carefully by heat and so prepared Nitrous oxide.

7363. Put on a tube to the pumps with all the precautions of former cases, as condensing tube—refrigeration, etc. etc., and condensed this nitrous oxide into a liquid. The guage had 58 parts, Barometer 30·2 inches, and on the condensation of the first portion of liquid the pressure with E.C.A. bath in air was to 16 parts or 3·625 atmospheres. Threw in more gas and increased the liquid, but now pressure was to 8½ parts or 7 atmospheres nearly. This shews that the gas must be a mixture of substances of different volatilities.

7364. Let off, first on one side and then on the other, vapour, until the liquid reduced to nearly one half, so that all the vapour at first in the tube must have been cast out and now the pressure was less than even at the first. The following are results in the order in which they were obtained.

7365. *Nitrous oxide* (7379). Guage 58 parts. Bar. 30·2 inches = 58·39 parts at 30 inches Bar.

Bath	Temp.	Parts	Atmospheres
Ether and C.A.	−35°	5·75 parts	10·15 atmospheres of 30 inches.
	−13°	3·75	15·57
	−90°	21	2·78
	−86°	20	2·92
	−82°	18	3·24
	−70°	14	4·18
	−60°	11	5·31
	−50°	8·5	6·87
	−40°	6·6	8·84
	−38°	6·5	8·98
	−27°	5	11·67
	−25°	4·8	12·16
	−13°	3·8	15·34
	− 7°	3·4	17·17
	− 2°	3	19·46
	0°	3	19·46
	0°	3	19·46
Ice and salt	− 2°	3·125	18·68
	− 1°	3·08	18·96
	4°	2·85	20·48
	8°	2·62	22·28
	14°	2·44	23·93
	22°	2·13	27·41
	28°	1·85	31·56
	33°	1·8	32·44—but little fluid left.
	− 1°·5	3·17	18·42
	− 2°	3·14	18·59—let off perhaps $\frac{1}{2}$ inch of the fluid.
	− 1°	3·10	18·84—hence there is little change at this

temp.; but now went back to a good ether C.A. bath and even to one cooled under the air pump.

− 98°	29	2·013
− 99°	31	1·88
− 99°	31	1·88
−121°	50	1·167
−118°	48	1·216
−114°	45	1·297
−110°	42·8	1·364
−106°	39·5	1·478
− 82°	20·3	2·876
− 79°	19	3·073
− 58°	10·6	5·51
+ 6°	2·8	20·85

At last relieved the pressure and mercury in guage returned well to 58 parts.

7366. *Olefiant gas.*

Two days ago took Olefiant gas (7350) and prepared it by washing with water and leaving it over lime and water. After the first day, when the film on the surface of the lime and water was broken, it did not form again, shewing the absence of all carbonic and sulphurous acids. There was also little or no smell of ether.

7367. Condensed this gas by the pumps in a tube with aid of E.C.A. bath—the first portion of liquid condensed gave a pressure of 4 parts (in a guage of 58 parts with Bar. 30·2) or 14·5 atmospheres. Then relaxed the cock plug and left it so far open as to allow the escape of about half the liquid—the rest gave a pressure of $9\frac{1}{2}$ parts or 6·1 atmospheres, or less than one half of the former, shewing the escape of a gaseous or more elastic portion. Reduced a little more and then guage was 10·5 parts or 5·6 atmospheres. Threw in and condensed more Olefiant gas—after which, let off vapour two or three times at both cocks, and at last had a fluid left with pressure of 10·5 parts or 5·6 atmospheres in the E.C.A. bath. Proceeded to obtain the following pressures with this fluid. See over page[1].

7368. This fluid is probably the part really soluble in Oil turpentine, but perhaps may consist still of two bodies, and that its solution may tell me. The part which occasioned the high pressure above is most likely the light hydro carbonate found by former expts. (7346, 7353) insoluble in Alcohol.

7369. *Olefiant gas* (7380). Guage 58 parts. Bar. 30·2 inches = 58·39 parts at 30 inches.

− 93°	11·25 parts	5·23 atmospheres of 30 inches Mer.
−102°	12·25	4·76
−102°	12·25	4·76
−103°	12·3	4·74
− 96°	11·25	5·21
− 83°	9·75	5·99
− 82°	9·7	6·02
− 71°	7·6	7·68
− 69°	7·55	7·73
− 57°	5·8	10·07
− 40°	4·3	13·58
− 28°	3·4	17·17

[1] i.e. par. 7369.

− 27°	3·4	17·17
− 15°	2·8	20·85
− 10°	2·6	22·46—little liquor—joined across the tube.
− 6°	2·4	24·33
− 5°	2·38	24·53—very little liquor—did not join across.
+ 3°		
− 13°	2·72	21·47

Now threw in some more Olefiant gas—then let out some vapour and let down tension as before. Now—

−89°	7·9 parts	7·39
−74°	6·6	8·84
−66°	5·9	9·89
−66°	5·85	9·98
−56°	5·35	10·91
−39°	4·18	13·97
−38°	4·12	14·17
−27°	3·4	17·17
−26°	3·35	17·43
−16°	2·8	20·85
−14°	2·78	21 —very little liquid.
−42°	4·3	13·58
−66°	5·75	10·15
−68°	5·45	10·71

7370. After this the liquid was allowed to expand into gas, and examined this gas by oil of turpentine—45 vols. of the gas from the liquid left 5 parts nearly of insoluble gas or $\frac{1}{9}$; which is nearly the proportion of the best specimen of the many (7352). This gas, being in small volume, burnt with a pale blue flame like that of yesterday, and very like the same vol. of coal gas, but not quite so bright as the latter. It was most likely light hydro carbonate.

7371. So very evident that the soluble gas is also the most condensible—but it dissolves with it portions of the light hydro carbonate. If this be separated first, then shall *perhaps* have a pure uniform Olefiant gas—i.e. if there be not also a mixture there.

7372. There is however appearances of a diffnce. here, for Oil of turpentine will take up twice its volume easily of Olefiant gas when the absorption begins, but when $\frac{2}{3}$ of the Olefiant gas is gone, then the rest does not seem so soluble in fresh oil of turpentine.

7373. *Chlorine and Sulc. acid.*

Sealed up some condensed chlorine and well boiled Sulphuric acid in glass tubes (2 specimens). Of course the Sulc. acid easily freezes in the E. C. A. bath, but the chlorine does not. The chlorine floats on the Sulphuric acid, being lighter. Also the Sulphuric acid has no colour and does not appear to have any power of dissolving chlorine. I knew that it did not dissolve the gas chlorine.

7374. *Carbonic acid and Sulphuret of carbon* (7272, 7419).

Sealed up these two fluids in a green glass tube hermetically, and mean to leave them in darkness and in light to ascertain if there will be any reaction—if carbon will separate—and if so, whether it will be crystalline or not, i.e. whether it will be diamond.

7375. Observed that liquid Carbonic acid is heavier than Sulphuret of carbon—much; also that volume of sulphuret of carbon is increased, as if it could dissolve a certain amount of carbonic acid but no more—for the line of demarcation is very sharp. The refractive power of the sulphuret of carbon is enormous as compared to the carbonic acid. The part of the tube containing the latter does not differ much in refractive effect from the part filled with carbonic acid vapour at about 45 atmospheres, *above* the sulphuret of carbon; but the part between, containing the sulphuret of carbon, is in striking contrast with the portions above and below.

7376. When cooled in the E. C. A. bath, the carbonic acid freezes at -70° or there away. Then Carbonic Acid freezes *out of the sulphuret of carbon*—leaving the latter to flow freely in its fluid state—but if the freezing be done hastily, then the crystals of C. A. in the sulphuret of carbon keep the latter among them, and it seems as if the sulphuret were also frozen.

7377. When the carbonic acid is frozen out, if the tube be inverted so as to let the fluid sulphuret run down on the warm glass, it boils freely, from the further evolution of Carbonic acid—shewing that some is dissolved at the lowest temperatures and much more when the Carbonic acid is allowed to assume the liquid state. From the refractive condition of the liquid C. A., it would not seem as if it had dissolved much Sulphuret of Carbon.

7378. *Carbonic acid.*

This morning—after lecture in the Laboratory, whilst the air was very warm, the tube (7300), like that of 10th Jany. and put asid[e] about the same time, exploded, i.e. it blew the cap off; but the tube did not burst or break in any part and it was found that the carbonic acid had gradually softened and disintegrated the Cement, for the tube was as if pulled out and the cement surface was rough and dull, like that which in other cases has been left when, by taking off the pressure, the gas condensed by the cement has escaped again, and left it a dry brittle powdered substance. Has however stood a very long time.

7379. *Nitrous oxide.* Data of 7365 arranged in a curve etc. as on former occasions.

		atmospheres
	−125°	1·00
√	−121°	1·09
	−120°	1·10
√	−118°	1·15
	−115°	1·22
√	−114°	1·24
√	−110°	1·37
√	−106°	1·53
	−105°	1·55
	−100°	1·77
√	− 99°	1·82
√	− 98°	1·86
	− 95°	2·03
	− 90°	2·34
√	− 86°	2·64
	− 85°	2·70
√	− 82°	2·96
	− 80°	3·11
√	− 79°	3·22
	− 75°	3·58
√	− 70°	4·11
	− 65°	4·70
√	− 60°	5·36
√	− 58°	5·64
	− 55°	6·09

		atmospheres 30 inches
√	−50°	6·89
	−45°	7·76
√	−40°	8·71
√	−38°	9·15
√	−35°	9·74
	−30°	10·85
√	−27°	11·56
√	−25°	12·04
	−20°	13·32
	−15°	14·69
√	−13°	15·30
	−10°	16·15
√	− 7°	17·15
	− 5°	17·70
√	− 2°	18·75
√	− 1°	19·03
√	0°	19·34
√	4°	20·70
	5°	21·07
√	8°	22·25
	10°	22·89
√	14°	24·30
	15°	24·80
	20°	26·80
√	22°	27·55
	25°	28·90
	28°	30·20
	30°	31·10
√	33°	32·40
	35°	33·40

7380. *Olefiant Gas* (7369).

Arranged the data 7369 as a curve—the following are the numbers.

−105°	4·60	−45°	12·23
√ −103°	4·70	−42°	13·15
√ −102°	4·75	√ −40°	13·46
−100°	4·82	√ −39°	13·75
√ − 96°	5·05	√ −38°	14·16
− 95°	5·10	−35°	14·79
√ − 93°	5·23	−30°	16·22
− 90°	5·44	√ −28°	16·86
− 85°	5·84	√ −27°	17·18
√ − 83°	6·00	√ −26°	17·44
√ − 82°	6·12	−25°	17·75
− 80°	6·32	−20°	19·38
− 75°	6·89	√ −16°	20·80
√ − 71°	7·43	√ −15°	21·11
− 70°	7·55	√ −14°	21·45
√ − 69°	7·73	√ −13°	21·82
− 65°	8·30	−10°	22·94
− 60°	9·14	√ − 6°	24·44
√ − 57°	9·76	√ − 5°	24·87
− 55°	10·07	0°	26·90
− 50°	11·10		

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7381. Some ether put into a dry bottle containing air at the temperature of 58° F. and shaken till atmosphere saturated—at 58° F. the Elastic force of Ether vapour is 11·4 inches mercury nearly, so 30 inches of the mixed atmosphere would contain 11·4 inches of Ether vapour, or 1 vol. 0·38 of vol. Ether vapour.

7382. A cubic inch of this air was conveyed by a syringe into a jar over mercy. containing 370 c.i. of common air, and left for an hour or more to mix perfectly. The air was then very etherial to the smell—and by that test contained as much ether as other air which had been saturated and was then washed with its volume of water. Yet the bulk of ether vapour in the air was only 1 vol. in 974 volumes.

7383. When the quantity of Ether vapour was reduced to half this quantity, or only $\frac{1}{2000}$ part, still the Ether smell was very sensible.

7384. *Air and Ether.*

33 Vols. of air were enlarged to 36 vols. by the addition of Ether vapour. This air, being agitated with 3 vols. of water, had so much ether vapour taken away as to be reduced to $33\frac{1}{2}$ vols.—by a second washing with 3 vols. of fresh water, the bulk was reduced to $33\frac{1}{4}$ vols.—and by a third washing to still less. The air was then washed in a still larger proportion of water, and afterwards found to measure $33\frac{1}{8}$ vols., from the separation of air from the water and its addition to the measured air.

7385. 50 vols. of air were expanded by Ether vapour to 70 vols., and then washed once in about 10 vols. of water—then being washed by agitation with 50 vols. of water. The bulk was at once reduced to 50 vols., there being no sensible enlargement from ether vapour. Yet the air contained a little, and enough to give it a smell as strong as that above.

7386. *Solubility of Olefiant gas.*

In *water*—is as Berzelius (*vol.* i, 332) says, soluble in it—15 vols. of Olefiant (which had been well washed by shaking with its bulk of fresh water) were agitated with 95 vols. of water and left 6 vols. These with about 60 vols. of fresh water left 4 vols., so that 11 vols. had been dissolved. When this residual gas was burnt, it burnt with a blue flame like hydro carbonate, and very different to an equal bulk of Olefiant gas.

7387. *Olefiant in oil of Turpentine.*

4 vols. of oil of turpentine were agitated well in 90 volumes of olefiant gas (washed as above in its bulk of water) and then, being opened over oil of turpentine, quickly the absorption was noted; it equal[led] 10 vols., so that 1 oil of turpentine had dissolved $2\frac{1}{2}$ vols. Olefiant gas.

Again, 6 vols. oil of turpentine in 92 vols. of Olefiant absorbed 12 vols.—or 1 vol. oil turpentine condensed 2 vol. of the gas.

Again, 6 vols. oil turp. in 96 vols. of gas absorbed 14 vols.—or 1 vol. oil turpentine absorbed 2·33 vols.

On the average, 1 vol. camphine condenses 2·27 vols. at the pressure of   inches barometer and temperature of 60° F.

7388. Took a portion of the washed olefiant gas and dissolved away one half of it by camphine—then experimented on it to ascertain what proportion of it would dissolve in camphine.

5·5 vols. of camphine dissolved only 8·5 vols. of this gas at common temperatures and pressures, i.e. 1 vol. camphine dissolves 1·54 vols. of the gas. This is much less than before—yet if more camphine was taken, three fourths of the undissolved portion of gas could be taken up. This seems to shew that, in the part of olefiant gas soluble in oil of turpentine, there is not one substance only, but two or more.

7389. Again, dissolved out two thirds of Olefiant gas by camphine and then ascertained the solubility of the remainder in camphine, having always abundance of the gas (20 times for instance in volume) for the bulk of camphine employed. Now 5 vols. of camphine dissolved only 5 vols. of the gas, or 1 vol. camphine 1 vol. gas.

Hence at first . . .	1 vol.	Camphine	dissolved	2·33	vols. gas.
But when richer part removed	1 „	„	„	1·54 „	„
and when still more removed	1 „	„	„	1·00 „	„

This can only be accounted for by the presence of various compounds in the soluble portion of the gas.

7390. *Solubility of Oleft. gas in Ether.*

First some of the washed Olefiant gas was expanded by vapour of ether, and may assume that it then consisted of 62 vols. Olefiant and 38 vols. vapour of Ether. 5 vols. of liquid ether agitated in 98 vols. of this mixture absorbed 16 vols. = 10 vols. nearly of the gas. Hence ether absorbs about twice its volume of the richer part of the gas.

7391. This solution, being mixed with 8 or 9 times its volume of water, the ether dissolved and minute bubbles of gas gradually separated. By applying a little heat, this was hastened, and at last gas to the amount of about half the volume of that dissolved separated. This burnt like very rich Olefiant gas.

I did not try ether with the partially exhausted Olefiant gas, as in the case of oil of turpentine.

7392. *Solubility of Olefiant gas in Strong Alcohol.*

First the Olefiant gas was saturated with the vapour of the Alcohol, and probably then contained 1 vol. Alcohol vapour in 25 vols. of the mixture. Then 5 vols. fresh alcohol caused a condensation equal to 10 vols., or 1 vol. such alcohol can absorb 2 vols. of the rich part of the gas.

7393. 3 vols. of this solution mixed with 21 vols. of water—very little appearance of separation of gas—heated gradually, but still very little separation—at last, when boiling, above 3 vols. of gas (at common temperatures) was obtained, which by burning was olefiant gas.

7394. 1 vol. of the same alcoholic solution was mixed with only 2 vols. of water—the gas came off more easily, but not more than half the gas was obtained.

7395. *Olefiant Gas and Olive oil.*

Olive oil dissolves nearly its volume of olefiant gas at common temperatures and pressures.

1 MARCH 1845.

7396. Made some light hydro carbonate by Dumas' process. Tried its solubility in Absolute Alcohol; Ether; and Camphine. If it had any solubility in the two first, it was scarcely sensible. In Oil of turpentine or camphine it seemed to be a little soluble, perhaps 1 vol. in 14 or 15 vols. of Camphine. The difference between it and Olefiant gas was very striking.

8 MAY 1845.

7397. *Olefiant Gas.*

Profr. Graham sent me some Olefiant gas made by continuous process—it was brought here on   and was in glass stoppd. bottles with a little water in each, and the stoppers tied over, one with caoutchouc, the other with bladder. Both were put into water, stoppers downds. and left until now. To-day they were examd.—the one with bladder stank and water had entered the bottle—and so the state of the gas being uncertain, I threw it away. The other was in good condition.

7398. This gas was examd. by solution in Camphine (   ) as before. In three experiments, 100 parts of the original gas left undissolved 21·1, 20·7, 21·1 parts, and this burnt with a pale flame, in appearance like that of light hydrocarbonate.

7399. The same gas in two experiments left undissolved, out of 100 parts, 31·2 and 31·5 parts.

7400. So this Olefiant gas contained twice as much insoluble gas as the best portions of that obtained in the ordinary way.

17 MAY 1845.

7401. Put certain substances into the E.C.A. bath in Vacuo to cool to lowest temperature, and then brought them near a delicate magnetic needle. The following bodies were not magnetic at common temperatures—nor at the low temperature of −166° F.

Platinum	Fusible metal	Carb. Iron—native
Gold	Speculum metal	Prussian blue
Silver	Plumbago	Green vitriol crystd.
Palladium	Retort (gas) carbon	Calomel
Copper	Kish	Chlo. silver
Tin	Orpiment	,, lead
Lead	Realgar	White arsenic
Cadmium	Sulphuret Antimony	Prot. oxide Anty. fuzed
Zinc	,, Bismuth	,, ,, lead ,,
Rhodium	,, Copper	Oxide bismuth ,,
Chromium	,, Iron Per.	Oxide Tin—*native*
Titanium	,, lead	,, Manganese—*native*
Iridium and osmium	,, silver	
Antimony	,, tin	
Arsenic		
Bismuth		

7402. *Nickel*—Percy's pure, prepared by Mr Askin of Birmingham
Cobalt Do. Do.
Electrum or white copper
Iron—also micaceous oxide of Iron

were magnetic at common temperatures and also at the lowest temperature. All these bodies, being held in the line of the dip, become polar as Iron.

7403. *Iron* surrounded by jackets of *Copper*, *Tin*, *lead*, *zinc*, *silver*, *platina* and then cooled to the lowest. No obstruction was presented by the cold jacket to the inductive action of the magnet through them.

7404. *Manganese*—prepared by Mr Thomson—was not magnetic either at common or very low temperatures.

22 MAY 1845.

7405. The following were the effects of heat on Nickel, Cobalt and Electrum.

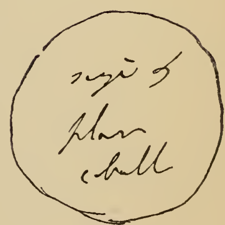
7406. *Nickel.* My bar and also the large button given me by Mr Askin of Birmingham, both lost their power on a needle when heated in a spirit lamp flame and very far below a visible red heat. Also a half button of magnetic metal which proved to be nickel did the same.

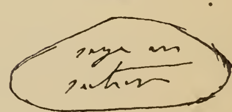
7407. *Electrum*—a plate of this metal, Fischer—was nearly as Nickel.

7408. *Cobalt*—neither my button, nor that belonging to Dr Percy, could be made hot enough in blow pipe flame to loose their Magnetic power on a needle. When hung in copper wire and heated in the furnace until the copper was nearly melted, still it was magnetic, whilst a piece of Iron hung and heated in the same manner lost all magnetic power at a red heat far lower in temperature than the Cobalt.

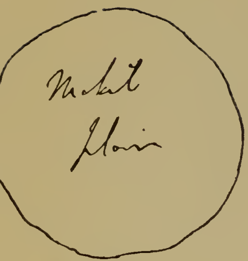
7409. Being put into a crucible with some borax and heated more highly (as highly as our Laboratory furnace could readily make it hot), it did then lose all magnetic power, and as it fell in temperature, it suddenly regained its power; but this temperature was very high—higher much than that requd. for Iron.

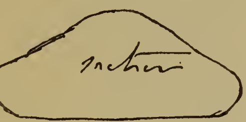
7410. *Cobalt.* The button of cobalt[1], being put into a helix made round a card and adapted to its shape, had an Electric current





[1] The figures to pars. 7410 and 7411 are reduced to $\frac{3}{4}$ scale.





of 4 Grove's cells sent round it, and then became an excellent magnet. It attracted a large bunch of filings, and could sustain a button of nickel heavier much than itself. I could not make out distinctly the magnetic sound.

7411. *Nickel.* The button of nickel in the helix also became a good magnet, picking up a large bunch of filings and taking up the button of cobalt freely. I could not determine clearly whether it produced the magnetic sound or not. It was hardly to be expected from such short lengths of materials, for the sound is alway[s] the note of the longitudinal vibration of the mass.

28 MAY 1845.

7412. *Manganese* from Marsh. Metallic grey globule, remaining pretty bright in the air and looking rather like highly carburetted Iron. In dilute Mur. acid it slowly dissolved, evolving hydrogen, leaving portions of silica and giving a solution principally a chloride of manganese, but containing a little iron. Some of the metallic looking plates, which were very light and would not dissolve, looked like Kish and were perhaps Carburet of Manganese.

7413. *Manganese* on charcoal before Oxygen blow pipe burnt like iron with sparkles, each sparkle burng. with further sparkling just like iron—but the oxide produced was fusible and bulky and frothy, and quickly stopped as a flux the intensity of combustion. Stopped the combustion before all the manganese was burnt, and when the whole was cold, the protoxide formed was white or nearly so—fused—brittle, easily breaking off from the metallic manganese. This white globule was not magnetic.

7414. This white globule being again submitted to heat and oxygen on charcoal, frothed, swelled and then shrunk into a globule, quietly fuzed at the bottom of the charcoal hole, which was much smaller than the white globule, dense and black. When cold it was hard—but broke with a close vitreous fracture into shivers of a dark brown and translucent glass. This was, I believe, a true oxide of manganese. It appeared to be slightly magnetic, but very little so.

7415. The piece of manganese left above ( ), being burnt, produced first the puffy oxide and then the dense hard black globule—also slightly magnetic.

7416. The manganese alone was slightly magnetic; perhaps the trace of Iron present was enough to account for this.

7417. *Nickel* on charcoal before oxygen burns, with some spluttering but quietly on the whole, and not as Iron or Manganese. It left a globule—dark gray—brittle—and looking rather like a mixture of fuzed oxide and metallic nickel—and I think that contact with the hot charcoal below reduced part of the oxide, and that then, as the metal comes uppermost, it is burnt again. The globule is very magnetic—but then there is no assurance as yet that the metallic nickel does not make it so.

7418. *Cobalt*, filings on charcoal before jet of oxygen, melts, burns feebly and produces a globule—*not magnetic*—very brittle—breaking with a crystalline fracture, and being fuzed oxide of cobalt.

12 JUNE 1845.

7419. *Carbonic acid and Sulphuret of Carbon.*

The tube sealed up 14 Feb. (7374) had undergone no change by the 6th of this month. On the morng. of that day, put it out on the house top on the lead, covered over with a bell glass and exposed to light. On the 7th, no change—the 6th had been a sunny day. On the 10th, it was found exploded and the jar shattered. I think that the sun had heated the leads and the lead the contents of the jar. If the tube had been hanging freely in the air away from the leads or wall, probably it would not have become hot enough to burst (see also 7272).

1845. AUG. 18TH.

7420. *Nitrous oxide.*

Examined the bottles of Ns. oxide made of pure Nitrate of Ammonia (   ) and found several perfectly closed and the gas excellent. So made experiments on its solubility in different fluids in hopes of finding a useful solvent.

7421. *Alcohol*—absorbs this gas moderately freely and easily took up $\frac{7}{8}$ of the volume of gas—the $\frac{1}{8}$ left supported combustion better than air and more like nitrous oxide, and I found that when transferred to fresh alcohol, not more than half of it or about $\frac{1}{14}$th of the original gas remained, and this appd. to be air.

7422. *Water*—dissolves Ns. oxide but not so freely as alcohol does; however, by displacing the saturated water by fresh water, not more than $\frac{1}{16}$th of the gas was left and that was air.

7423. *Strong Acetic acid*—dissolves a little of the gas but is in that respect much inferior to water.

7424. *Solution of Oxalic acid*—dissolves a little more gas than Acetic acid but is not so good as water.

7425. *Solution of Oxalate of Ammonia*—about as oxalic acid.

7426. *Strong solution of Ammon.* diluted by its volume of water—was not so good a solvent as water but better than the last three fluids.

7427. *Sol. of Caustic potash*—as feeble in solvent power as Acetic Acid.

7428. *Dilute Sulphuric acid*—as the last.

7429. *Ether*—dissolved the gas more rapidly than Alcohol—it left a large bulk of residue but that appd. to be due first to the stock of gas over water having now obtained more air from the water, and next that that residual gas was expanded by vapour of Ether—for Alcohol and then water reduced residue nearly to common air.

7430. *Camphine.* Dissolves the gas as freely as any of the former bodies.

7431. So the order of these solvent[s] is the following, Camphine being the best—

Camphine	Oxalic acid solution
Ether	Oxalate of Ammon. Do.
Alcohol	Strong Acetic acid
Water	Dilute Sulphuric acid
Sol. Ammonia	Solution of Caustic Potash;

but none very useful.

7432. *Nitrous oxide.* In obtaining the pressure at the lowest temperatures, plenty should be condensed and then let off in successive portions at the lowest temperatures, observing at the same time the *lowest tension*—and observing also if on standing it gradually rises again. At the first moment should have nearly the pressure of Nitrous oxide, allowing an instant for the rise of temperature to compensate the fall by evaporation within.

7433. *Flexibility of Glass under pressure.*

I had an impression that, in some of the cases where the glass condensing tubes (in the gas experiments) were under the influence of high internal pressure, they were more flexible and bent more than when the pressure was lowered. Therefore have to-day made an experiment on purpose with a narrow tube, long, etc., and observed the position of the free end both when the pressure within was 1 atmosphere and 20 atmospheres—but I could perceive *no difference*—and believe that my impression was not correct.

30 AUG. 1845.

7434. *Polarized light and Electrolytes.*

I have had a glass trough made, 24 inches long, 1 inch wide and about $1\frac{1}{2}$ deep—in which to decompose electrolites, and whilst under decomposition, along which I could pass a ray of light in different conditions and afterwards examine it. 12 or 14 fluid ounces of liquid filled this trough to a convenient height.

7435. Two pieces of plate paper about 14 inches long and 4 inches wide were folded length ways down the middle and then put into the trough, in which they formed an inner v trough reaching from [one] end to the other; they overlapped in the middle.

7436. Two platina electrodes about $\frac{3}{4}$ of an inch wide, one 20 inches long and the other 15, were introduced on the outside of the paper trough, between it and the glass, one on each side, and these were afterwards connected properly with a Grove's Battery or with other electric apparatus.

7437. *Saturated solution of Sulphate of Soda* was poured into the trough to the depth of about $1\frac{1}{4}$ inches, and of course permeated the paper and connected the platina electrodes.

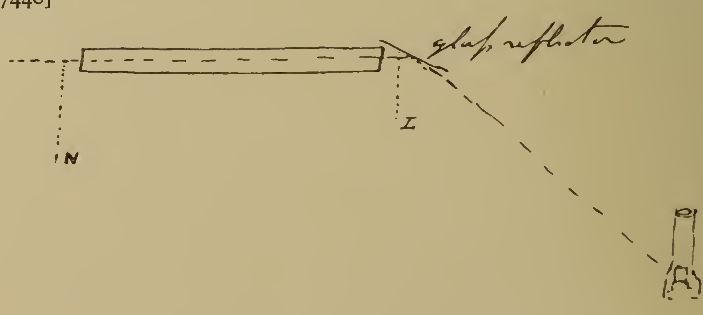
7438. In this way the electric current could be passed across the trough, and at the same time the centre of it was preserved perfectly clear for the passage of light.

7439. The Voltaic apparatus used was a Grove's battery of five pair of plates. By the aid of a magnetic breaker and its associated set of 2 coils of covered wire and an iron wire core—either the *continuous current* of the battery—or the *continually intermitting current* of the battery—or the *rapidly recurring series of double currents* obtained by induction from the helices, could be sent at pleasure across the electrolyte in the trough.

7440*. The polarizing apparatus first used was a glass plate blackened at the back surface so as to reflect chiefly from the first surface. This was so placed as to polarize the light of an Argand lamp in a horizontal plane, and then send it along the glass cell. At the other end of the cell was a Nicholl's[1] eye piece in a revolving

[1] Evidently for "Nicol's". Faraday's spelling of this name varies considerably. See pars. 7498, 7550, 7577, 8642.

* [7440]



socket with which to examine the ray and any effect the liquid in the cell might have upon it. The polarization was good and the apparatus good.

7441. With *no electric current* the polarized ray was not affected.

7442. With an electric current across the cell, there was no effect on or change of the polarized ray.

7443. When the current began or when it stopped, there was no sign of change.

7444. When the intermitting (primary) current was sent through, there was no change.

7445. When a plate of sulphate of lime was put in the polarized ray before it entered the electrolyte (making the ray red or blue), there was no effect in further modifying the ray by the electrolyte under any of the previous conditions of the E. current.

7446. Keeping all other things the same, the polarized ray was turned on its axis 45°, and afterds. 80°, and then sent through the electrolyte and examined under the different conditions of the Electric current, but still all was *nul.* No effects were observed of the Electrolyte on the ray.

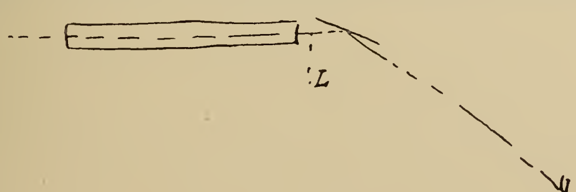
7447. I made the coming on and going off of the current gradual by interposing in the Electric circuit a glass containing a strong solution of Sulphate of Soda, causing one of the metallic terminations in it to be a metal plate and the other a fine wire; by dipping this end in and out, *time* was given to the full establishment or rising up of the current—but no sensible effect on the polarized ray could be perceived.

7448*. I placed a tourmaline between the end of the cell and the polarizing reflecting plate (at L) in such a position as to polarize in a vertical place, so that now no light could be expected to pass into and along the trough or electrolyte; still, under the possibility that any rays might be there, the electric currents already spoken of were sent through the electrolyte and examination made at the other end of the tube by the Nicholl's eye piece, by a plate of sulphate of lime, and by the naked eye, but nothing could be perceived.

7449. Sent a ray polarized by a tourmaline along the Electrolyte with all the varieties of Electric currents. Perceived no effect.

7450. Sent a common ray along the electrolyte—then polarized

* [7448]



it by a tourmaline and examd. it by Nicholl's eye piece. No difference occasioned by the presence of the electric currents or absence.

7451. In all these experiments the Electric currents in the Solution of sulphate of soda had been *across* the course of the *ray of light*. Now made them go parallel to it, and for this purpose used two platina electrodes not more than $1\frac{1}{2}$ inches Square or perhaps 2 inches, putting one of these on one side of the paper trough at one end of the glass trough, and the other on the other side of the paper trough at the other end of the cell. The electric current was very much diminished by the introduction of this great length of Electrolyte, but then there can be no doubt that what did pass was for 20 inches or more parallel to the course of the ray of light.

7452. With none of the forms of Electric current, constant or intermitting or secondary, could I obtain any effect on the polarized ray.

7453. Shortened the distance of the Electrodes to 12 inches—still no effect though a much better current.

7454. Shortened the distance to 4 inches—still no effect.

7455. Put the two electrodes on the *same side* of the paper trough so as to have *no crossing* of the line of decomposition—still no effects.

7456. Again, introduce plate of sulphate of lime at L (7448)—still no effects.

7457. In reference to the force of the intermitting secondary current, tried its effect on the hands and body and found it terribly powerful. So that even at the greatest length of Electrolyte it must have passed well—yet no effect.

7458. Here therefore give up Sulphate of Soda solution and next try dilute Sulc. acid. Perhaps there may be some difference between an electrolyte yielding acid and alkali, and another yielding oxygen and hydrogen. The dilute Sulphuric acid consists of 1 vol. oil vitriol and 3 vols. water.

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7459. *Dilute Sulc. acid*, i.e. 1 vol. acid + 3 vols. water, with the same trough and arrangements as before.

7460. With Long Electrodes (7436), so as to send the Electric current across the course of the ray. There were no effects on the ray, either when the current was *constant* (7439) or *intermitting* (7439), or with powerful *recurring secondary* current (7439), the ray through the electrolyte being once polarized by reflection from glass beforehand.

7461. When the tourmaline was placed at L (7448) so as to polarize the second ray, or rather stop it, nothing took place either with constant or intermitting primary current or with the recurring secondary current.

7462. *Sul. of Lime* at L, so as partially to affect the beam passing through the electrolyte—no effect with the variations of the electric current.

7463. Common beam of light through the electrolyte and this examined by Nicholl's eye piece or tourmaline or both—but no effect on passing the various electric currents.

7464. Now used *smaller platina electrodes*, putting one at each end on the *same* side, and on *opposite* sides of the paper—and putting them also at half way and smaller distances—thus passing the current *along* the course of the light ray. Also made all variations of the ray and also of the electric current, but obtained no effects.

7465. Now put one of the platina plate electrodes in the middle of the fluid, so as to look close along it in examining the ray, to see if there was any effect at the surface of the electrode or *very near* to it. Used also a platina wire held sometimes across the ray and sometimes along it, and examined these under the condition of a passing electric current, but could find no effect on the polarized ray of light. Except this, that when the current passed, the ray was obscured by the rising bubbles of gas evolved. No affection of the electrolyte in relation to the ray of light was produced.

7466. *Sul. copper*. This solution consisted of 1 vol. saturated solution of sulphate of copper and 1 vol. water. Plenty of blue light passed through and had the usual properties.

7467. All the variations just described (7442, etc.) were made with this solution, using always copper electrodes on both sides, but not the slightest trace of action on the polarized or common ray of light, referable to the current of electricity, could be perceived.

7468. *Distilled water.* The object was to see if the high intensity of the secondary current could produce any condition on this badly conducting electrolyte, for the perception of any state preparatory to decomposition might perhaps be expected to manifest itself here. The cell was made very clean and all was in good order. But no effect on the polarized ray or any other ray could be perceived—under any of the variations of current.

7469. Used also the constant primitive current—but then very little electricity passed.

7470. *Solution of Sugar*, i.e. 1 Syrup + 2 water. The light was very obscure through the two feet of length. I obtained effects of circular polarization, but not the Slightest signs of any influence of the Electric current.

7471. From all these experiments I am led to conclude, as I had reason to do long ago (Expl. Researches 951, etc.), that an Electrolyte under decomposition has no condition impressed upon it in relation to polarized or ordinary light, different to its condition when not under electrolyzation—and further, that any change which its internal constitution may suffer in that respect cannot be examd. or reached by a polarized ray.

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7472. Arranged certain electrolytes so that platina wires should be the Electrodes, and the space between (preserved clear for the light to pass by a little wedge of paper) rendered as small as possible, perhaps the $\frac{1}{8}$ of an inch. The wires were also so placed that occasionally the course of the ray was along the electric current and at other times across it.

The different electrolytes were:

7473. *Distilled water.*

7474. *Sul. soda*, saturated solution.

7475. *Dilute S.A.*, 1 vol. oil vitriol + 3 vols. water.

7476. Saturated sol. *Sul. copper.*

7477. The Electric current was applied as a constant current—as a beginning current—as a ceasing current—as an intermitting current—and also as a rapidly recurring secondary current.

7478. The light passed through the electrolyte was polarized in a horizontal plane and occasionally partially depolarized by sul. lime—and it was examd. by a Nicholl's eye piece.

7479. But *no effect* on the electrolytes in any of these cases could be perceived.

7480. A *Plate of Glass*, 2 feet long and about $\frac{1}{6}$ of an inch thick, had tin foil coatings attached to its two sides, and then these were connected with the ends of the secondary coil—so that a rapidly recurring and strong secondary current *tended* to pass through the glass. Polarized light was sent along the glass plate through it and examined, but no effects were produced.

7481. *Air.* Two tin plates 22 inches long were arranged at about $\frac{1}{12}$ of an inch distance and so fixed and attached as just described to the secondary coil. Polarized light was sent along the air between them, and the secondary current or its tendency thrown on to the air—but no effect on the ray was observed.

7482. *Oil of turpentine.* The tin plates were put into a trough of oil of turpentine, and the experiment repeated—but the secondary current which tended to cross the oil of turpentine gave it no power over the polarized ray.

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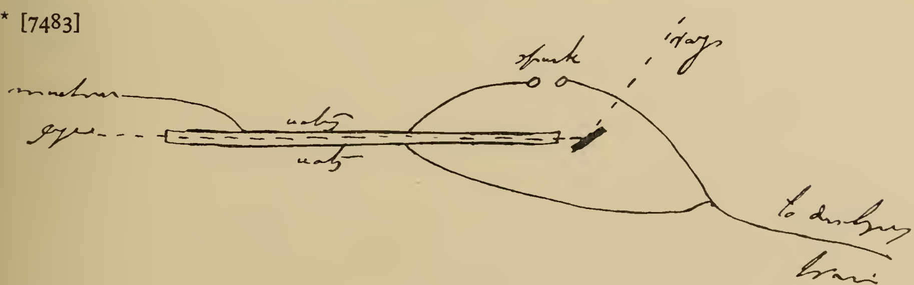
7483*. Now worked with common Electricity in this way. Applied coatings either of tin foil or plates of tin or other metallic forms on to plates of Glass or of air or oil of turpentine, etc. etc., and arranged one of these coatings with the discharging train and the other with an Electric machine, so as to charge the interposed body. Then by 2 balls at different distances the discharge could take place continually on arising to a certain degree of intensity, and be again renewed, to be again discharged. The following are results with spark discharges from $\frac{1}{4}$ to half an inch or more of interval. Consequently the intensity in these cases was far higher than any the former arrangement of apparatus could give, but there was no decomposition. Therefore any effect previous to decomposition and due to that previous state acquired might be here observed.

7484. *Glass*—a plate 4 feet long—and another about 2 feet long, each about $\frac{1}{6}$ of an inch thick, were thus treated and the polarized ray passed along them and examined, but there were *no effects*.

* [7483]



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.

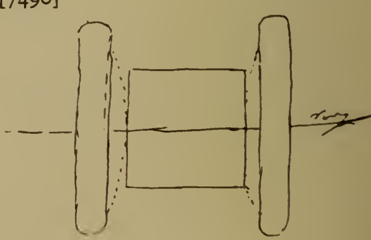
7492. *Oil of turpentine* in the long glass trough (7434). The light was passed *along* and *across* the lines of inductive action, but with no effect.

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 $\frac{3}{4}$ 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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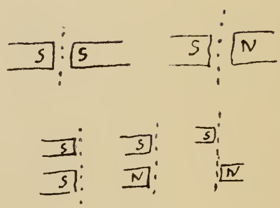
7498. To-day worked with lines of magnetic force, passing them across different bodies (transparent in different directions) and at the same time passing a polarized ray of light through them, and afterwards examining the ray by a Nichol's Eyepiece or other means. The magnets were Electro magnets, one 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.



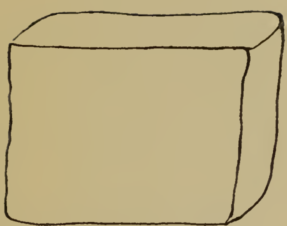
7500. *Flint Glass.* A piece of flint glass about 2 inches square and half an inch thick, polished on all sides, was tried with all the above variations of conditions (7499), but with no effect. It was also shaken so as to move it, and it was tried in all positions, but in vain. It was very full of striæ, 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 ¾ 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.

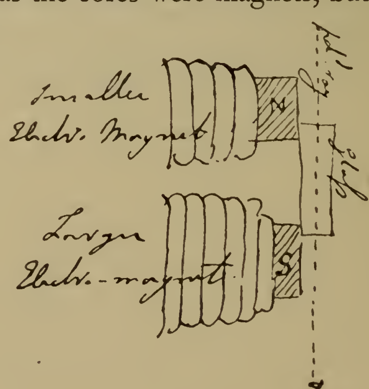
7504. *Heavy glass.*

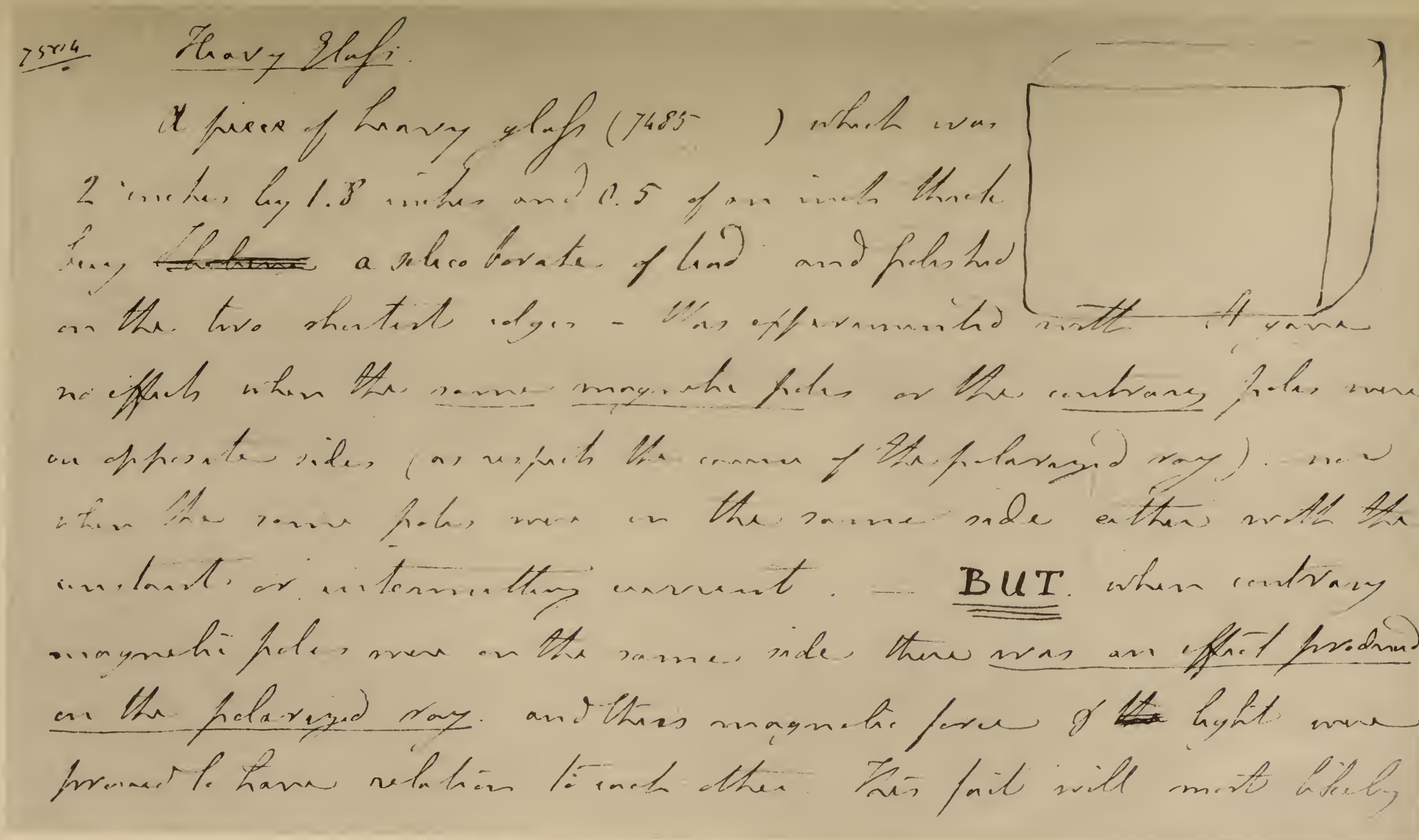


A piece of heavy glass (7485) which was 2 inches by 1·8 inches, and 0·5 of an inch thick, being a silico borate of lead, and polished on the two shortest edges, was experimented with. It gave no effects when the *same magnetic poles* or the *contrary* poles were on opposite sides (as respects the course 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. 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

The entry recording the discovery of an effect of magnetism on light. September 13, 1845. Par. 7504 (*full size*)

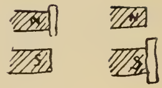
instant the electric current was stopped. Hence it was a permanent condition, and as was expected, did not sensibly appear with an intermitting current.

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.

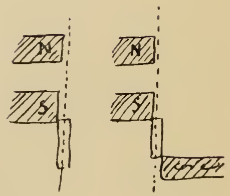
7509. When the heavy glass was opposite the end either of N or S, as thus or thus, then the effect was much less than when it was as figured above (7508).



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).

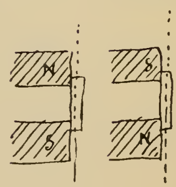
7511. By the Negative results obtained with the crystalline cubes (7502, 3) with the present degree of magnetic force, it would appear that crystals have no special power in one direction more than another. But this will want investigation with more powerful magnetic forces.

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.

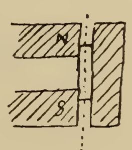


7513. When the heavy glass was in this position, there was no sensible effect on the polarized ray, but when a core of soft iron was held at the outer edge of the glass, so as to determine a certain amount of magnetic curves through it, then there was a trace of effect.

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

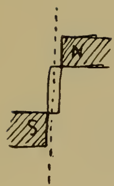


* [7508]



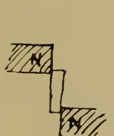
one would rather expect *some difference* in the condition and therefore in the result of things; though the amount of action might be equal, a difference in kind may perhaps be expected.

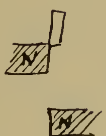
7515. Now again (7504), put the same Mag. poles on the same side, as thus*, but could not perceive any effect on the ray of light. When the heavy glass was placed thus†, then I thought I could perceive a very small effect.



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 position§, then the effect was as good as ever.





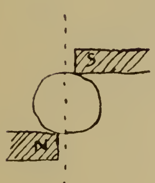
7518. When the same magnetic poles were at opposite ends of the heavy glass, there was no effect on the polarized ray, but when the glass was moved on to the edge and beyond one of the poles, then there was a little action.

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 $\frac{3}{4}$ 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.

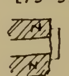
7524. *Oil of Turpentine*—shewed its own rotating effect on the polarized ray but *nothing* in addition.

7525. *Distilled water*—as above (7523). No effect.

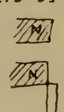
7526. *Dilute Sulc. acid*, 1 Oil vitriol + 3 water—no effect.

7527. *Sat. solution Sul. Soda*—nothing.

* [7515]

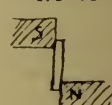


† [7515]



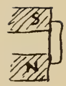
‡ [7517]

§ [7517]



7528. Sat. solution *proto sul. iron* with a little sulphuric acid to render it clear—nothing.

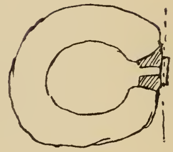
7529. Now went back to some of the other substances to retry them in the favourable condition, as thus.



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.

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7537. See as to Magnetic induction, etc. through *crystals*, 4919–4924; and for the crystals, 4657–4885, especially 4773.

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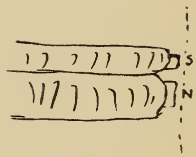
7538. Have now borrowed and received the Woolwich Magnet, a cylindrical Electro-magnet far more powerful than ours. When in action it holds easily a half hundredweight at each end of the core, and almost a second half hundred besides. This magnet and ours were arranged thus, and excited by five pair 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.

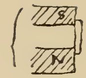
7543. When contact is broken of the electric current (7505), the image disappears far more suddenly than it rose. Shewing in some degree the state of tension in which the iron is held by the Electro current.

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.

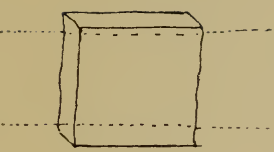
7550. I find that the *new quality or force* impressed on the heavy glass by the Magnetic curves is a *circular polarizing force*—for when without the Magnetic curves, the Nicholl eye piece is in that position which extinguishes the polarized ray—and when by inducing the Magnetic curves and peculiar 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 $\frac{1}{3}$ 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 $3\frac{1}{4}$ 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.

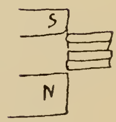
7568. *Glass.* Should be perfectly well annealed to shew this effect. Is there any real magnetic relation between this circumstance and the fact that iron when most annealed takes up best the Magnetic effect?

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.

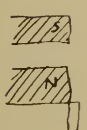
7577. I find that it is easier for the eye to distinguish the effect of the new power conferred on the *dimagnetic* when the image is (by the Nichol eye piece revolution) rendered slightly visible upon one side or the other of utter darkness. The Magnetic curves then cause increase or diminution of the 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.

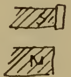
7579. The former best position still best with these powerful magnets (7538), and the effect excellent. When the image was made a little sensible by elevating the Nicholl eye piece handle, the Magnetic curves made it far more sensible.

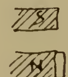
7580. When the glass was thus* or thus†, there was no sensible effect.

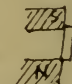


7581. But when carried on further on either side, i.e. beyond the S or the N pole, then there was effect. It was, however, the reverse of the former effect, for the image which was brightened by the glass in this position‡ was darkened by the glass in either of the positions just given.

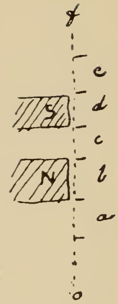
7582. If I brought in a faint image by raising the handle of the eye piece, and then made the glass travel from *o* to *f*, when at *a*

* [7580] 

† [7580] 

‡ [7581] 

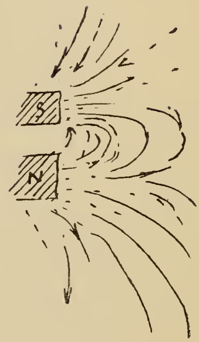
it darkened the image—when at *b* the image was in its natural state—when at *c* the image was brightened—at *d* it was natural and at *e* darkened again. Or, if I adjusted the eye piece so that there was no image in the normal condition, then the glass at *a* gave an image which, as the glass gradually 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.

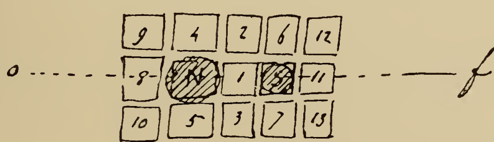


7586. So it is clear and consistent that, if the Magnetic poles were both changed, then the direction of rotation in the glass in its central or best position would be changed also.

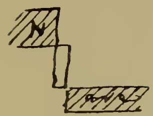
7587*. Now placed the glass in various positions, which may be easily understood by the figure, in which a front view of the ends of the Magnetic poles is given: *o f* is the line of the polarized ray, and the squares represent different places of the glass plate, through

[1] Query S.

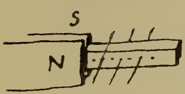
* [7587]



which the polarized ray always moved parallel to the direction given, *o f*. Found the following results: 1 the most effectual position; at 2 or 3 the effect was much less than at 1, but of the same kind; at N, S, 4, 5, 6 or 7 the glass was ineffectual, as might be expected; at 8 or 11 the glass was active, with a power contrary to that at 1, 2 or 3, and at 9, 10, 12, 13 the power, though weak, was very sensible and like that at 8 or 11.

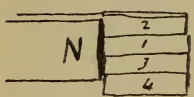


7588. The condition of the glass when outside either magnetic pole has been already stated. When, retaining this position, a long core of soft iron was put at the outer edge of it, its power was strengthened, not being altered in its character.



7589. By looking through the glass at different distances from the magnetic poles, it was found that the power of the glass rapidly diminished as the ray passed at a greater distance from the poles, i.e. as it went along Magnetic curves of weaker intensity and in a less favourable position. At the distance of $1\frac{1}{4}$ inches the glass was almost inactive with this magnet and arrangement.

7590. The same examination was made with one piece in the best position but removed to different distances from the poles; the power fell in proportion and at $1\frac{3}{4}$ inches the glass had little or no sensible effect.

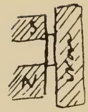


7591. When the pieces (4 alike) were placed thus, it was very striking to see how superior 1 was to 2 or 3, and these again to 4. This was expected, being a natural consequence of their position with respect to the magnetic curves.

7592. The poles were arranged thus, and the heavy glass in the best position. The figure being brought into view, the direction of the rotation was determined by ascertaining which way the Nicholl eye piece required to be revolved to compensate for the rotation produced. Then by altering the direction of the electric current, the magnetic poles were changed, and now it required exactly the contrary rotation of the eye piece to compensate for the rotation effect produced.

7593. So if in the normal state of the glass the Eye piece was adjusted to give a faint image, when the magnetic poles were made in one direction, that image was brightened; when they were made in the other direction, it was put out.

7594. When the heavy glass was in its good position and a large piece of soft iron put opposite the poles outside of it, there was a little diminution of power in the glass, but not much.

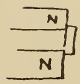


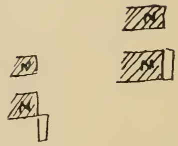
7595. The heavy glass as above and thick masses of lead or of copper put in place of the iron. No effect on the powers of the glass.

7596. A plate of copper $\frac{5}{8}$ of an inch in thickness *between* the heavy glass and the magnetic poles. There was no difference in effect whether the copper was there or away.

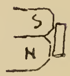
7597. Heavy glass in good position and then the magnetic poles sometimes connected by a mass of soft iron, and sometimes not. The glass was much better when the iron was away than when present, shewing the disposal of the curves by the latter when in place.

7598. Now made both the Magnetic poles the same in kind, but the Woolwich Magnetic pole was far stronger than our pole. The heavy glass in its best position gave an effect the same in character, but by no means so strong, as before. This is because the curves of the Woolwich N are so much stronger than the other N that they still go off towards it, though in smaller proportion than before. When the glass was at the end of Woolwich N, it had no effect, and when it was thus it had power, but the rotation was in the reverse direction.





7599. When the magnets were arranged in the best position but with the iron cores away, no sensible power was given to the glass. This shews how important the iron is in generating the magnetic curves, and how dependant they are on the iron. The same effect was shewn before by the gradual rising of the power (7541).



7600. It seems impossible to suppose that the electric current is the sole cause and *source* of this power. There must be an immense store in the iron before hand, which the electric current does not generate but merely direct. Shall perhaps be able to penetrate a little into this point by the optical mode of investigation.

7601. Worked with magnetic curves across the course of the

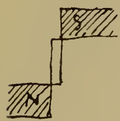
polarized ray by placing the electromagnetic poles on opposite sides of the glass: thus, but no effect.

" " no effect.

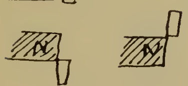
" " no effect.

" " no effect.

" " no effect.



7602. When the poles were thus, there was an abundant effect on the glass—but when S was brought opposite N, there was none. In the position depicted, the rotation was such that I had to *depress* the eye piece handle to correct it. But when the glass was thus*, there was also effect, though less, and I had to *raise* the handle to correct it.



7603. Experimented with *one pole* only, being the N pole of the Woolwich magnet, and had all the essential effects with it. Thus with the glass opposite the pole, it had no power. With the glass at one edge of the pole, it had the power of rotation in one direction—but with the glass at the other edge of the pole, it had the power of rotation in the contrary direction.

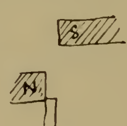
7604. Surely there must be a connection (direct) between this rotation of the ray and some tendency of electric or magnetic forces to rotate in the dimagnetic itself.

7605. *With a common Magnet.*

I used our strongest horseshoe magnet, and setting up a piece of heavy glass, approached the poles of the magnet and withdrew them again; then reversed the direction of the poles, approached them and withdrew them; and so on for some time.

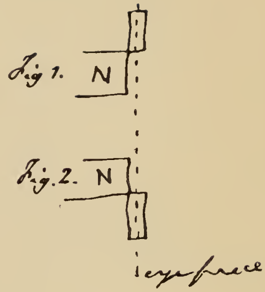
7606. There was a distinct influence upon the colour of the feeble image (7577, 7593) in these two positions, proving the effect of the magnetic curves of ordinary magnets, and also the production of rotation in two directions. The influence was very feeble, but still distinct enough for me to find out, by its apparent contradiction in direction with the effects of the electro magnets, that I had mistaken in them one pole for the other in certain of the experiments, and it led to their repetition and rectification.

* [7602]



7607. *Direction of the Magnetic curve and the rotation.*

This was determined by a single pole as being the simplest condition of things. First I made the image invisible by the right position of the Nicholl eye-piece. Then I rendered it a little visible by turning the eye piece a little to the left, or against the clock hand motion—then position Fig. 1 increased this brightness, i.e. turned the ray to the *right hand* or with the clock, and increased the difference between it and the position of the eye piece; whilst position No. 2 diminished the brightness of the image, i.e. it turned the ray *left hand* or against the hand of the clock, following indeed the previous left hand motion given to the eye piece.



7608. When the eye piece is in the dark position, and then the rotating effect of fig. 1 brought on, the image produced is darkened by turning the eye piece round watch hand fashion, i.e. the ray has been rotated to the right hand and the eye piece has to follow it. When fig. 2 is made effective, then the eye piece has to be revolved to the left hand, to overtake the ray which has been revolved in that direction.

7609. A specimen of oil of turpentine, being put into the course of the polarized ray, without of course any magnetic influence but merely by its own power, produced an image, which was reduced to darkness by turning the eye piece right hand fashion or with the clock. So oil of turpentine and glass as in figure 1 (7607) have the same direction of rotation.

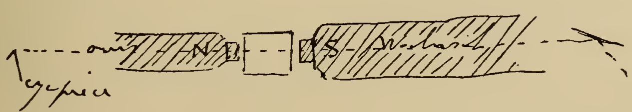
7610. A specimen of syrup in the course of the ray was also as oil of turpentine or as glass in fig. 1.

An excellent day's work.

19 SEPTR. 1845.

7611*. Have arranged the two large helices, ours and the Woolwich, with hollow imperfect cores, so as to be able to look through them and therefore directly along the magnetic curves going between the two adjacent magnetic poles. Put heavy glass between the adjacent poles. There was the expected effect when the magnetic curves were established, but not at all striking in amount. On the whole, this plan is much inferior to the former in effect and convenience. The cores were iron tubes, etc., and were not

* [7611]



good, but still were good enough to shew this was not a plan worth pursuing.

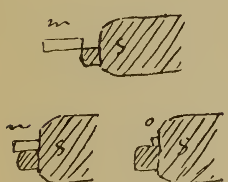
7612. In the position given, when the glass had acquired its power of rotating the ray, the eye piece required to be turned to the right hand or with the watch hands in order to restore the figure or condition existing without the glass; so this was a right hand rotation.

7613. Now used only one Magnet (the Woolwich) left in its former position. The heavy glass *174* placed before it edgeways gave very good effect. The rotation was as before (7612), i.e. it was necessary to revolve the eye piece to the right hand or watch fashion to put it right or compensate for the change. The glass had received right hand rotation.

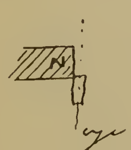
7614. The core in this case consisted of two good iron tubes, one within the other. Took out the inner one and again tried the glass. The effect was produced but much less than before.

7615. Took out the remaining tube or rest of the core, and now on trying the glass in the same position, the effect was only just sufficient to be perceived. The effect was however certain—so that either the helix alone, or the Electro magnet, or the ordinary magnet can produce the effect.

7616. When the glass was put *into* the helix, forming as it were a core in it, its power upon the light was not sensible as long as it was in the helix. On bringing it out in the direction of the helix action, it acquired a little power, and was best when just out, as thus—but even then the effect was very small.



7617. Put our iron core into the Woolwich helix—this allowed room for the piece of glass to go into the helix on the top of the core, and so it could be either as *m*, *n*, or *o*. In the position of *m*, the effect was very fair. In the position at *n*, it was very greatly diminished, almost nothing; and in the position of *o* there was no sensible effect. It was carried on into the middle of the core, but there was no effect produced.



7618. Now arranged the Woolwich magnet in the old way, so as to look across the end of its core (7607), and all the former effects returned—also as to direction, when the glass was thus, the eye piece had to be moved to the left or against direction of watch hands; so the rotation is here left handed.

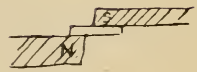
7619. When the glass was thus, then the eye piece had to be advanced watch hand fashion, i.e. the rotation was righthanded.

7620. *Air*: no effect could be perceived on air in any of to-day's experiments. But must remember that in air I should get the contrary effect at the outer edges of the magnets, and so could only have the difference, which is perhaps little or nothing.

Use a vacuum or other means.

20 SEPTR. 1845.

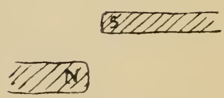
7621. Arranged the magnets thus* and found it very good, for I could approach the poles nearer and so strengthen the force of the Magnetic lines. Obtained an excellent effect even with the smallest thickness of the heavy glass.



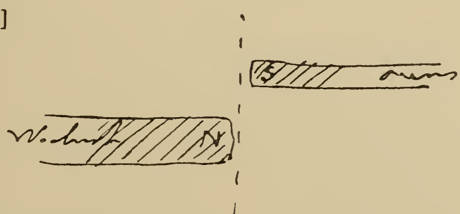
7622. *Flint glass plate* (7500) across; the effect was very distinct.
7623. *Crown glass* about half an inch thick, as above—effect obtained.
7624. Glass No. 10 (   ); very irregular in quality but still obtained the effect.
7625. *Tourmaline*—a thin polarizing plate placed in the neutral position so as not to affect the polarized ray. When the Magnetic curves were on I think I obtained the effect, but must get it again with the poles nearer.
7626. *Carbonate of lime*, *rhomboid*, and *sul. lime*, *crystal* and *plates*, in neutral position. No magnetic effect.
7627. *Iceland spar* and *Rock crystal*, *cubes* (7502, 7503); nothing (7558, 7559).
7628. *Rock salt*, a small piece—nothing.
7629. *Spirit lamp flame*—nothing.
7630. *Oil of turpentine* in bottle—very sensible effect.
7631. *Per sul. Iron solution* in bottle—sensible.
7632. *Chloride of Arsenic* in bottle—Yes.

22 SEPTR. 1845.

7633. Employed the two Magnets as arranged on Saturday and found it very convenient—for now, as different things were put between the poles, the latter could be brought as near together as the substance would permit; the Magnetic lines of force being thus strengthened.



* [7621]



7634. Compared Oil of turpentine in a bottle two inches in diameter and in a tube of $\frac{2}{3}$ of an inch, to see whether the greater intensity of the lines of magnetic force in the latter case would compensate for the diminished thickness of fluid through which the ray passed. But the effect was best with the bottle.

7635. In using fluids contained in cylinders, as bottle or glass, it is advantageous to have the poles and fluid near the eye piece, so that adjustment may be made to suit the refraction in a horizontal plane and obtain the best image. The striæ, etc. in the glass of the bottle give a good deal of light and form their own image, but it is easy to distinguish the soft equal image due to the power of the fluid from that due to the bottle, and its place is generally distinct and in the middle of the bottle image. The advantage of beging. to observe with a feeble image and noting its increase or diminution is very great (7577).

7636. Now worked with a number of solutions contained in bottles from the shelves of the Laboratory, and had not a case in which I could not obtain the effect, except where great depth of colour or turbidness of the solution interfered with the observation.

7637. Further, in all case[s] the direction of the rotation was the *same*. In fact it depended upon the direction of magnetic force and not upon the solution.

7638. The following are the solution[s] in water used:

7639.

Iodic acid—very fair.
Mur. Acid—*rather good.*
Nitric acid, strong and pale—feeble.
Nitrous acid, Green.
Oil of Vitriol—not strong compared to its weight.
Sulc. Acid, S.G. 1·75.
Phosphoric acid.
Phosphoric acid from bones.
Boracic acid.
Sulpho cyanic acid.
Arsenious acid.
„ „ dissolved in Mur. Acid; *rather Good.*
Arsenic acid.
Chromic acid.

7640. Ammonia, strong—*rather good.*
„ Carbonate.
Potassa.
„ Carbonate.
Soda.
„ Carbonate.
Silicate of Potassa.

7641.

Mur. Ammonia—fair.
Chloride Sodium.
,, calcium.
,, ,, sat. solution—*good.*
,, barium.
,, strontium—*good.*
,, Magnesia—*fair.*
,, Manganese.
,, Iron.
,, Zinc.
,, ,, dense.
,, cadmium.
,, tin (proto).
,, nickel.

Chloride cobalt.
,, copper (proto).
,, ,, (per).
,, lead (in M. Acid).
,, antimony (in M.A.).
,, bismuth (in M.A.) dense. *Good.*
,, Uranium.
,, Mercury. Cor. Sub.
,, gold.
,, platina.
,, palladium.
,, Rhodium and Soda.
,, Aluminium.

7642. Iodide potassium—fair.
,, Zinc.
,, iron.

7643. Cyanide mercury.
,, silver in Cy. Potm.
,, gold Do. fair.

7644. Borax.
Ferro pruss. pot.
Oxalate Ammonia.
Tungstate soda.

7645.

Sulphate Soda.
,, Magnesia.
,, Manganese.
,, ,, and Ammonia.
,, Iron proto.
,, ,, per (7631).
,, Zinc.
,, Nickel.
,, Copper.

Sulphate molybdenum.
,, cerium.
,, uranium.
,, cobalt.
,, Mercury per.
,, Silver.
,, platinum.
,, rhodium and Soda.
,, alum. and pot. (alum).

7646. Sulphite Soda.

7647. Sulphovinate lime.
,, lead.

7648. Nitrate Amm. *fair.*
,, potassa.
,, soda.
,, lime—if strong perhaps good.
,, baryta.
,, strontia.

Nitrate magnesia.
,, manganese.
,, iron (sesqui).
,, zinc.
,, tin—*fair.*
,, nickel.
,, copper.

Nitrate bismuth—*good*.
,, uranium.
,, mercury (proto) fair.
Nitrate mercury per.
,, silver.
,, alumina.

7649. Phosphate Ammonia.
,, Soda.
,, ,, and Am.
,, Nickel in M.A. (good).

7650. Chromate Ammonia.
,, potassa.
,, ,, bi.
,, soda.

7651. Arsenite Ammonia.
,, Potassa.
Arseniate Potassa.

7652. Ammoniuret Cobalt.
,, copper—poor.
,, nickel.

7653. Chloride of Arsenic anhydrous—*fair* (7632).
,, ,, hydrated—feeble.
,, Sulphur—*rather good*.
Sulphuret carbon.
Kreosote—*fair*.
Water—*fair*.
Alcohol—poor.
Ether—about as alcohol.

7654. Tincture Wohler's cyanate potassa.
,, Sesqui chloride iron—feeble.
,, Galls—feeble.
Honey sugar in Alcohol.
Sugar in alcohol—yes, has naturally a little righthanded rotation.

7655. Mr Jones here with his magnetom[et]er—also Captn. Johnstone, Lieut. Riddle, etc.

7656. Placed a bar magnet so as to deflect the magnetometer. Then observed the deflection by a telescope, etc. which would be able to tell a change in the power of the magnet on the needle of only $\frac{1}{20,000}$ of its actual power at that given distance. Interposed one plate of the heavy glass, edgeways, endways—alternating—interposed at last 6 plates making a great mass between the magnet and the magnetometer, but could not obtain the least signs of any difference. Hence there seems to be no particular or specific magneto induction belonging to the glass. It was quite indifferent.

7657. Wrought with the Magnets as before (7621), and tried numerous substances of which the following are lists. Find very few bodies which do not give way and shew evidence of the action, but find none which comes up to the heavy glass in exhibiting it.

7658. Sol. of Carb. potash saturated—very feeble.
,, Nitrate lime Do. Do.
,, Nitrate Amm. Do. Do.
,, Mur. Magnesia Do. Do.
,, Chlo. calcium Do.
,, Nit. Bismuth in N.A.—feeble.
,, Chlo. Bismuth in M.A.—fair.
,, proto sul. iron—little.
,, Sul. Nickel.
,, Phos. Nickel in M.A.
,, Carb. Ammonia.
,, Iodine in water.
,, Acetate Morphia.

7659. *Oils.*
Almond.
Castor.
Olive.
Poppy.
Bitter Almonds.
Resin oil.
Sperm.
Elaine[1] from Hog's lard
Oil Do.

7660. *Essential Oils.*
Bitter almonds.
Spike lavender.
Lavender.
Jessamine.
Cloves.
Laurel.
Spikenard.

7661. Copaiba balsam.
Canada balsam.
Naphtha Native.
Naphtha from coal gas.
Oil gas liquor rectified—not bad.
Resin in potash.

7662. *Solutions in Alcohol.*
Mastic.
Camphor.
Do. and Cor. Sub., a saturated Solutn.
Spikenard.
Myrrh.
Naphthaline.
Amygdaline.
Cholesterine.
Silvic acid.
Pinic acid.
Resin in cold Alcohol.
Soap.
Cinchonia.
Morphia.
Acetate copper.
Sul. Cinchonia } in dilute S. Acid.
Sul. Quina }

[1] *Elain.* A synonym of *Olein. O.E.D.*

7663. Muriatic ether.
Phosphoric ether.
Acetic ether.
Œnanthic ether.
Essential oil from grain.
Oleum Etherium—Bullock.
Aldehyd—hardly sensible.
Pyroligneus Ether.
Solution of Pepsin.
Glycerine.
Aloxan in weak N.A.

7664. Fluid chloride of Carbon } saw nothing; but quantity of fluid (in
„ Bi carb. hydrogen }
a little tube) too small to allow a judgment.

7665. Citric acid in water—very fair.
„ „ in Alcohol.
Acetic acid, glacial—very little.
Very Strong acetic acid—Do.
Oxalic acid in water—fair.
Tartaric acid.
Tartaric and boracic acids.

7666. *Solutions of Salts* in water.
Tartrate potassa.
„ „ and Soda.
„ „ „ Anty.
„ „ „ Manganese.
„ Manganese.
„ Iron.

7667. Citrate potassa.
Benzoate Ammonia.

7668. Solution of (in water)
Acetate ammonia.
„ potassa.
„ soda.
„ lime.
„ baryta.
„ manganese.
„ Iron.
Acetate Zinc.
„ nickel.
„ lead.
„ „ sub acetate.
„ copper.
„ cobalt.
„ alumina.

7669. The following bodies were examd. in a fuzed state in tube[s] from 0·7 to an inch in diameter:

Sulphur—distinctly.
Nitre—doubtful.
Spermaceti—yes.
Wax—doubtful—wax not clear.
Camphor—doubtful—must try larger quantity.

7670. The following gases were tried in bottle about 3 or 3½ inches in diameter. Must remember the counteracting effect of the air where the Mag. lines pass in contrary direction.

Nitrous oxide Olefiant gas Sulphurous acid Ammoniacal gas Muriatic acid Sulphuretted hydrogen Carbonic oxide	no sensible effect.

7671. *Gold leaf*—nothing.
7672. *Rock salt*—a plate about 1 inch thick and polished on surfaces—obtained the effect distinctly but not like the heavy glass; it is better than water I think or than fluids generally and is a case of a *crystalline body*.
7673. *Rochelle salt*—could not make out the effect.
7674. *Rock crystal*—a natural crystal 1½ inches thick—tried through two sides and with very fair light, yet could obtain *no effect*.
7675. Another crystal 2 inches thick—yet no signs. In both these cases I could obtain perfect darkness of the polarized image, by the Nicholl Eye piece.
7676. *Calcareous spar*. A Rhomboid, 1 inch thick; could not obtain a good separation of images by the eye piece but could not obtain Magneto effects.
7677. The large plate (7554) examined again carefully and could find nothing particular in it. The polarized image and apparatus is now well arranged—perhaps before it was not.
7678. Many of the bodies tried had *rotating power per se*, and it was important to ascertain how this was affected by the Magnetic force. On former occasions the magnetic rotation was simply either added to or subtracted from the inherent rotating force of the body (7607–9, 7565).

Castor oil possesses *right handed* rotation and took up Mag. rotation.			
Resin oil		Do.	fair.
Oil of Spike lavender	a little	Do.	
Oil of Laurel	well	Do.	
Canada Balsam	little	Do.	
Copaiba Balsam—good *left handed* rotation		Do.	

7679. *Camphor*—melted—has a right hand rotation per se, but could not obtain a sensible effect of Magnetic rotation; perhaps mass not enough—was in a tube about 1 inch diameter.
7680. Camphor in Alcohol—has right handed rotation and receives Mag. rotation.

7681. A strong solution of Camphor and Corrosive sublimate in Alcohol has as good a right hand rotation as *camphine*, and also receives magnetic rotation.

7682. *Tartaric acid.*

A solution of Tartaric acid has a trace of rotating power per se.

7683. Solution of Tartrate of Soda has a little rotating power per se—but I did not observe it in Tartrate of Potassa.

7684. Solution of Tartrate of potassa and Antimony has rotating power per se.

7685. Solution of Tartaric and Boracic acid has very strong rotating power per se—stronger than *Camphine.*

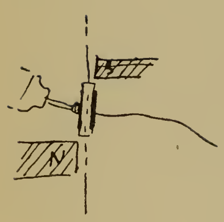
7686. *Sulphate of Nickel.*

An aqueous solution of this salt has fair rotating power per se. A solution of Phosp. Nickel in Phos. acid has not.

7687. Bodies which have rotating power per se do not seem by that to be capable of assuming more freely the Magneto power.

7688. Cannot but suppose some relation or similarity of constitution between bodies rotating per se and such as are under the influence of magnetic forces. Such bodies are to the latter what ordinary magnets are to Magneto helices when the current is passing through them.

Magnetic and Electric induction.

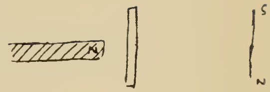


7689. A piece of heavy glass 4 inches square and 0·4 of an inch thick had Leyden coatings of tin foil applied to the sides. It was then placed in position between the Magnetic poles and insulated—one lining was connected with the ground. The Magnetic curves were thrown on and the effect of the glass observed. During the observation a charged Leyden Jar was brought against the other coating to produce electric induction also. But *no change* on the ray was produced.

7690. A cube of heavy glass (*174*), about 0·4 of inch in the side, was mounted on cork and examined as to its magneto power at common temperatures. Then it was gradually heated (from time to time being observed) until it was so hot as to char the cork—but no alteration in its magnetic relation could be perceived. When irregular in its heat it depolarized the ray and so on, but no additional effect could be perceived upon the superinduction of the magnetic curves of forces. Cracked on cooling.

7691. As to *heavy glass assuming ordinary Magnetic powers.* Examined it in all ways by means of a magnet and filings but could not find anything else than perfect indifference.

7692. Arranged a magnet and needle according to the manner of Haüy so as to make the test very delicate. Then interposed either continuously or intermittingly a fine plate of glass, and that either broad ways or end ways, reaching from magnet to the needle, but could not procure the least signs of action or difference whether glass was there or not.



7693. Appears that the Magnetic force does not act on the ray of light directly (as witness non action in air, etc.) but through the mediation of the special matters; i.e. it gives them, and they it, the power of acting on light.

7694. But that shews that Magnetic force has a direct action on these substances—very different in its result to that which it has on magnetic matter. It is in fact, as far as regards our knowledge, a new magnetic force or mode of action on matter.

7695. It is also the first true relation of Magnetism and light.

7696. It is also in favour of the view I entertain that magnets and magnetic bodies act by means of the intermediate particles.

7697. The phenomena shew that Magnetic lines of force induct far more powerfully in certain respects on matter than lines of Electrical force.

7698. The magnetic lines of force would doubtless act upon other rays, as rays of heat for instance. Then must consider this relation to heat rays and see to what it would lead.

7699. As to magneto relation of light—I could not make Morrichini's[1] expt. to succeed when at Rome.

7700. The power of conferring rotating forces on matter may perhaps serve hereafter as a means of measuring (Biot fashion) the internal condition of bodies in philosophical investigations.

7701. Biot says that motion of fluids does not affect their action on polarized light. Lit. Gazette, Decr. 9, 1843, p. 797. Paris letter.

7702. The heavy glass in the magnetic curves must have some new and equivalent magnetic properties.

7703. If not a magnet, then the molecular action in it is specifically

[1] D. Morichini.

different to that of Iron. If a magnet, then we could examine a transparent magnet.
7704. Bodies which appear to have no power may be only like or nearly like air—for then the effect of the contrary lines of Mag. force are always present.
7705. *Probable conclusions, relations or points.*
Relation of Magnetism and polarized light.
„ „ magnetic lines of force to dimagnetics.
Magnets act by intervening particles.
Relation of Electricity and polarized light (through Magnetism).
How to observe the effect.
Internal constitution of dimagnetics.
Relation of the Rotating polarization to the rotating currents in the effective magnet—same direction—i.e. lie in the same plane.

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7706. *Electric currents and Magnetic lines of force conjoined in Electrolytes.*
7707. Have made a small glass cell such as could go between the magnetic poles—it was 1·5 inches long, 1 inches wide and 1·25 inches deep internal measure. It consisted of pieces of plate glass ground at the edges and cemented together by white hard varnish.
7708. Prepared two electrodes of platina plate nearly as large as the size of the cell. Prepared for them jackets of plate paper to prevent the gas evolved against the electrodes mingling with and disturbing the transparency of the fluid in the body of the cell. When the electrodes were in their places and connected with the wires proceeding to a Grove's battery of 5 pair of plates, a ray of light could either pass between them and so *across* the course of the electric current or, because the plates were not quite so large as the cell, the ray could be passed by their edges through the cell and so *along* the course of the electric current.
7709. Also, by varying the disposition of the Magnetic poles, the lines of magnetic force could be determined in the direction of any of the three coordinates.
7710. Supposing the direction of the examining polarized ray to be constant, then the direction of the magnetic curves may be

in three directions to it, and also the direction of the electric current may be in three coordinate directions, giving nine variations of the two forces relative to the position of the polarized ray, which I will note as follows:

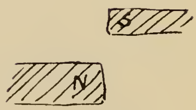
1. M.C. parallel to ray . . . E.C. parallel to M.C.
2. ,, ,, ,, . . . ,, perpendicular to M.C. in horizontal plane.
3. ,, ,, ,, . . . ,, ,, ,, vertical plane.
4. M.C. perpendicular to ray in horizontal p.. . . E.C. parallel to ray.
5. ,, ,, ,, . . . ,, parallel to M.C.
6. ,, ,, ,, . . . ,, perpendicular to ray and M.C.
7. M.C. perpendr. to ray in Vertical p. . . . E.C. parallel to ray.
8. ,, ,, ,, . . . ,, perpendicular to ray and M.C.
9. ,, ,, ,, . . . ,, parallel to M.C.

7711. The ray was polarized by reflexion in a horizontal plane. The magnetic lines of force may each be considered as reversed, but in the experiments made, it was not thought necessary to do this. The Electric currents may also be reversed, and that *was done* by changing the poles of the battery.

7712*. The positions 1, 2, 4, 5, 7 and 8 were easily obtained by the use of vertical electrodes. The positions, 3, 6 and 9 were also obtained by the use of vertical electrodes combined with the use of a ray of light polarized by reflexion in a vertical plane.

7713. The magnetic arrangement was that recently used of the Woolwich magnet and ours, and the polarity was as before (7621).

7714. First a saturated solution of sulphate of Soda was employed, and the lines of power disposed in it according to figs. or positions 1, 2, 4, 5, 7 and 8, the electric current in each case being reversed and observed both continuously and intermittingly. But I obtained *no additional effect* on the polarized ray.

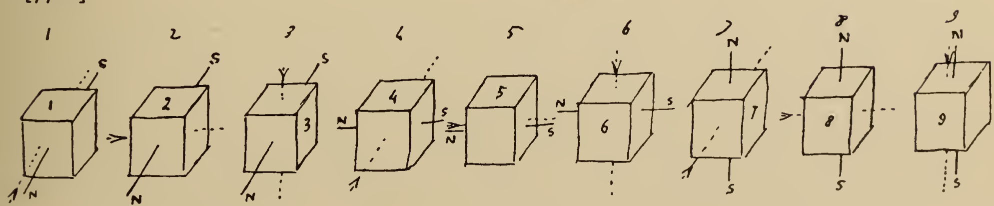


7715. Then dilute Sulc. Acid (1 oil vitriol + 3 water) was used and put into all the conditions from 1 to 9, with reversion of the Electric current, etc., but still without effect.

7716. When the electrolyzation was going on, strong striæ (   ) were produced in the fluid near the electrodes and there caused distortion and change of shape of the polarized image, but no other effect than was produced without the magnetic lines of force.

7717. So the combination of Electric currents with magnetic forces does not give any very striking effects and perhaps there

* [7712]



are none which polarized light can shew. But I am not sure of that. The quantity of fluid was very small for length of ray to pass through (for in making it more I should have weakened the magnetic curves), and considering the nature of the relation between Mag. and Electric forces, I think there must be some effect produced which stronger magnets and other forms of apparatus and the progress of our knowledge will enable us hereafter to develope.

7718. Still, I have at last succeeded in *illuminating a magnetic curve* or *line of force* and in *magnetising a ray of light.*

7719. What effect does this force have in the Earth, where the magnetic curves of the earth transverse its substance?

7720. Also what effect in a magnet?

7721. Does this force *tend* to make iron and oxide of Iron transpar[en]t?

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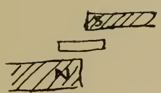
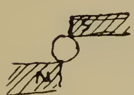


7722. The two magnets, Woolwich and ours, arranged as heretofore (7689, 7621).

The coated plate of heavy glass (7689) placed between the poles so that the coating[s] were *perpendicular to the Mag. lines* and *to the polarized ray*—but two small holes had been made in them by which the ray might pass. Then uninsulated one coating and applied the knob of a charged Jar to the other, so as to put on lines of *electric tension parallel to the magnetic lines.* Nothing came of it.



7723. Query effect of *Expansion.* Placed a large mercurial thermometer between the Magnetic poles and then laid the power on and off; *no effect of change of volume.* Did the same with a large spirit thermometer $1\frac{1}{2}$ inches in diameter in the bulb.

7724. *Ice.* A piece of good American ice—made surfaces square and tried it between the Mag. poles. Being crystalline, it depolarized the ray, but when turned round into a neutral position *and then* the magnetic forces put on, I could perceive *no effect.* I cannot say there is none. The effect with water is only small, and ice is inconvenient to work with at this time of the year, from the melting of the surface and flowing of water over it.

7725. *Sol. of Tartaric and Boracic Acids*—excellent for Rotating power per se. Magnetic force added effect to it.

7726. *Borate of lead* in rough fragments—does—promises well to be an available and ready body for general experiment.

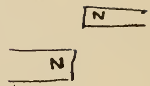
7727. *Bromine Vapour*—no effect.

7728. *Hydrogen*—no effect.

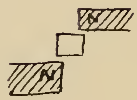
7729. *Oxy alcohol flame*—nothing sensible but the flame too luminous.

7730. *Rock crystal from Mr Dollond*—no effect.

7731. Now altered the connexions of the Magneto helices so as to make the two vicinal Magnetic poles both N; but in fact the Woolwich Magnet was so powerful that the near end of our magnet was hardly N. On making a steel bar touch each, that by examining it with a magnetic needle I could have proof of the state of the poles, if I took away the bar from our Magnet N *before* I caused the electric current to cease, it was scarcely magnetised at all. So that I must not count on these as equal poles (   ).

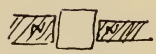


7732. *Sol. Sul. Soda saturated*, in the square cell (7707), placed between the poles. An electric current of 5 pr. Grove's battery sent across it parallel to the magnet lines, i.e. the line between N and N, and to the polarized ray, the latter being examined through two small holes in the middle of the electrodes. There was no effect.



7733. All other things remaining the same, the direction of the electric current was changed, it being now sent from side to side *across* the *ray* and *the magnet lines* or line between N and N. No effect.

7734. The magnetic poles were placed on each side of the ray and the electric current also sent *across* the ray from N towards N. *No effect.*



7735. The ray and Magnetic poles remaining the same, the electric current was now sent parallel to the ray. No effect.

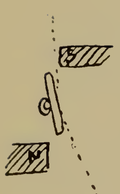
7736. Now arrangements were made in which the electric currents would certainly rotate. Thus the cell had one electrode arranged as a lining to the inside and this had two holes cut on opposite sides, that a ray of light might pass across the cell. The other electrode was a small plate in the middle, confined by a little cylinder of paper so that the gas from it should not obscure the liquid. Thus the electric current would be in the direction of radii. The Magnetic poles were placed above and below and were *contrary*, so that lines of magnetic force passed vertically through the liquid. When the current was passing, the contents of the cell rotated well and rapidly. But being examined by the horizontal ray, there was *no change*, for except the effect of the bubbles of gas set free from the platina lining of the cell, and which were easily taken account of by themselves, there was no difference in the action of the liquid on the ray, whether it was as simple liquid —or the Magnetic curves were through it, or the Electric current— or both combined. So no effect.

7737. The same arrangement now had a ray passed upwards through it, being polarized in a vertical plane, so that it was parallel to the lines of magnetic force. Now the effects of these lines of force were seen, but *no effect* of the electric current.

7738. *Heavy glass.*

Placed a piece so that the Mag. contrary poles should be above and below it, i.e. that the lines of magnetic force should traverse it in a direction *perpendicular to the plane of polarization*. Obtained no effect.

7739. All seems to shew that it is the *magnetic lines only* that are effectual, and they *only* when parallel to the ray of light or *tending to parallelism*.



7740. Want a transparent oxide of Iron—thought perhaps steel buttons tinted by oxidation might do, using the bright steel beneath as a reflector—but could obtain no clear effect. In fact the steel becomes magnetic and it is doubtful whether there are magnetic lines of any force in the film of oxide where the ray is reflected from it; at least, not such lines as I want.

7741. But observed that without the Magnetic poles and simply by themselves, many of these buttons *have to a slight degree the power of rotating the ray by reflexion*, and that sometimes the rotation is to the right hand and sometimes to the left; and further, that this does not seem to occur in any particular order as to the degree of oxidation—only that a certain amount of oxidation is required to rotate at all.

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7742. Arranged a magnetic needle so as to vibrate freely on a low support, so that its motion should be near to the plane beneath it, in Arago's manner, and then vibrated it either upon a flat book or on a mahogany table, or on the large plate of heavy glass (*219*); but could discover no difference; the number of vibrations in falling from a large to a small arc, and the time of the vibrations were in all cases sensibly the same. So probably no effect as to motion, attractive or repulsive by magnet, either when still or moving.

7743. Floated a bar of heavy glass (7807) (*174*) in water, and kept it in the middle of the glass by a wire rising into a tube, etc., and then on the outside of the glass brought the opposite poles of our horse shoe magnet, and left all to ascertain if any effect. Could not discover that the glass paid any respect to the magnet; it kept any position it happened to take up and was not drawn or repelled either into one position or another (7902).

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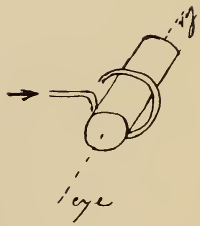
7744. I have prepared a long helix. It consists of two glass tubes of an inch external diameter, and these are covered each by a coil of copper wire silked, which having gone round to one end as a close helix then was returned in a similar and continuous manner, so as to enclose each tube in a double helix. Then these tubes were fixed on a board in a straight line—they were conjoined by a caoutchouc tube in the middle and terminated at the extreme ends by brass caps with glass terminations, so that the tube could have a ray of light passed through. The whole helix was 65 inches long (being double in that distance)—consisted of wire $\frac{1}{30}$ of an inch thick—contained 1240 feet of wire; it was conjoined at the wires so as to form a helix all in one direction, and a battery of 10 pr. of Grove's plate[s] was used to pass a current through it. The direction of the [current] round the helix in respect of the polarized ray and the eye was under the coming ray, up on the right hand side, then over the tube and down on the left hand side—in fact, left handed or against the watch hand motion.



7745. The magnetic force of the helix on a magnetic needle was comparatively very small. The end towards me or twds. the eye attracted the S end of a magnetic needle and the further end attracted the north end of the magnetic needle. So that the helix was parallel in position to a magnetic needle in its natural position (7795, 7801).

7746. All this was arranged so that a ray polarized by reflexion in a horizontal plane could be sent through the helix and observed at the other end by a Nicholl's eye piece or otherwise.

7747. *Air* being in the tube, the E. Current was sent through the helix, but no action on the polarized ray could be observed.

7748. An ordinary ray was also sent through the air and helix and observed afterwards, but no effects on it could be perceived.

7749. An ordinary ray was also sent through the air and helix, afterwards polarized and then observed, but nothing particular could be seen in it.

7750. *Water* was put into the tube (distilled water) and now an effect was produced. The water acquired the power of *circularly polarizing*, and that to the *left hand*. This was seen by the increase of the image when it was made visible on the one side of the obscure point and diminution when brought in by motion of the handle in the other direction. The effect was instantaneous and not lingering as with the Electromagnet. It was not much on the whole, but clear and precise.

7751. When an ordinary ray was sent through, then polarized and examined, I could perceive nothing.

7752. *Sulphate of Soda*—saturated solution—care is required to have a very uniform solution—the wet sides of the glass tube in the first instance caused striæ and irregularities of composition that continue for a long time, many hours, and confuse the beam of light. When all was right, the effect was very good. Circular polarization was produced (left handed) as long as the current was continued, but to a much greater extent than with the distilled water. So the helix is a good thing, and in this case the polarized ray may truly be said to be electrified, though its action is no doubt exactly the same as that produced by magnetic forces. All goes to shew that these unmagnetic bodies, as they are called, are acted upon and assume new conditions under the influence of magnetic and electric lines of inductive force, and that according to very definite and precise laws.

7753. But now came forth a new and striking result, not obtained as yet with the electro magnet. It is a much slower effect than the former and of the following kind.

7754. Suppose the eye piece so arranged that the polarized ray is extinguished: on sending in the Electric current, the ray becomes (as above) slightly visible by left handed rotation, and that effect goes off the instant the current is stopped. But continuing the current, the light increases in size round about and the form of the original light becomes obscure and confused, and the amount of light which comes to the eye is greatly increased. This increase goes on for perhaps half a minute or more, then goes down somewhat, but some of the effect still remains. When the current is stopped, this luminous appearance gradually goes off as it came on, and things apparently return to their first state.

7755. If by a little advance of the eye piece handle clock fashion the polarized ray is rendered slightly visible, then on making contact the *increase of light* (being the first or old phenomenon) is very striking, and on breaking contact at once it goes off again as before. But *continuing contact*, *i.e. the electric current*, the parts about the aperture at the end become luminous and large, and this goes on increasing and changing form for half a minute or more: when at a maximum it goes down in effect and returns slowly towards that state or appearance at least which it had on first putting on the current. On taking off the current, then that part of the remaining effect which belonged to the first phenomenon went off at once and the remaining part or that left of the second phenomenon went off gradually.

7756. If during any part of this course of events the Electric current be stopped, *all the light* visible falls in intensity, and on renewing contact rises again, thus shewing for the *whole* of it the power of suffering and sustaining the immediate and direct action of the current. So that the induced light has the same property as that coming direct from the polarizing reflector.

7757. If the current be stopped when the extra light is half grown up or more, then the extra light goes down again gradually—returning nearly through its phases—and going down much quicker than if the current be continued.

7758. If the current has been continued long until the extra light is gone down, then be stopped and then *at once renewed* again, the extra effect is feeble. It appears as if the solution must either rest, or else have time to return out of a state of tension, before it is able to shew the effect again.

7759. The extra light has in its appearance some dependance upon the reflexions from the inside of the glass tube about the further end, and its form and shape of development most likely depend much upon that: but I do not think the effect originates there. However, whether it is in that surface or in the fluid between that surface, the aperture and the eye, wants further investigation.

7760. There is no doubt that more light comes to the eye when the extra effect is on than when simply the first effect is on—and further, the extra effect remains for a time when the current is stopped, whereas the first effect goes off at once.

7761. Can it depend upon water adhering to the tube from the former experiments, i.e. on striæ or reflecting or inflecting surfaces and parts? But if so, still *it is dependant* upon the current and produced by it and remains for a time after the current is stopped.

7762. Perhaps motion is produced in the fluid particles.

7763. There is *no sensible colour* in these *extra light images*.

7764. If the polarized ray be made visible in the first instance by turning the eye piece handle to the left—then on making electrical contact, the light is decreased—and on breaking contact it rises again. This is the old or first effect.

7765. But *continuing the current*—then after the first decrease, diffuse light as before and from the same parts grows up like a phosphorescence, and at last the image is by comparison very bright—after which it gradually goes down again.

7766. During the whole time, if contact be broken—*all* the light rises in intensity—and it falls again on contact. Shewing as before the separate condition of the first and the second effect—of the momentary and continuous one—and shewing also that all the light which appears is subject to the first or momentary state.

7767. In fact, the first effect is an effect of circular polarization. But the second effect is apparently one of depolarization.

7768. If the current be discontinued at any moment, the extra light falls to its normal state quicker than if the current be continued (7765).

7769. In this case also the fluid wants rest to prepare it for a new effect (7758).

7770. Emptied out all this solution of *Sulphate of Soda* and put in another portion of the same, so as to have the liquid more uniform (7752). The image now seen through it was very good. *The effects were just as before.* So that they cannot be due to striæ or differences of density in the solution.

7771. Poured out the sulphate of Soda solution and without washing the tube, put in distilled water—so no doubt there are irregularities in the fluid within.

7772. The effect of making and breaking contact was there—but weak.

7773. The second or extra effect was also there—but the extra light moved much, becoming obscure and appearing as if there were waves within or motion of the different fluids inside the tube.

7774. After standing horizontal some time—observed the effect again. The object was at first pretty clear but there was a line of refraction along the middle of the tube from difference of density in the solution.

7775. On putting on the Electric current, the *second effect* was very fine, especially on the lower half of the solution. The effect was quite like a strong phosphorescence; then it went down in that part and the upper part became a diffuse dull luminous object.

7776. On letting it rest and then repeating contact, the same effect was produced.

7777. The cause why the effect appears here and there, first above or below and then below or above, is simply either that the surface of the side of the tube in that direction, or the density of the solution there, reflects or refracts the rays more or less favourably at different times in the course of one effect, according as that effect which takes time has attained to a more or less perfect degree in the parts of the fluid which the ray is passing through.

7778. But there is certainly some *new motion or condition* of the particles of matter in the contents of the tube.

7779. Try a shorter tube with Sulphate of Soda solution and stronger helix.

7780. Removed the last water from the tube and put in *fresh* *distilled water*, so now the contents of the tube nearly pure and uniform, and the image of the polarized ray at the end was very good.

7781. The *first effect* was as before, though smaller than with sulphate of Soda.

7782. The *second effect* was also produced (7753) with this *pure water* and very beautiful. Yet here there could be no striæ—so that it must be in the particles of the fluid itself.

7783. The image here was separated by a dark space from the image formed by light reflected from the sides of the tube. But the increase of light began at and spread from the central image; as if it was the particles of water in the middle of the mass that first made their effect visible to the eye. It would not have been so if the effect had been due to any action of the surface of the tube or of it on the fluid. Must be an effect of the particles of the fluid. Strongest first at the axial line and then coming on in proportion to the parts farther from the centre. Can divide these portions by different tubes in a larger helix.

7784. This effect is not one of circular polarization, but a power (permanent for the time) of depolarizing the light.

7785. Introduced a sul. lime plate so as to produce a colour and then produced the effect. The colour was not changed but the new effect appeared to be added on to the effect of the sul. lime.

7786. Singular and beautiful Phenomena there. No guessing what they will extend or lead to—an open door.

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7787. Worked again on these phenomena, and after much experiment and consideration made out the cause of the second effect, that of the diffuse phosphorescent light; and so, though I have lost an imagined discovery, have removed a stumbling block. It was altogether the effect of the heat evolved in the helix by the electric current and communicated through the glass to the fluid within. This heat raised the temperature of the outer layers of water before it attained to the central or axial portion, and so the ray[s] of light were bent towards the axis and made the light from the object end converge to my eye at the eye end. Hence the effect of a lens was produced. Hence also the wave like motions of the light as the water of different temperatures mixed. Hence also the falling of the effect with the continuance of the current and yet a remains of part of it. Hence also the use of rest to cool the water again, and so on throughout.

7788. But at least one useful effect is learned, for it is a good thing to have a lens of long focus at the object end of the liquid tube to converge the rays to the eye: for when the warmed tube did so, the effect was brilliant, and from the no-light state, abundance of light sprang up. Very small effects were in this way made powerfully visible, and I have no doubt that now I shall find the effect in air.

7789. As to the other phenomenon of circular polarization, that comes out constant, clear and beautiful.

7790. Used the same long helix in the same order and position with a battery of 10 pair of Grove's plates. So the N end of the helix is towards me and the S end towards the polarizing mirror; as was proved by the magnetic needle.

7791. *Water* in the tube. The effect good, and the rotation to the left hand; i.e. I have to move the handle of the eye piece to the left hand to compensate for it and return the ray to the condition it was in before the current was sent through the helix.

7792. Reversed the electric current, leaving the helix and all other things unchanged, and now the effect was the reverse of the last (7791), and the polarization right handed.

7793. Sent the whole current of the battery through only the further and smaller half of the helix. The effect was very good, as good as with all the helix—the order of rotation conformable to the results above. Much more electricity going through the half than through the whole.

7794. Sent all the current through the near half of the helix. The first effect was not so good—rather poor. But probably the heat before spoke of (7787) was in some degree counteracting the results *as to appearance*.

7795. Now employed our large cylinder helix. It has a length of 19 inches of spirals—is from 14 to 15 eighths of an inch internal diameter—nearly 3 inches external diameter—and contains in two spirals (concentric) about 80 feet of copper wire nearly $\frac{1}{5}$ of an inch in thickness. It makes a very strong electro-magnet with its iron core—and the wire conducts a great quantity of electricity, so that the battery hisses when in communication by it. *Without* its core and acting by its own magnetic force on a magnetic needle, it is vastly more powerful than the long helix (7744). Whether its effect in producing the rotatory polarization is in proportion is an important point (7801).

7796. A stout glass tube, 3 feet long and $\frac{10}{8}$ of an inch in diameter inside, had glass ends fitted to it by Canada balsam, corks, etc. It was filled with distilled water and placed *centrically* in the helix so that about 15 inches was out of the helix at the further end.

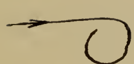
7797. And now the eye piece, being near the eye end of the tube and helix, the rotation induced by the Electric current was observed. It was produced, but not strongly—indeed it was very small, less than with the long helix (7744). The helix was a magnet in the same direction as the former, i.e. the end near the eye was the N end and the rotation was according to it.

7798. I removed the eye piece 4 feet back, leaving all the rest unchanged, and then could scarcely distinguish the first effect.

7799. Drew the glass tube forward until the eye end projected about 15 inches (7796), placed the eye piece near this end and observed. The effect was there but small and just as before.

7800. In this state I placed a magnetic needle near the out or eye end of the water tube, and then making the current travel, observed the extent of the motion of the needle. Then pushed in the tube through the helix so as to remove it far away, leaving the helix and needle as before. On making the current pass, the needle had the same amount of vibration as before. The water appeared to have no effect upon it magnetically.

7801. Worked with the *Woolwich magnet*. The helix is $26\frac{1}{2}$ inches long—$2\frac{1}{2}$ inches internal diameter—$4\frac{3}{4}$ inches external diameter—it has four concentric coils and in these there is 501 feet of copper wire about $\frac{1}{6}$ of an inch in thickness. The four coils were joined in succession as one long coil. It happened that the eye end of the helix attracted the N end of the magnetic needle, the electric current being the reverse of the former one, or thus (7744). The helix alone was exceedingly strong as a magnet on a magnetic needle (7795).



7802. The water tube already described (7796) was put into this helix and the battery action put on. The effect was very good and the rotation *right handed.*

7803. Then *sulphate of Soda* solution was put into the same tube and the power of the helix applied. The effect was good but not better than that of the water.

7804. Pushed in the tube until not more than a foot in length of it was left in the helix at the polarizing mirror end. Could then obtain the effect but not so strong as before. It appears to be in proportion to the length (apparently) in the helix.

7805. The effect appears to be by no means proportionate to the power of the helix to act externally as a magnet, as is shewn by the great effect of the first small long helix compared with the two larger ones (7745). Nor does it appear to be proportionate to the power which the helices have of making soft iron a magnet, as appears by the same comparison.

7806. As to the second effect or that of heat (7753, 7787), neither of these large helices shewed the least trace of it, and with good reason: there was no heating of the fluid.

7807. The little prism of heavy Glass about $1\frac{6}{8}$ inches long, being placed in the axis of the Woolwich magnet, gave indications of the rotation, but only small. The prism is indeed very short.

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7808. A fine sunny day, so made a few hasty experiments with the sun light (7912–22, 8683–93, 95–706).

7809. A simple helix of uncovered wire about 0·077 of an inch thick turned into a spiral of 1 inch external diameter, 4·2 inches long and containing 56 convolutions, was connected by wires with a most delicate galvanometer (Matteucci's) and placed with its axis parallel to the sun's rays. There was no effect.

7810. On alternately allowing the sun to shine on it and shading it, I could obtain no effect.

7811. On allowing the rays to pass through the middle and not outside, then outside and not through the axis—alternating the action—no effect.

7812. On putting a small neutral steel needle into the helix in the ray or taking it out, or alternating these motions—no effect.

7813. Revolving the needle whilst in the helix one way or the other—no effect.

7814. A *magnetic needle* put into and out of the helix made galvanometer needle move—but not strongly—so all the connexions right.

7815. Put in the *Mag. needle* and left all quiet; then whilst one pole in the helix and in the sun's rays, rotated the needle with the fingers—no effect. With the whole needle in the helix, rotated it with the fingers only, one way and the other alternately—no effect.

7816. Altered the position of the helix—polarized the sun's ray by a glass mirror and sent it through the helix—no effect.

7817. Put the needle in and repeated the above conditions, but no effect.

7818. Used a larger helix of covered wire, consisting of four concentric coils and containing 150 feet of wire, and repeated all the former circumstances—still no effect.

7819. The entrance of one pole of the magnetic needle (7814) caused the Galvanometer needle to move 5° or 6°.

7820. Joined the ends of the helix together so as to complete the circuit. Then put a non magnetic needle in parallel to the sun's ray, and rotated it well in one direction, but it acquired no magnetism.

7821. Rotated the other way—no action or magnetism.

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7822. A strong solution of *Sul. Soda* was put into a glass tube about $\frac{10}{8}$ of an inch in diameter, 18 inches long and furnished with glass ends to see through. Two platina electrodes were fixed in this tube at one end, so that the current between them should pass across the axis of the tube. Another platina Electrode was fixed in the further end of the tube, so that an electric current could be passed along the axis of the tube. The electrodes were plates about $\frac{3}{4}$ of an inch wide and 4 inches long.

7823. This tube was put into the Woolwich helix (7801) and then the magnetic lines of force thrown on by a battery of 5 pr. of Grove's plates; the circularly polarizing power was immediately made evident. Then the electric current from a battery of 5 pr. of plates was sent across the contents of the tube, but there was no effect on the polarized ray. In this case it was sent across parallel to the plane of polarization (by reflexion), but afterds. it was changed so as to pass vertically and so across the plane of polarization. Still there was no sensible effect.

7824. Then the electric current was sent *along* the axis of the tube and of the helix. Still no sensible effect.

7825. *Lens.* Whilst the sul. Soda solution was in the axis of the tube, tried the effect of a lens (No. 1 Spectacle glass) which, being placed between the helix and the polarizing reflector, could be easily adjusted so as to be full of light to the eye and very brilliant. In many cases it may be very useful to increase the appearance and have a very good effect.

7826. I have had two bars cut off from a large plate of heavy glass, *219*. They are 7 inches long, about $\frac{3}{4}$ of an inch square and are polished at the ends, so as to allow a ray of light to pass and be examined. On putting them in the middle of the Woolwich helix, they were affected by it and produced circular polarization, *but* being cut from a plate, though it had been well annealed, still they shew a depolarizing power over the ray, and each has a black cross, parallel to the sides, i.e. each has four corner portions which depolarize and a black cross which does not.

7827. When the prisms are at the near end of the helix and therefore near the eye, the center of this cross can be adjusted to the visual ray and then the effect of the magnetic lines of force is very good. But when the prism or bar is carried forwd. and removed to a distance from the eye, then the whole of the black cross becomes visible and the effect is seen in it only. But it is seen there very well, and the change of light in the image is perhaps more distinct sometimes in this way by reference to the luminous quadrants.

7828. In these cases, taking away the lens (   ) and so making the lamp image smaller caused it to come better within the cross, and the effect on the eye was more distinct. So the lens will probably be of more use with liquids than with solids.

7829. When the glass prism (7826) was at the near or eye end, and the center of the cross in the line of the ray, and the Nicholl's eye piece a little turned and adjusted about a vertical axis so as to obtain the best extinction of the polarized image, then the effect of the magnetic forces was very good.

7830. And then the lens, brought into its place in the course of the ray, makes the effect very good.

7831. The electric current was going from left to right over the helix—and the rotation was right-handed. So the rotation goes with the current. This is easy to retain in the memory and lucky for me.

7832. By retreating with the eye piece from the helix and bar 2 or 3 feet or more, the dark cross in the bar is very visible.

7833. I placed the glass prism in different parts of the helix but the rotation effect appeared to be the same in amount. It did not seem to be less in the middle of the helix than at either end.

7834. I placed it in the middle of the helix, and two hollow iron cores, one before and one beyond it, in the same line, so that the ray might pass through all three. The rotation force was thus increased, but it was important to pick out a part of the glass which did not depolarize.

7835. *Air.* Adjusted the lens to its situation (7825) and then looked through *air* in the Woolwich helix, but could perceive no effect.

7836. Water in a *brass tube* produced as good an effect as in glass.

7837. This brass tube was then put into a good iron tube full $\frac{1}{8}$ of an inch in thickness in the side, and longer than either the brass tube or the helix. Still the water shewed the rotating power, as much perhaps as before.

7838. Hence such a means may be very useful to examine the state of spaces near iron, etc. etc.

7839. Placed a bar of Iron in the Woolwich helix and looked close along it in the air. Could perceive no effect.

7840. Arranged a narrow glass tube containing *water* so that it could be placed either at the axis of the helix or on one side, so as to examine the different parts; but I found no difference. The effect was sensibly the same in all parts, as was indeed to be expected from the law of internal forces.

7841. Placed the iron bar in the helix (7801) and then this small tube upon it. I could find no difference certainly; perhaps there was a little less effect, but if so, it was only a small difference. Ought to use a greater bar and also a tube quite internal.

7842. *Solution of Tartaric acid* in the narrow tube (7840). Has its own rotating power. Then the magnetic rotation added to it in the Woolwich helix and very well shewn. The natural rotation of the Tartaric acid and also that of the magnetic force were here both right handed (7831).

7843. Fastened on caps and plates of glass to the tube with very thick *gum*, and then experimented with the following fluids.

7844. *Alcohol absolute.* Shewed the rotating force but not so good as water.

7845. *Ether.* Also shewed the effect but was less than Alcohol.

7846. *Camphine.* Possessed of itself a fine left handed rotation. Was also very well affected by Magnetic force, but this was *right hand rotation.*

7847. So all these bodies can be affected in the helix as between the poles of the magnet (7504, 7605).

7848. Beautiful identity of the forces of the helix and of the magnet in confirmation of Ampère's general views.

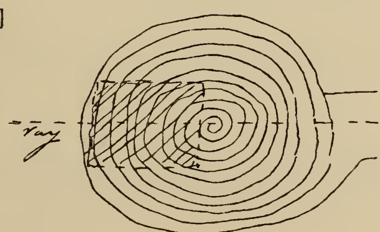
7849. *Apatite.* Put some that becomes very luminous by heat into the helix—closed up the end and made the interior very dark. Then put on the magnetic forces, but could perceive no trace of the evolution of light.

7850*. Arranged two *flat helices* of copper wire so that the polarized ray could pass between them—and so that a piece of heavy glass could be placed between them, either symetric to the axis, or on one side of it or the other, as in the figure; and then sent the Electric current through the helices. When the current in both helices had the same direction they strongly attracted each other, and when they had contrary currents they repelled each other. But in neither case, nor in any position of the glass, could I observe any effect on it.

7851. The experiment in some respects corresponds to that where the same or opposite magnetic poles were on opposite sides of the glass (7601). But here we could pick out *parts* of the rings of currents whilst there we could not, and as yet do not know certainly how they exist as to size and place.

7852. When glass and water had rotation power given to them—

* [7850]



removed the eye piece to different distances but found no difference in the amount of rotation; i.e. no further rotation of the ray occurred after it left the substance—no permanent rotation was impressed on it.

7853. The Magnetic forces have no visible action either on the ordinary ray of light or on the depolarized parts (7827) of a polarized ray.

7854. *Magnetism of the Earth* as *derived from light of the Sun.*

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7855. The seven inch bar of heavy glass (7826) was put into a small short helix, i.e. wide enough to take in the bar, about 9 or 10 inches long and made of 4 coils of thin ordinary wire. The pressure of the helix on the angles of the bar distorted the form of the black cross (7826), but the power of the helix to give rotating force to the heavy glass appeared to be but small, the battery being 10 pair of Grove's plates.

7856. Put the short piece of heavy glass (7807, 7743) in different parts of the same helix. It produced more rotating effect when in the middle of the length of the helix than when at either end inside.

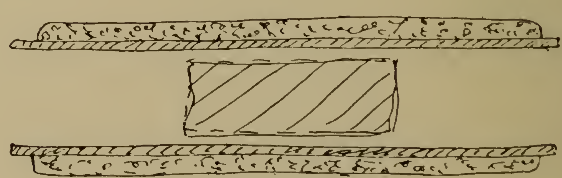
7857*. I have a short helix consisting of 2 coils of copper wire upon an iron tube as a core, and the size of the figure[1]. The short prism of heavy glass (7807, 7743), being in the middle, was more powerful in rotation than it would have been in the helix without the iron, and it appeared to be equally effective whether the glass were in the middle or near either end.

7858. The small prism put into the middle and in different places at and near the ends of the Woolwich helix (7801). There seemed to be no sensible difference—it was as good in the middle as at the ends. It was apparently better in the axis than near the side of the helix. But any effect of that kind can be better ascertained with a tube of liquid.

7859. The 7 inch prism of heavy glass (7826) was put into different parts of the Woolwich helix (7801). Its effect seemed to be a little better in the middle than near the ends—and it seemed

[1] Diagram is reduced to $\frac{3}{4}$ scale.

* [7857]



to be very equal whether in the axis or at the side; but with these pieces of glass which are almost certain to depolarize light somewhere, it is very difficult to pass the ray at all times through the same part of the dark cross, or in exactly the same direction.

7860. Put the iron core of our cylinder magnet into the Woolwich helix and the 7 inch prism of heavy glass between the core and the helix in different parts. It seemed to have the best effect at the eye end—next best effect at the middle distance and least effect at the further end—but the depolar[iz]ing parts make these observations difficult.

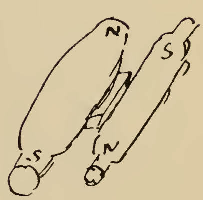
7861. With uncovered copper wire $\frac{1}{20}$ of an inch thick, made a helix in close spirals upon a glass rod 0·3 of an inch in diameter—the helix was one series of spirals and 20 inches long. This was put into a large glass tube filled with water, so that the current of 10 pr. of Grove's plates might be passed through the helix and the ray passing both through the middle and close on the outside of the helix be observed.

7862. The water within the helix received a righthanded rotation with a right hand electric current (7831). But there was no signs of rotation in the water outside of the helix.

7863. I easily obtained the effect of heat here (7787) within the helix and also certain effects of a similar kind due to the same cause on the outside of the helix.

7864. Placed the Woolwich helix (7801) and our large one (7795) in a line, and then placed the long helix (7744) within them and connected them so that the current passed through all in succession; they being so as to have all their spirals in the same order, that one powerful helix might be formed around the *air* in the small central tube. The polarity of all the parts was in the same direction, but I could see no signs of rotation in the air. At times I thought I saw symptoms, but could not determine the fact in the affirmative.

7865. Placed the Woolwich helix and ours (7795) side by side and the 7 inch prism (7826) of heavy glass between them—could perceive no effect on the glass. Placed the iron cores in the two helices, and now there was a slight effect, the glass having a little left handed rotation, and the polarity being as above[1].



[1] See diagram.

7866. On reversing the current so that *all* the poles were changed, the glass then had a trace of right handed rotation.

7867. Then the direction of the current in the smaller helix was changed, so as to endeavour to place the magnets with similar ends together, but in fact the Woolwich magnet was so powerful that it still kept the polarity of the iron core of our magnet the same in quality as before and as marked above. At the same time, the glass had more left handed rotation than before.

7868. Three *iron* tube[s] each 27 inches long and $\frac{1}{10}$ of an inch thick in the sides and of such diameters as to go into each other were prepared, and the smallest supplied with glass ends and filled with water. This placed in the axis of the Woolwich helix, the water had a certain amount of rotating power. When this tube was placed in the next larger, and both put into the axis of the helix, the water had more rotating power than before, as if thus far the iron had increased the energy of the lines of magnetic force. When the two were put into the third tube and the whole arrangement placed in the axis of the helix, then the water had fallen again in power, but still possessed a marked proportion.

7869. The short prism (7807, 7743) of heavy glass was examined as to its power when in the second only of the above iron tubes, or when in the second and that also in the third. I think it had less power in the two tubes than in the one tube.

7870. A lens may be occasionally employed at the eye end of the arrangement for the purpose of magnifying the apparent effect.

7871. A magnetic needle put under a glass jar. A helix placed at some distance and at right angles to it, the helix magnetised by an Electric current, and its effect in deflecting the needle observed. When all stationary, put the 7 inch bar of heavy glass into the helix, but could not perceive the slightest additional effect on the magnetic needle.

7872. Thus another of the powers of nature is *directly* related to the rest, or rather another form of the great power is distinctly and directly related to the other forms; or perhaps rather the great power manifested by particular phenomena in particular forms is here further identified by its direct relation in the form of light to its forms of Electricity and Magnetism.

7873. Consider the case of a Magnet rotating upon its own axis by a current passing through it, and my form of expt. The mag. curves evidently act through iron already filled with force—as is shewn by the water in center of an iron core.

7874. Have had a new horse shoe magnet made out of the half link that Mr Enderby gave us. The iron is $3\frac{6}{8}$ inches in diameter and its axial line 46 inches in length. Two compound coils are placed round each leg. Each coil, being 16 inches in length, consists of three superposed wires of copper of an inch in diameter. The wire, after passing round one leg, then crosses over and goes to the next. The first wire on (that next the iron) is $166\frac{1}{2}$ feet in length—the second is $172\frac{1}{2}$ feet and the third or outer $182\frac{3}{4}$ feet in length. They are now connected by clamps as to make a total length of $521\frac{3}{4}$ feet of coil.

7875. The iron is not the very best, nor has it been annealed. It is on a board, and the whole together weighs lbs. 238. Shifting iron poles have been attached to it for the sake of varying the distance between them.

7876. When this magnet was excited by 5 pr. of Grove's plates it easily held a half hundred weight against the front of the face, hanging in a very bad position. Nor could I pull it off. Its power increased from one pair up to ten pairs of plates, and so much each time as to shew that the iron had capacity for the application of much more wire or voltaic power.

7877. Was excited by ten pair of plates as wanted, and further experiments made with it.

7878. *Gases.*

Carbonic acid, carbonic oxide, Air, ammonia, Olefiant gas, sulphuretted hydrogen, Sulphurous acid gas and muriatic acid gas gave no effect. The doubtful shade observed of certain was most likely due to the glass of the bottle.

7879. Borax neutralized by Tartaric acid and concentrated till very dense rotates well to the right hand by itself and gives good colours. The power thrown on by the magnet adds a small amount to this, or rather abstracts a small amount, for the magnet rotates to the left hand in its present position.

7880. Strong solution of Tartaric acid and Boracic acid. Boraxated tartar and other solutions gave their particular effects, but no[t] so highly altogether as I expected.

7881. With a piece of heavy glass ascertained clearly that the amount of rotation increases really with the increase of magnetic power and with the increase of length in the glass.

7882. *Rock crystal*, in crystals 4 inches across, or in smaller crystals or in the cubes (   ) gave no signs of rotative force, though placed in such positions before hand as least affected the polarized ray.

7883. *Iceland Spar*. Neither the cubes (   ) nor natural rhomboids shewed any effect. The rhomboids placed thus did not depolarize the ray (polarized in a horizontal plane); but still there was no effect on the light by the Magnetic forces.

7884. *Sulphate of Baryta*—a large crystal—no effect.

7885. *Sulphate of lime* or selenite—no.

7886. *Carbonate of Soda*—polished sides—no.

7887. *Crystals of sulphur*—in oil of turpentine in a tube. Could perceive no effect, but the crystals [did] not let much light through and experiment undecided.

7888. *Rock salt*—yes, plainly enough.

7889. *Alum crystal*—I think yes, but doubtful.

7890. *Camphor*. Melted and in a tube about 1 inch in diameter. When fluid has much rotative force of its own. Also the rotative effect of the Magnetic forces very doubtful. Now this might be because the mass was small, i.e. the length of the course of the ray. Or because the natural force of the camphor on the ray prevented the reduction of the light beyond a certain amount, and so the small effect of magnetic force could not be observed. Or because the Camphor retained in the fluid state part of its crystalline relations, and these incompatible with artificial rotation —but that is not very likely, I think. (Sulphur, 7669).

7891. *Fuzed nitre*—did not rotate, but else it also did not shew the power: at the same time being in a tube about 1 inch in diameter. The ray here was very short.

7892. Made certain very rough measurements thus. The aperture between the magnetic poles was made constant $2\frac{1}{2}$ inches. Then different bodies were brought into the line of the magnetic forces, the fluids being in bottles, and the eye piece adjusted to a scale before and after the magnetic force was put on; i.e. the effect was adjusted before, then the magnetic effect produced, and then the eye piece moved until this added effect was compensated for. The battery power was always the 10 pr. Grove's plates.

7893. The eye piece handle had a prolongation about inches long as an index, and this index at the extremity moved before an inch rule, the $\frac{1}{8}$th of which I counted for degrees. So the ray length of the substance and the number of degrees give a rough expression of the rotating force.

7894. *Oil of turpentine*—in bottle $\frac{35}{16}$ of inch in diameter—adjusted the eye piece in air, then introduced the oil of turpentine and adjusted again. The number of degrees in three observations was 19·8: 18·2: 18·2: and the average was 18°·7—for the natural power in the above length.

7895. *Heavy Glass*—a piece of which the ray length was $\frac{34}{16}$ of an inch. The induced rotation was 9°·1 or ($\frac{1}{8}$th of an inch (1893)) for an average of three observations.

7896. *Flint Glass*—a plate—ray length $\frac{36}{16}$ of an inch. The induced rotation was 4°·5 (or $\frac{1}{8}$th) as the average of three experiments.

7897. *Rock salt*—ray length only $\frac{14}{16}$ of an inch. It was difficult to obtain a result here, perhaps 2°—or perhaps that too much. The effect distinct but small. Being crystalline, the piece of salt requires arrangement that it may not depolarize the light.

7898. *Water*—ray length $\frac{38}{16}$ of an inch. The induced rotation was, on an average of nine observations, 1°·7.

7899. *Alcohol*—ray length $\frac{35}{16}$ of an inch. Could not get the measure by my rough arrangement, but was less than water.

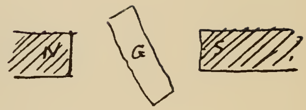
7900. *Ether*—ray length $\frac{35}{16}$ of an inch—could not get the measure, but was less than Alcohol, I think.

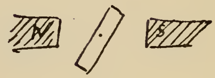
7901. Now assuming that the effect is proportionate to the ray length of the body in lines of equal magnetic force, 100 parts of each of the above bodies would have the following expressions in degrees[1].

Oil of turpentine	100 ray length	produce	rotation of	(11·8)	53·4	by natural rotation.
Heavy Glass	”	”	”	(6)	26·8	by the given Magnetic force, these Nos. therefore express roughly the ratio of these bodies.
Flint Glass	”	”	”	(2·8)	12·5	
Rock salt	”	”	”	(2·2)	10	
Water	”	”	”	(1)	4·5	
Alcohol	”	”	”			
Ether	”	”	”			

[1] The numbers in brackets are in pencil in the MS.

7902. The bar of heavy glass (7807), $1\frac{6}{8}$ of inch long and of an inch square, was suspended by cocoon silk in a glass jar on principle as before (7743) and placed between the poles of the last magnet (7874). When it was arranged and had come to rest, I found I *could* affect it by the Magnetic forces and give it position; thus touching dimagnetics by magnetic curves and observing a property quite independent of light, by which also we may probably trace these forces into opaque and other bodies, as the metals, etc. The nature of the affection was this. Let N and S represent the poles and G the bar of heavy glass. It was arranged so as to stand as in the figure. Then on making N and S active by the Electric current, G traversed not so as to point between N and S but across them, thus, and when the current was stopped the glass returned to its first position. Next arranged the glass when stationary thus, then put on power, and now it moved in the contrary direction to take up cross position as before; so that the end which before went to the left hand now went to the right, that being the neutral or natural condition. When the current was off, the torsion of the silk brought the wire to the oblique position.





7903. If 1 was the natural position, on making the poles magnetic the glass swung into position 2 and on to position 3. If contact was continued after 2, the tendency to 3 was diminished, i.e. was less than if there was no current. If, whilst swinging, contact of circuit was continued during vibration from 1 to 2, and broken from 2 to 3, then renewed from 3 to 2, and broken from 2 to 1, the bar was soon sent spinning round the whole circle.

7904. If, when freely vibrating, contact was made as the bar went from 2 to 3 or 2 to 1, and broken as it went from 1 to 2, and 3 to 2, the motion of the bar was quickly stopped.

7905. In fact the bar was under command just like a galvanometer needle, and the best way of stopping its vibrations and bringing it to a state of rest to apply the force of the magnetic curves according to these laws.

7906. How well this shews a tendency to specific action, for if the air and the bar were alike, no motion of the bar would take place. Or if the bar were a globe, no motion would take place.

7907. How well this shews the new Magnetic property of matter.

7908. How curiously it contrasts with the results and conclusions of those who say that all bodies are magnetic and point, since the great class of dimagnetics, being nearly all bodies, instead of pointing, tend to stand equatorially.

7909. How will this influence terrestrial relations and solar relations?

7910. May now examine Specific property of dimagnetics by this form of result.

7911. Try effect with ordinary magnets.

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7912. Made certain experiments to-day on the top of the house with sun light and helices. The day not good; still, some beams came down from Sol.

7913. A Glass tube 28 inches long and 1·4 inches external diameter had brass caps with flat glass ends attached by gum, the tube being for oil of turpentine. 20 inches of this tube was covered with a helix of one set of spirals containing 220 feet of covered copper wire $\frac{1}{26}$ of inch in diameter. This tube was filled with clear oil of turpentine and then put into the large Woolwich Magnet (7801), the ends of which were connected with my very delicate Galvanometer (from Matteucci).

7914. Then a ray from the sun reflected by a silvered glass mirror was sent through the oil of Turpentine. But neither by allowing it to go through, or stopping it, or reversing it, could any action be found at the Galvanometer.

7915. Next a ray was polarized by passing through a bundle of glass plates and treated in the same way—no effect.

7916. Next, two ordinary rays were sent in, one at each end, and cut off at once or intermitted or alternated, but no effect.

7917. Next, polarized rays were sent in at the two ends and changed in all these ways. Also sometimes the planes of both rays were in the same plane and sometimes one was perpendicular to the other, but no effect on Galvanometer.

7918. Then the Galvanometer was attached to the ends of the tube helix (7913) and the expts. with Polarized light repeated—no effect.

7919. Now 10 pr. of Grove's plates were connected with the

Woolwich helix—next, the Galvanometer wires were connected, and lastly the rays, common and polarized, were sent in, but still no effect.

7920. So that with this sunlight—and oil of turpentine either *rotating alone* or *made to rotate* more by the Voltaic battery, still no action of the ray of light evolved a current in thė other helix.

7921. Tested the correctness of all the junctions by a little thermo-arrangement—all good.

7922. Should like to try a finer sun and also water as the dimagnetic (7808–21, 8683–93, 95–706).

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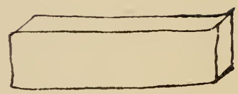
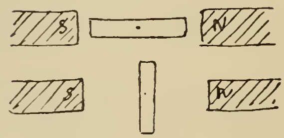
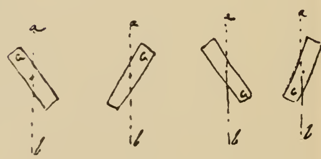
7923. Worked with the large horse shoe magnet (7874) excited by 10 pair of Grove's plates, and dimagnetics of various kinds suspended by a line of silk as before (7902), repeating and verifying that experiment. One end of the same prism of heavy glass (7807) is a little thicker than the other, only by a small portion [illegible], and this was made a marked end.



7924. First of all the great fact of the 4th (   ) was verified. If the glass were placed in either of these positions, i.e. parallel or perpendicular to the magnetic curves, then on making magnet it did not move; but if the glass was inclined to these positions, then it moved on to take up the cross or equatorial position. Also whichever way the marked end of the glass was, that made no difference, but the prism moved directly into the equatorial position *a b*. Shewing that that was the state of rest under the powers, and that the effect was not due to particular ends of the glass but to the whole mass.





7925. This is a fine quality of the dimagnetic[s] in illustration of the mode of magnetic action through them. No longer indifferent bodies.

7926. Here a needle which points East and West as respects magnetic lines of force—and so theoretically could have such a needle.

7927. It moves until the circles of force in it are in the same plane as the inducing currents of the Magnet. In this respect therefore it points axially.

7928. But still, why the equilibrium is best held in this final position is not shewn by that alone, and requires some force acting across the curve.

7929. Settles as De la Rive's ring does, in same plane. If it were a plate of glass, shew that exactly.

Worked with various substances:

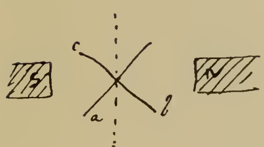
7930. *Borate of lead*—a wedge shaped fragment, of course long in its general form. Presented exactly the same and all the phenomena of heavy glass.

7931. Two pieces of Borate of lead, each squareish but together making up a long arrangement, were put into a little folded paper tray and subjected to the magnetic action. They acted precisely as one long piece.

7932. *Heavy Glass.* Three pieces of heavy Glass arranged in the same way had exactly the same effect. So pieces will do very well.

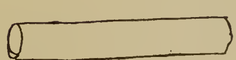
7933. *Rock crystal.* A natural prism about 2 inches long half an inch in diameter was used; it obeyed exactly as the heavy glass.

7934. So though I could not find the effect by the optical test because of crystallization, it easily appears here and probably will do so with all crystals.



7935. Observed with it that if from its rest position at *a*, it be pulled magnetically up to the equatorial position and then be allowed to pass on to *b* by its swing, it will on relieving, by torsion alone, go back to *a* and on to *c*, and *beyond it*, perhaps to the equator position—as if there were some kind of reaction on letting down the magnetic force—but doubt it and must repeat.

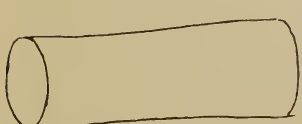
7936. Employed three little cut cylinders of Rock crystal placed end to end, and they answered exceedingly well.



7937. *Flint glass*—a small rod of it shewed the phenomena very well.

7938. Two such rods side by side did better. The more matter there is, if in the long form, the better the effect.

7939. Bodies having less power than air, if such there be, ought to arrange themselves along the Mag. curves, because of the air which surrounds them. So also if a body have less power than water, then *in water* it ought to arrange itself along the magnetic curves. Can try this probably with Alcohol or Ether.



7940. *Sulphur*—a cylinder of this body did exceedingly well and as heavy glass—acts so well probably because of its size.

7941. *Sealing wax, red.* Here a good case in distinction of magnetic action from the new action, for whichever way it stood, it was, on magnetic action being induced, at once drawn up into a line between the poles, just as a piece of iron would be. It was magnetic, doubtless from some ferruginous matter in it, and made a beautiful contrast with the other bodies.

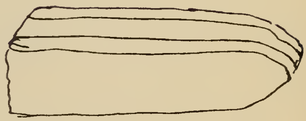
7942. Amusing to think of the proofs that some have brought forward that all bodies are magnetic as iron, and point so. The correction given by the truth of nature is not merely an answer of no action, but of an action in striking contrast with that assumed (7981).

7943. *India rubber.* Two prisms were cut from one thick piece of natural rubber. One, being tried, proved to be magnetic after the manner of sealing wax but not so strong.

7944. On trying the other, which had been WELL washed, it was found not magnetic and to point well as heavy glass.

7945. So must be careful of my small quantities of magnetic matter.

7946. *Sulphate of lime*, crystal, was in three plates standing side by side. Obeyed as heavy glass and very well.

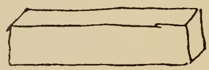


7947*. *Asbestus*, a natural bundle of this body was magnetic as the Wax and more so.

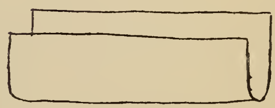
7948. *Iceland Spar.* Two rhomboids placed end to end as one long piece; obeyed as well as heavy Glass—so crystals settled.

7949†. A flat rhomboid, being like a thick plate, was hung up edgeways and acted well—moving as a plane into the plane perpendicular to the magnetic curves.

7950. *Jet.* A small piece of jet was not magnetic, and obeyed the force of the curves as heavy glass.



7951‡. *A small thin glass tube*, intended to hold liquids if its presence might be allowed, was tried by itself. It acted and took up position, though feebly because of its small mass. Still, in using them, must remember the amount of action due to the tube.

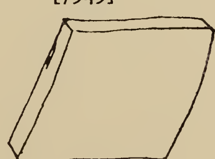


7952. The piece of paper used to hold fragments tried by itself. It was *not magnetic*, and shewed scarcely a trace of heavy glass action.

7953. *Foolscap*—half a sheet torn into 4 strips and rolled up into a cylinder proved to be magnetic, as wax or iron (8052).

* [7947]

† [7949]



‡ [7951]

7954. *Muriate of Ammonia*—some long fragments broken from a mass were selected and put together as a little bundle about 2 inches long. It acted very well, as heavy glass, and was not at all magnetic.

7955. *Nitre*—a crystal about 2 inches long and half an inch thick—not magnetic—acted as heavy glass.

7956. Easy to see if a body magnetic or not. Being at an Angle of 45° with the Magnetic line or thereabouts, and the action put on, if it is magnetic it *goes one way*—if not magnetic it *goes the other*.

7957. *Litharge fuzed* and *in two pieces*. Not magnetic. Acts as heavy glass—but not so well as I expected. The pieces were not arranged well end to end, supposing there is action of one piece on the other.

7958. The oxide of iron which is in it is no doubt fully oxidized, i.e. above Magnetic condition. So bodies cleaned with litharge, as silver, gold, etc. most likely free from magnetic iron on every account—not so platinum, coins, etc.—nor is iron without action ever.

7959. *Sulphate of Soda*. A large crystal—not magnetic; was as heavy glass.

7960. *Sulphate of Nickel*, crystals—are fine crystals—but they are magnetic, and from the appearance of the crystals I suspect they are properly so of themselves. If so, probably Sulphate of Iron (proto) will be magnetic also. Curious to ascertain this point.

7961. I will now note certain experiments made with one of the metals, *copper*, with very striking and new results, and which doubtless belong to all the metals: time will shew.

7962. The *copper* was a small bar about 2 inches long, 0·33 of inch wide and 0·2 in thickness. It was set up edgeways in the stirrup after being well cleaned with sand paper.

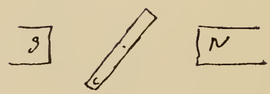
7963. Let the dotted line *ab* always represent a line perpendicular to the magnetic lines of force, and let it be always understood that the end of the bar marked with *c* is the *same end* of the bar—a marked end; and to begin, suppose the copper in this position before the Magnetism is on. On putting on the Magnetism the rod does not take up the equatorial position, but turns the end *c*,

advancing towards S, and gradually comes up to this position, and stops there. It goes also steadily up to its position of rest and not a jot beyond it, though in one part of its course its momentum merely is enough to carry it farther on. This *coming up to a position* like that shewn, but not constant, being sometimes more sometimes less, and its manner of coming up, is very striking. All this time the magnetism is supposed to be constant.

7964*. Next, as to the cutting off the Electric current and letting down the magnetism. *This does not leave the bar where it was* but a very singular effect takes place, for the moment the contact is broken the bar is driven round in the other direction nearly a whole circle, and then slowly returns by the torsion vibration, and after many oscillations takes its natural place of rest, returning nearly to the position from whence it set out. Fig. 1 represents the first swing on being driven from its constrained position. Fig. 2, its first return by torsion; fig. 3, its second vibration by torsion and Fig. *n*, about the final place of rest.

7965. It is manifest that neither the first advance up to a position nor the second impulse in the opposite direction are either of them ordinary magnetic actions such as that of iron, or the sealing wax (7941); and they are in appearance equally removed from the action of the heavy glass and other such bodies, being non- or bad conductors.

7966. Now put the copper thus, and made the poles magnetic. *c* immediately moved up towards S, exactly with the same appearances as before and forming much the same angle (7963); and then on breaking contact it was thrown off and back, repeating all the former phenomena but in the reverse order.

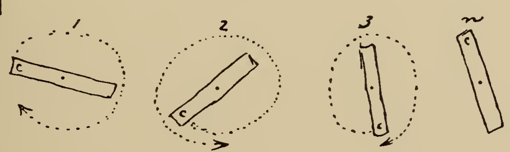


7967. Instead of waiting until the copper bar was quite still, I let it vibrate by the torsion of the silk film, and found that that was not of much consequence to the general effect; for if the contact was made as the copper bar came up to the position figured above[1], then all the other effects, i.e. the advance to a certain point, and fixture there, and final revulsion followed just as before.

7968. If the bar is nearly parallel to the equatorial line at the moment the magnetic action comes on, then the first pull up towards the direction of the Magnetic curve is not so strong as

[1] See par. 7966.

* [7964]



when the bar forms an angle of 45° with that line, nor is the after revulsion so strong.

7969. If the bar be in the equatorial line, there is neither first pull nor after revulsion. Or, if the bar be in the magnetic lines, i.e. parallel to them, there is neither first pull nor after revulsion. These are two positions of equilibrium or non action.

7970. The two other positions intermediate to these, and forming angles of 45° with the magnetic curve and equatorial line, are the two positions of greatest force, both in the first pull up and the after revulsive action.

7971. If when the copper bar was moving gradually by torsion the magnetic contact *was made and broken in an instant*, then the effect both of making and breaking was very striking—that of making was a *sudden jar* or *stop*, and that of *breaking also*; but the latter was not a strong revulsion as before—for the bar simply resumed its course of vibration as if nothing had happened. The results of the making and breaking were in fact two impulses, but *opposite in direction* and *equal in amount* under these circumstances, and so being added on to and again taken off from the original state of the bar, leave it at last in that state. Very different all this to mere magnetism.

7972. If the bar be originally thus and is then by a *prolonged* magnetic contact brought to this position, then on breaking contact the revulsion take[s] place and the bar moves clock fashion: but if the contact be renewed at any moment, as when the bar has attained this position, or this, the bar is instantly arrested and is either held in the place it was caught in or else returns a little towards the magnetic line, but not to the first position in which the magnet brought it. Also the quicker after breaking contact it is renewed the less does it return—the more decidedly is it seized and held in the place where the magnetic force finds it. Being let loose the second time it is not so strongly revelsed[1] as before. So by managing the contacts, it may be caught in any

[1] In the MS. this word appears to have been altered from rev*u*lsed to rev*e*lsed, but subsequently 'revelsed,' 'revelsion' and 'revelsing' are written unmistakably (pars. 7972, 4, 6). Note that this follows Faraday's use (par. 7967 *et seq.*) of 'revulsion' in the literal sense of a drawing back or away. *O.E.D.* does not admit 'revelse,' but gives as obsolete a verb 'revel' with the meaning 'to draw back.'

position and *kept there*, being revelsed less and less when let loose, according as the position is nearer to that of the equatorial line. If it be caught in the equatorial line, it is kept there and has no revelsion afterwards: so that this position is a striking position, and the after action diminishes up to it and ceases at it.

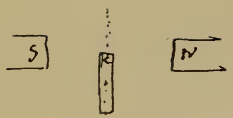
7973. After an absence of $\frac{3}{4}$ of an hour found the bar in this position and still the marked end being on this side. A momentary contact and break (7971) gave just the little pull and reaction and left the bar *still*, as it found it. This is not like *magnetic attraction of iron*, for there would have been *a strong and continuous swing round.*

7974. Contact for a *prolonged moment* and then broken, first moved the bar up (left handed) about the $\frac{1}{10}$ of an inch (radius 1 inch) and *left* it there, *not revelsing it.* When a prolonged contact was made, the bar was brought up to a place of rest and on breaking contact revelsion occurred.

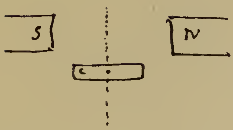
7975. If the revulsion were ever so strong, could stop it at once by contact, and now and then there was also a little return of the bar towards direction of the magnetic line. On breaking contact, the revulsion belonging to the position it then had (7964, 7966) was manifest. When caught in the equator, it was kept there and left there.

7976. When allowed to swing *by revulsion* from one side of the equatorial line to the other side, and then magnetically caught, it seemed to be extra well urged on to that side, giving this general result. Suppose 1 the natural position and contact made; it moved *left handed* a little with a certain force and was at last held in a certain position: then on breaking contact it would be revelsed and if caught magnetically in position 1, could now be *kept there.* On breaking contact it would now be revelsed *right hand* fashion, but if when at 2 the magnetic force were again laid on, the bar would continue to move on towds. N *more powerfully* than if in its natural state, apparently from its having been in the reverse state previously on the *other side* of the equator, and far more powerfully than if it had been placed in 2 with a condition like that it had acquired in 1. *That is*, the states on the two sides of the equator are complementary in some respects to each other—for when least attracted by S, it is in a corresponding position most attracted by N, and vice versa.

7977. Also there is a sort of exhaustion or charging, so that as regards direction *one way* the copper can be made more or less capable by the process described (7976).



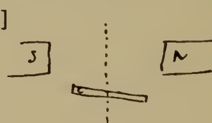
7978. When the point of suspension for the copper was in the equatorial line but out of the magnetic axis, then there were two positions in which it was not moved either by making or breaking contact—namely when parallel or perpendicular to the equatorial line. But when it was in any other position, as thus*, it obeyed the resultant of magnetic forces as before; only as the parts of the straight bar did not accord with the form of the resultant, was not placed symmetrically to the forces, or have forces of equal intensity in its various parts, there were differences of degree in action and differences of position: but all under the general result expressed above.



7979. This action of non magnetic metals is beginning to come out pretty clear, but in the first place, how distinct it stands from any ordinary magnetic action. When the Sealing wax (7941) or Asbestus (7947) or paper (7953) was in the magnetic field, if the cylinder was first thus on making contact, it was drawn into the magnetic line pointing from one pole to the other; or if its force of vibration carried it beyond, it returned to that line and found its place of rest there. But the non magnetic metal never goes up to that line, for it finds a place of rest before and never swings past it. Again, if when the *magnetic matter* is in its place of rest the current be stopped and the magnetic force removed, the magnetic piece of substance is *left quiescent*: but if the non magnetic metal be in its place of rest and the current stopped, a powerful revulsion takes place and the metal is anything but quiet.

7980. Again, if when the magnetic substance stands oblique the current be laid on and the substance pulled towards the magnetic axis, and if as it reaches that position the contact be broken, the body *swings by* the pole of the magnet with great power, and can by a proper set of touches be brought into powerful revolution. *But*, with the *non magnetic* metal, no kind of touching can *get it past the pole* of the magnet; it stops before it comes up to it and the revelsion sends it *back again*. It is true a strong revulsion can make it swing past the opposite pole, but that is a result of momentum and the state acquired at the first pole and is in the

* [7978]



contrary direction to the movement induced upon mere magnetic matter.

7981. Those therefore who say that all bodies are magnetic *after the manner of iron* and *point*, are as badly off here as with the non conductors (7942).

7981½. These results give an excellent test for the presence of ordinary magnetic force in bodies. Thus the sealing wax or paper, etc. are magnetic and differ from unmagnetic bodies; they are not like pure non conducting dimagnetics, for they do not rest in the magnetic equator, and they are not like pure conducting dimagnetic[s] for they do not start back again, etc. But they are like iron, nickel and Cobalt, for they go to the magnetic axis and hang there or else continue the direction of their revolution (8213).

7982. Must consider how these results with pure metals are related to Ampère's old results.

7983. Must consider also how it is connected with my first inductive and tonic state.

7985[1]. How connected with the rotation of the magnet on itself.

7986. Has it any connection with the motions of mercury either in an Electrolyte under decomposition or in Davy's heaps?

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7987. In reference to the copper effects of yesterday, observe the following explications of the actions.

7988. The copper is not magnetic in the ordinary manner.

7989. The copper is apparently attracted at first or brought up a little, because as the Mag. curves rise in force they induce, according to my old principles, currents in the copper in the *same direction* as those moving in the helices (and iron of the magnet), and so contrary Mag. poles are opposite each other and there is attraction.

7990. That the attraction ceases is because, as soon as the Mag. curves have done rizing—all is quiescent in the copper. The currents and therefore their magnetism continue only whilst the curves rise or whilst there is motion, and the rising ceasing, then the apparent attraction ceases. Then there is rest.

7991. When the current is broken and the magnetism falls (which

[1] 7984 is omitted in the MS.

requires time), then the contrary currents are induced in the copper, so that its magnetism (momentary) is changed, whilst that of the Magnet, though falling, is still the same in kind. Hence the repulsion observed.

7992. When the making and breaking are instantaneous, then the two induced states in the copper are nearly equal in intensity and contrary in action, and as the Iron has not had time to have its state fixed, the internal change there also is instantaneous and all things are balanced.

7993. So shews that by longer contact, not only is the iron raised in Mag. force but it acquires a sort of set which a momentary contact does not give it.

7994. The cause of the neutral position in the equatorial and polar points is evident, being cases of balance of forces in opposite directions.

7995. Also why the angle of 45° is the most effectual position for the phenomena.

7996. Also why the first motion is towards the pole nearest or to the right or left of the equatorial position.

7997. The reason why the revulsion is so much more powerful than the attraction is because the rise is slower than the fall. The rise of the Magnetism is gradual and the conducting power of the copper disposes of the currents; the fall is much more sharp and sudden, and the moment or time of revelsive action more compressed and concentrated, i.e. if long contact made. If a short contact, then both are equal.

7998. Also another reason in addition is[1]

7999. Tried many other bodies in the same manner as the heavy glass or the copper. When they were in fragments, as Starch or Gum, piled them up in a little paper tray in the stirrup or put them end to end so as to make a long arrangement.

8000. Pieces of matter having no action when arranged as a central heap acted very well when drawn out into a long parcel.

8001. In the first place, some bodies were magnetic; thus a piece of red sealing wax was so, pointing and being pulled up to the poles from either side, etc. at pleasure.

[1] A blank space is left in the MS.

8002. *Boxwood charcoal*—very magnetic.
8003. A piece of *burnt bone*—magnetic.
8004. *Shell lac*—magnetic; had been fuzed and was in lump.
8005. *Red lead*
8006. *Vermillion*
8007. *Sul. Zinc in crystals*
} all magnetic and I believe from oxide iron in them—except the Sul. Zinc, and that probably from presence of Sul. Iron (8008).

8008. *Sulphate of Iron* in good crystals is magnetic. Both this salt and the Sulphate of Nickel I think are magnetic of themselves and as salts. Curious to see action of these on light—also action of their solutions and if they also are magnetic.

The following salts in crystals:

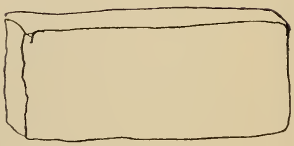
8009. {
Sulphate of Potassa
Sulphate of Magnesia
Alum
Carbonate of Soda
Tartrate of Potassa and Antimony
Rochelle salt or Tart. potassa and Soda

were not magnetic, and all obeyed freely the magnetic influence, taking up the equatorial position like heavy glass (7902).

8010. Citric acid.
Tartaric acid.
Cinchonia.
} These bodies also were not magnetic, but acted as heavy glass. The Cinchonia crystals was put into a little cylindrical thin paper case and acted rather strongly, considering that the form of many small parts is disadvantageous (8098, 8099).

8011. Candle Margaric acid
Spermaceti
Wax from Shell lac
Common Rosin
Caffeine
} not magnetic and acting as heavy glass, taking equatorial position.

8012. The Margaric acid was in a plate and acted beautifully, shewing how well that form and the existence in one piece tells in favour of the success of the experiment.



8013. Starch
Gum arabic
Crust of bread
} not magnetic—all setting as the heavy Glass—the crust very well.

8014. If a man could be in the Magnetic field, like Mahomet's coffin, he would turn until across the Magnetic line, provided he was not magnetic.

8015. *Phosphorus*—one stick only—not magnetic and acted exceedingly well. I think excels in this respect (8051).

8016. *Iodine*—in a glass tube, closed by a covering of paper. The result not satisfactory. It seemed to tend to point—but in one position shewed a little magnetic, as if a particle of ferruginous matter were there. Besides which, the division of the mass against it. Must fuze a cylinder.

8017. *Chloride of lead* in pieces—feeble if at all. It did not seem either Magnetic or as heavy Glass, but I suspect a little magnetism—just enough to hide the other result. Must fuze a pure cylinder.

8018. *White Arsenic* in two lumps. As heavy glass, feebly rather, but here I suspect a little Magnetism, for now and then in certain positions there was a sort of hesitation in the motion.

8019. *Nitrate of lead* in small crystals. Was as heavy glass but not good. The numerous small crystals or pieces bad.

8020. *Acetate of lead*—very feebly as heavy Glass—hardly certain —yet not magnetic sensibly—perhaps is as White Arsenic above (8018).

8021. *Sulphuret of Copper.* Artificial, made from thick copper wire and sulphur by heat. Was not Magnet.—nor as heavy Glass—nor as Metal copper (7962, 7987). Seems to be quite indifferent. Perhaps midway as to conduction and non conduction of Electricity, so as to be between these two bodies. Or perhaps a little magnetic. Make more carefully (8041).

8022. Went to the metals again.

8023. *Copper* (7962). Is as yesterday but very feeble, and probably because the battery, having been long in action, is going down. If it had been this way yesterday, I should not have observed the facts. Must *strengthen the magnet* also.

8024. *Lead* } (8044) In bars also, 2 inches long and 0·33 and 0·2
8025. *Tin* } (8045) thick—were Magnetic, from iron doubtless.

8026. *Zinc.* Do. gives feeble traces of being like copper with a stronger magnet (8042).

8027. *Bismuth.* Not Magnetic. *But* is like heavy glass in all points and on every side—and not at all like copper. Is better than any other thing for shewing the glass position, i.e. the equatorial position (8035).

Is this because of its crystalline character?

" " bad conducting power?

or its thermo electric relation? or what?

What will a more powerful magnet do with it?

8028. *Antimony*. Not Magnetic. Is as *bismuth* but not so strongly; approaches in degree more to heavy glass.

Strange!

8029. *Silver*. Is not like copper—nor like glass or bismuth, and is a little magnetic. Perhaps careful cleaning alone will remove the doubt.

8030. *Arsenic metal*. Moved, etc. much like heavy Glass, but I suspect a little Magnetism on the sides. Then used two other shorter pieces from the same lump and these were clearly magnetic. Must sublime a cylinder of it (8040).

8031. One would have thought that the zinc would have contained iron—but some of the metals seem by other facts to cover up the magnetic property of iron far more than other metals.

8032. Worked again at the magnet but with a good battery of 10 pr. Grove's well charged, to see if there would be any difference in the action of copper, bismuth, etc. by a stronger or weaker battery, i.e. any difference in nature as if bismuth was [? would] rise up to copper or copper approximate to bismuth.

8033. Heavy glass prism, as before (7902).

8034. Copper bar, just as at first, revelsion, etc. etc. (7962, etc.).

8035. Bismuth still as bismuth with this battery (8027), and far better than heavy glass. As of course the revelsion could not be seen at the equatorial position, I broke contact when the metal was at angle of 45°, but could not perceive any signs of revelsion there. So no electric currents or weak ones induced in it. This agrees I think with Babbage and Herschell's results on Arago's motion.

8036. Antimony as Bismuth—and well (8035).

8037. *Silver* appears to be as copper and also a little magnetic. It is as copper because, if there be sudden making and breaking of contact (   ), there are the two jerks and resumption of the previous state, i.e. no after action either of approach to or revul-

sion from the axis. It is so also because, if drawn up to this position and then the current stopped, there is the after revulsion.

8038. But it is magnetic, because if contact be continued, it is steadily though slowly drawn up to this position, or even quite into the axis, and does not stop as copper does (7963) at a place.

8039. Is the silver itself magnetic or not quite pure?

8040. *Arsenic.* The three pieces (8030) examd. by a common magnetic needle—one of the square pieces magnetic in one spot, the other two not. The large single long piece again acted well, as bismuth and antimony (8035, 8036) and quite certainly. The other two pieces put together shewed that there was a little magnetism in them, but the first piece a good piece.

8041. *Sulphuret of copper* (8021). Still as before and very indifferent. Is *not magnetic* and apparently midway between condition of the crystalline and conducting metals.

8042. *Zinc*, as copper and very fairly so (8034, 8026). This piece was in a crystalline state, being made out of a cast plate (8153).

8043. *Platinum* (   ): a piece of thick wire from Wollaston's Ingots, very carefully cleaned—but was magnetic. It is either so of itself or contains iron.

8044. *Lead* (8024) very magnetic, yet did not shew it to the magnetic needle; shews it is through the body and not any spot.

8045. *Tin* (8025)—a little magnetic and is as silver (8037), i.e. the magnetism caused a constant approach when bar near the pole or axial line; but besides that, when at 45°, there is a short pull up and on breaking there is the revulsion, though small.

8046. *Gold.* Four sovereigns well cleaned outside by boiling in caustic potash and afterds. in Muriatic acid, and afterds. a little Nitric acid—wrapped up in examined paper—and tried. Were magnetic very distinctly, and probably from iron. Still, when current was made a while and then broken, there were signs of rather strong revulsion. If pure, would probably be as copper (7962, 8034).

8047. *Palladium.* Some large pieces from Mr Brandt were magnetic—also part of a plate from Wollaston was magnetic. Also a portion reduced from the cyanide of Palladium and used in state from reduction. Still all were magnetic.

8048. *Rhodium*, Wollaston's. Magnetic and probably from Iron.

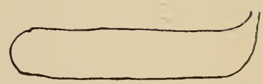
8049. *Cadmium*. Not Magnetic. It shews the copper phenomena but modified. If it were slowly swinging towards the axial line, and the Electric current put on, it would be arrested; and if after a moment the contact were broken, then there would be revulsion. But suppose the contact not broken: then the metal gradually goes to the equatorial position as bismuth does. So that it shews both sets of metallic effects. *Good*.

8050. Repeating the experiment—found that it returned well to the equator if the contact continued.

8051. *Phosphorus* (8015). Two short pieces as one. Still excellent action, as heavy Glass.

8052. *Paper* often magnetic, almost always. It is quite necessary to examine and select the paper that is used for wrapping up things or forming trays for powders (7953).

8053. *Mercury*. In a thin glass tube. This metal was as heavy Glass and bismuth, going to the equator—it was not magnetic. It shewed signs also of the revulsion, for if going to the equator, if contact broken, it went quicker.

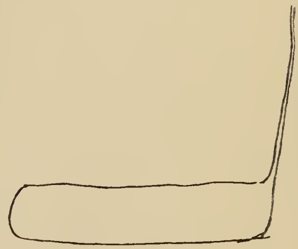


8054. Plumbago
8055. Fluor spar powdered
8056. Per oxide of lead—brown } all were magnetic and I think from iron.

8057. Examd. solutions of salts of Iron, and as I expected, found *them magnetic*, for the crystals of the sulphate were so (8008). They were introduced into thin glass tubes about two inches long and easily shewed their power; the solutions were

Proto sulphate of Iron
Proto muriate ,,
Per sulphate ,, } in water.
Tincture of Muriate of Iron.

All were Magnetic, both proto and per salts.



8058. *Per oxide of Iron* in powder in a tube was also magnetic, but not quite sure that, as it has been heated, there may not be in it a little protoxide.

8059. *Muriate of Nickel*, solution of, in water—shews the power of heavy Glass when near the equator, but at 45° it is nearly motionless, as if a trace of magnetism, enough to balance the equatorial tendency. Still, it does not go to the pole or axial line, so is only little magnetic if at all. I do not see how its neutral condition can be explained by any thing but a balance of the two powers.

8060. *Sulphate of Nickel* solution—is in a slight degree magnetic—enough so to hide the equatorial tendency.

8061. Sol. *Muriate Cobalt*—is magnetic; but must look for iron.

8062. *Rock salt* is as heavy Glass.

8063. *Siliceous sand* in a glass tube—was in the least degree magnetic, but it obeyed as Heavy glass, but only weakly, scarcely sensible as compared to rock crystal. This may be either from a certain amount of counteracting magnetic force or from division. I think breaking up into pieces much interferes with the power.

8064. *Marble* powder is as heavy Glass, but feebly, perhaps because of division.

8065. Water
8066. Alcohol
8067. Ether
8068. Nitric acid
8069. Sulphuric acid
8070. Muriatic acid
8071. Olive oil
8072. Oil of turpentine

} All act well as heavy Glass.

8073. *Barley Sugar*—as heavy Glass.

8074. *Lime wood*, a plate—not Magnetic—acts well as heavy Glass.

8075. *Dried Mutton*, dried years ago—*not Magnetic*. When near the equator position, obtain clear indication that it acts like heavy glass. Yet the piece had little substance, being very light.

8076. *Dried blood.* Not magnetic—nor as heavy glass—perhaps a balance of the two here (8059). I did at one moment catch the heavy glass effect but only weak.

8077. Must try recent fluid blood.

8078. *Dried beef*—dried years ago. Was magnetic—but no care in that respect had been taken in old time.

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8079. Optically examine and *consider* the crystals and solutions of salts of iron—for they are magnets though weak ones.

8080. Also salts of Nickel, especially the sulphate.

8081. So all things *point*, where there are differences of quality, either along polar axis or along equatorial line. Thus if heavy glass were in water, would point equatorially, and water polarly. If water in air—water equatorial and air polar; and so on in all bodies where any difference in the degree of the quality exists—and also in magnetic curves, however weak.

8082. What is the influence of this effect in nature—doubtless some. In crust of the earth perhaps not much. In center we do not know. In the air a difference, probably great by comparison, and all leaves of trees and plants will have the tendency. At least, unless air prove to be as strong as them, which the pointing of the other bodies, as wood, etc. already shews is not the case.

8083. Will metals stop at the equator as heavy glass does, i.e. if they are moved away, will they return to it? If so, how does this agree with the tension notion (7983), as they are conductors? (   ) Will heavy glass even stop at the equator if it could get away?

8084. Again went to work with a good battery and the great horseshoe magnet, with which I have worked ever since 3 Novr. (7874). Mr [illegible[1]] has lent me two specimens of *transparent fluor*, one from himself and one from Mr. Daniell (late of Jermyn Street). They are very fine and give ray dimensions from 1 to 2 inches long, but the surfaces are bad for vision. However, in them I found that the polarized ray *was rotated* by the influence of the magnetic curve or line of force. Now *Fluor* is also a crystal equiaxed and having no double refraction, so that it coincides with Rock salt and Alum in that remarkable law already announced in my paper given in as probable (   ). FLUOR SPAR.

8085. Wrought with bodies between the great poles, i.e. in the magnetic field, as to their motions under the influence of magnetic force.

8086. *Tourmaline*—a long narrow black crystal—and electric by heat, is *magnetic* as iron.

8087. *Hæmatite*—not magnetic to needle—is magnetic here and freely.

8088. *Pepper*. Three long peppers—were magnetic—but had been dusty and dirty and liable to contact of magnetic matter.

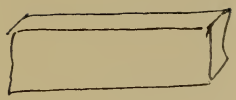
8089. *Berlin porcellain*—part of one of the Capsules of the labora-

[1] Possibly Tarrant or Tennant.

tory, very clean and good—*was magnetic*; heated it in N. M. Acid to dissolve off any iron from outside—still was magnetic. I did not expect this. Glass containing more iron than this seems to do is not magnetic.

8090. *Sul. baryta*—a piece of native—not magnetic; obeys as heavy glass.

8091. *Leather*—piece of Sole leather—not magnetic; obeys well as heavy Glass.



8092. *Apple*—a piece cut out of the middle either with a silver or a steel knife was very good, as heavy Glass—probably better for the water in it.

8093. *Beef*—a piece of red muscle—*not Magnetic*; obeys well as H. Glass.

8094. *China Ink*—a stick did not seem magnetic—did not obey as heavy glass—or as metal. Indifferent like sulphuret of copper. Another stick the same. One had been gilt on the outside, the other not.

8095. Is the body a little magnetic and so counteracts, or can the divided state of the charcoal do any thing here, as it appears to do in some other cases.

8096. *Silk worm gut*—a bundle made up into a compact cylinder—was neutral in action like the China Ink. Query whether a little magnetic or not, and so compensation or neutralization of the two forces.

8097. A rough piece of bismuth acted very well as Heavy Glass. It was then pulverized in a clean mortar roughly and put into a glass tube (7951) and was as a parcel half as long again as before. It still acted exceedingly well and did not seem to have lost power. Was really very good.

8098. A piece of Calcareous spar—obeyed pretty well. Then broken as small as cress seed and put into the glass tube. It obeyed quite as well as before. Again triturated much finer—and now the action seemed to be injured, for it did not obey well.

8099. Very fine trituration seems to injure (8010).

8100. Copper bar (7962) and good voltaic battery—action as at first (7963).

8101. If the copper is swinging by torsion or momentum rather strongly even, still upon electric contact it comes to its place at once, with no swing beyond or back from it; then, if after a few moments, contact be broken, there is the revulsion (7964). But if the contact be continued, the copper *does not move*, i.e. it does not tend as bismuth or glass towards the equator. Nor does it tend towards the magnetic axis.

8102. If whilst contact of E. Current continued and so the copper held in a place, it be pushed by a wire into a new place, *it stops there*—its vibration or motion is over at once, as if it were moving in a liquid or something able to deaden and stop its motions.

A push by a wire, which will not send it forward more than 40° whilst the magnetic force is on, will send it through two or three revolutions or more if the magnetic force be off. The effect is very distinct.

8103. But if the *Heavy Glass*, after it has come to the equator, be pushed away in like manner, it returns again to the equator.

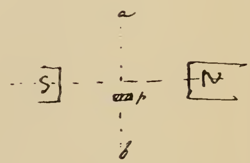
8104. Also when pushed, it moves or vibrates as easily whilst the curves of magnetic force are on as when off, i.e. allowance being made for the force which tends to pull it to the equator when magnetic force is on and which is of course either added to or opposed to the force of momentum.

8105. *Bismuth also* vibrates as easily and returns as surely to the Equator as Heavy Glass, indeed far better.

8106. *Take Bismuth henceforth* as the substance representing this class of bodies (   ).

8107. *Cadmium.* Is curiously between copper and bismuth—feeble as to both sets of characters but distinct in both. If it is moving by momentum or torsion, the magnetic force makes it stop in its place *like copper* AT FIRST; but continuing contact, it goes to the equator *like bismuth*.

8108. Now examined the Magnetic field by the bar of bismuth suspended in a *perpendicular* position by a fine thread 6 feet long, and placed in different positions as respects the magnetic axis and equator. The accompanying diagrams represent the magnetic poles, the line S N the magnetic axis, the line *a b* the magnetic equator, and *p* the place of the bismuth bar—the whole being seen in *plan* or in a horizontal plane, and therefore *p* appearing as a section of the bar of bismuth.



8109. When the bismuth was as thus ▱; putting on the magnetic force caused it to move outwds. or from the magnetic axis in the line of the equator—on taking off the force it returned to its first position, vibrating towds. the axis. By making contact as the bar swung outwards and ceasing it as it swung inwards, the vibration was soon made considerable.

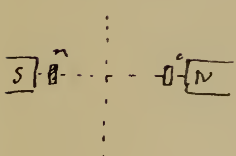
8110. When the bar was on the opposite side of the axis thus ▭, magnetic force made it swing *from* the axis, and *in* again when the power was off—so that the action was the same, but in both cases out from the axis when the power was on.

8111. When there were two bars, one on each side the magnetic axis—the magnetic force *seemed* to make them repel each other—but this was only the simultaneous occurrence of the two first actions.

8112. The bar of heavy glass (7807, 7902), slung and experimented with, acted exactly as the bismuth but not nearly so well—otherwise all the motions were precisely the same.

8113. A cube of heavy glass about one fourth the side of the bar experimented with—did as well, I think, as the bar. So cube or sphere shew this action very well.

8114. The bar of bismuth and that of heavy glass being on opposite sides of the axis and in the equator, *seemed* (8111) to repel each other as the power put on, and come together again as it was taken off.

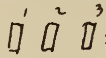


8115. Now placed the long bar of bismuth (end up) in the axis ▯ but near the S pole. When magnetic force was on, it moved towds. the equator along the axis; when the force was off, it moved back again. If the bar were at ▯, the magnetic force made it move from N towards the equator, and on taking off the force it moved back again.

8116. As long as the magnetic force was on, the bismuth was *permanently held out*, being apparently repelled from the pole.

8117. At the intersection of the magnetic axis and equator, the bismuth did not tend to move. That is a place of rest, but only because forces on each side the equator equal, for if cut into two halves, one would go one way and the other the other way from the axis along the equatorial line.

8118. When the bar was at a corner of the pole, on making

contact of E. Current, the bar immediately went off obliquely along the line of the magnetic curve at first, but afterds. outwards, as was found by other position[s] as at , and the same happened at the other positions or corners near the Magnetic poles. The course of the bismuth is along the curve at first but afterds. across them from stronger to weaker curves.

8119. Its endeavour is in fact not to go along or across the curves exclusively—but to get out of the curves going from stronger to weaker points of magnetic action.

8120. Hence it goes along a magnetic curve, either beginning at the N or S pole, and therefore in *either direction* along it. Hence also it will go across the curves from the axis and therefore also in *either direction*, but it always tends from stronger to weaker points of action.

8121. Hence also a new set of Magnetic curves marked out by this and other substance[s] and indicating both a new and peculiar action.

8122. What name shall I give them, and what name the others?

8123. Bismuth would no doubt go off from side of pole—also probably act better with a single pole.

8124. Announced repulsion of bismuth? Is now a law of action for all bodies like heavy Glass.

8125. A long piece of heavy glass (7807) shewed the same phenomena.

8126. Curious to see a piece of wood or beef—or apple—or a drop of water, etc. etc. etc. repelled by a magnetic pole in every direction.

8127. By a single pole would look like mere repulsion—but the law is *from stronger to weaker points of action.*

8128. Presents as a fact the only case, I think, of repulsion without polarity. But expect it will disappear as repulsion presently and prove to be a mere result of deficiency of action in relation to the air (8143).

8129. Now in place of the silk suspension I have employed for the former experiments on the setting of bars in the equator (7902), used this thread suspension of 6 feet long (8108), having an open glass cylinder between the poles of the magnet to shelter the bar from currents of air, and two pieces of paper to close over the top and cover it in. Suspended also the bars of bismuth, glass, etc. by fine copper wire made fast round them, and rising up 10 or 12 inches, and so by a hook in the upper end going into a loop of the suspending thread. Found all this very convenient and good—and using different sized cylinders, could have the magnetic poles more or less apart for any distance not exceeding five or six inches.

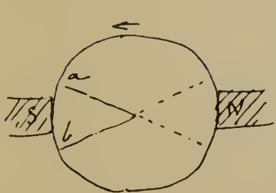
8130. Wood—had a plane of wood about 3 inches square and $\frac{1}{4}$ of an inch thick—it took its equatorial position very well between poles 4 inches apart.

8131. *Bismuth.* The bar (8027) 2 inches by 0·33 by 0·2 of inch—suspended by the middle in a horizontal position by the copper wire and thread (8108)—put into a glass between the magnetic poles and the glass filled up with distilled water. Then on laying on Magnetic force the bar pointed beautifully, taking up its position in the equator. Its beauty consisted in not going beyond it. It took up its position as directly and at once as copper did before (7963), but in the equator.

8132. The bar of *heavy Glass* (7807, 7902) in the same glass of water—also took up its position but more weakly and slowly. This for two reasons. One, the difference between it and water in respect of this power is not so great as between bismuth and water. Next, the Glass is lighter—larger in bulk and in moving has more water to displace.

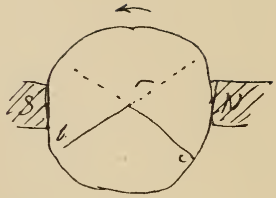
8133. This is a beautiful experiment, and being carried out to diffnt. fluids and bodies, will apply even to gases, and to bodies having *less power* than water, which then of course ought to set in the contrary or axial direction.

8134. *Copper.* The bar (7962) being put into the glass of water, was affected there exactly as in air, only not so strong or quick in the motions.



8135. There were some good indications thus obtained. Thus if the copper is moving slowly round by torsion—it may be caught by the magnetic forces any where almost and fixed there. Suppose it swinging left handed and caught at *a* position; if let loose there is but little revulsion, being in water and so opposed in motion. If it be caught in *b* position, there is under similar circumstances more motion of revulsion, because torsion is now with that effect, being before against it.

8136. If whilst going round by torsion moderately quick, it be caught by the magnetic forces at *b* and *contact continued* there, then after standing at *b* a while, it will slowly creep on, *accellerating* its pace as it nears the equator and enters the region beyond it; this acceleration goes on a while, then diminishes, and the copper bar comes to rest at about [ ]—stopping there, and from there it might if wished be *revelsed.* But if the contact still be continued, the bar very slowly moves (left hand fashion) by the force of the torsion and at last gets by the pole very slowly and with difficulty, and gradually creeps to the other side—still it *hangs* much—and it was only by stopping the electric current and so taking off the magnetic force that it then left this state and went off (moving left handed) on that side with strong *revulsive action.*



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8137. With Bismuth the effect (8115, 8118) appears to be an absolute and permanent repulsion. There is no apparently dual character in the force—is an unique phenomenon as to its kind.

8138. Can there be formation in Bismuth of currents in the *contrary* direction? Such an effect would account for the phenomena. If so, look for and find them.

8139. Then other bodies, as heavy glass, might have a tendency to these counter currents.

8140. There must be something particular in bismuth distinct from copper action.

8141. Would a *Bismuth wire* or rod, carried across the magnetic curves, give a current in the same direction as a wire of copper or a contrary current?

8142. So also of Antimony and Arsenic.

8143. *But*, on *the other hand*, is the apparent repulsion an absolute tendency off from the magnetic pole, or only a difference between it and the neighbouring matter, as the air or water?

8144. For the Bismuth goes *from strong* to *weak* points of magnetic action. This may be because it is deficient in the inductive force or action, and so is displaced by matter having stronger powers, giving way to the latter.

8145. Just as in Electrical induction the best conductors, or bodies best fitted to carry on the action, are drawn into the vicinity of the inducting bodies or into their line of action.

8146. In that case, if mercury or water were the medium about the magnetic poles, a globule of air kept to a horizontal plane or restrained in the vicinity of the magnetic field would be attracted to the pole and have all the reverse motions of bismuth or phosphorus or heavy glass in air.

8147. If one magnetic pole only were used, the actions would in almost all points be the reverse of common electrical attractions and repulsions—but not if 2 poles be used, for we have not the polar or dual action.

8148. Perhaps in fluids filling the magnetic field and the sides of the magnet there might be continual currents, though not likely. Might see by fine particles or other means, as coloured strata.

8149. Would not a piece of bismuth weigh lighter in latitudes where the dip is great, than at the equator—also in places where the intensity of magnetic force is greater than at other places? Also would not air weigh heavier in such places than in the others? The difference no doubt would be small.

8150. But may not this effect have some appreciable amount in the great phenomenon of the Globe, and at times of magnetic storms, may it not cause sensible differences in aerial phenomena?

8151. If an opaque Globe had its inside filled irregularly with air and ice or other dissimilar things like heavy Glass in their general quality, then could make out the general internal distribution—though the center of Gravity might be in the center of the Mass, for it would stand with its air parallel to the magnetic axis or line.

8152. This may have a bearing on the position of the earth in the ecliptic—and in reference to the sun's rays—and in many other ways.

8153. *Zinc.* The bar of zinc (8042) is part of a cast plate and is crystallized, the crystals running from both flat surfaces to the median plane and therefore being perpendicular to the larger forces of the zinc bar.

8154. Davy's elevations: have they, or not, any thing to do with these phenomena?

8155. If a magnetic pole were entirely in water, would the water circulate, or is the motion outwards in all directions with such degree of force as would make all stable?
8156. If it would circulate, then a globe of *iron* in the center of a glass of water ought to form two such sets of currents.
8157. Perhaps may find this by putting coloured fluid at the bottom of colourless—or by making the equator of the globe coincide with the plane of colour—or by alcohol and water and the striæ formed in them.
8158. If poles very close and small, perhaps a stream or wind in water or even in air—or some kind of circulation.
8159. Flame between 2 very close magnetic polar points.
8160. As bismuth is repelled without duality of force—two pieces ought to have mutual repulsion, or at all events, mutual action.
8161. Again, bismuth or Antimony, being used against copper as screens, ought to have some difference on the induction between magnetic needles.
8162. Must be some contrast or effect between bismuth and copper—also between bismuth and H. Glass—as well as between copper and Heavy Glass.
8163. Bismuth and air screens between two Magnetic needles.
8164. Babbage and Herschell. Effect of *bismuth* and *antimony* in their expts. on rotation. Phil. Trans.
8165. Ask Wheatstone for Conducting power of bismuth and antimony obtained by his apparatus.
8166. Bodies having less power than water, when in water should arrange themselves in the magnetic axis.

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8167. Why not call the new curves dimagnetic curves (   )?
8168. As different bodies, bismuth and water for instance, have different forces of motion along the magnetic lines, so on the earth's surface this should produce some curious results. A pound each of air, water and bismuth weighed at the equator and taken to latitude 60° or 70°, or where the lines of dip and force are most favourable, would change their relation of weight. The air would tend towds. the earth more than by gravity of a lb. and the bismuth less. If the water and bismuth balanced each other in vacuo in

one place they would not in another—and a place might be chosen where, if being balanced, taken to another place the metal might be heaviest and taken again to another the water might be heaviest, i.e. as regards their relative tendency to the mass of the earth.

8169. For their tendency downwards or to the body of the earth is not the mere pure result of gravity affected by centrifugal force or the gravitating relations of the sun and moon.

8170. And different bodies would therefore differ in gravity (apparently), and that differently in different latitudes.

8171. Is it likely there can be currents formed round the *particles* of air, Glass, bismuth and other bodies than the magnetic metals? If so, they must be currents in the opposite direction, or they would not account for the repulsion in all directions.

8172. As to effect of comminution: (if it be any) it is also very likely that the feebler the power of the body the more the breaking up of the mass interferes, and so bismuth may be less affected by pulverization than marble or common glass (8098).

8173. In bodies, too, which in powder be more open, more air gets in.

8174. As a plate of heavy glass in the equator rotates a ray of light—what mechanical reaction ought the same plate to have *if free to move*—or surrounded by copper, etc. etc.? May substitute bismuth for heavy Glass.

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8175. Air and things below as to *equatorial set* would evidently set and congregate between the poles, and the more strongly near the poles and as the poles are nearer to each other—to the expulsion of the other bodies, as heavy glass, phosphorus, bismuth, etc. etc.

8176. But this is what iron, nickel and cobalt does.

8177. If iron were in a mass of fluid nickel, between the poles of a magnet, it would not set more easily than things below air do in it.

8178. If nickel were in a mass of fluid iron, it would set equatorially as heavy glass does in air, i.e. assuming that iron is in all cases more powerful than nickel—or that iron is to nickel as air to water.

8179. Surround the poles of a magnet with fluid iron, and a bar of nickel in it would set and be repelled ( ) just as heavy glass or bismuth are in air or water.

8180. What difference then is there between the set of heavy Glass, water, bismuth, etc. etc. and iron, except that due to the different relations of the substances in the order of strength. Thus, assuming that Iron is stronger than Nickel and Nickel stronger than cobalt (which may not be the case) then this order

Iron Nickel Cobalt Air Ether Alcohol Water Crown Glass Flint Glass Heavy Glass Phosphorus Arsenic Antimony Bismuth	would form a series of bodies acting alike, but a bar of any one of them in a fluid sphere of any other, being in the magnetic field, would set equatorially in the substances above it and axially in the substances below it.

8181. Then is there any physical difference between these bodies, except in degree?

8182. In that case, how does red hot iron, nickel and copper come in? Either they *change their mode of action* altogether, or else they *fall suddenly into* a *very distant condition* of the same mode of action. Either may be true, but the latter is perhaps the most likely.

8183. The change is analogous to that of bodies from the solid to the liquid state. Especially since Henry has shewn that they do not lose much of their attractive force in the change.

8184. Consider the difference of conductors and non conductors as regards electricity. Will it affect the question? It is evidently unimportant to the ordinary action of *dimagnetics* whether they are the one or the other—but we have not yet a nonconducting magnet—though a crystal of sulphate of Iron approaches to it.

8185. These magnetic salts would require to be worked into the list on the last page[1].

8186. After all this, consider the effect on light—it appears to be the lesser phenomenon, i.e. all bodies appear to be affected alike by the magnetic forces, but in different degree; and when so affected, probably all would influence a ray of light, but it is only some which, by transparency and physical structure and fitness in other respects, enable us to see the phenomena in a sensible degree.

8187. And it is apparently these bodies, which being most simple in their effect on light in their natural state, are also at the same time nearest to the bottom of the list in the preceding page.

8188. Then what as to centers of magnetic action, such as a natural magnet—an artificial magnet—and an Electro magnet? Consider this point well.

8189. If particles from the top and bottom of the list (8180) could be mixed, there surely ought to be much motion amongst them when near and between strong magnetic poles.

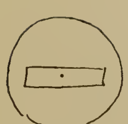
Try Bismuth powder and water.

„ Antimony or Arsenic powder and water.

„ Phosphorous powder in a solution of S. G. of phosphorous.

8190. Can there be convection of magnetic power by particles between?

8191. Placed the Woolwich helix and core (7801) vertical so as to work with *one pole only*, and also allowing motions in a plane perpendicular to the axis of the magnet. Excited it by 10 pair of Grove's plates—action was good. Used the suspensions by 6 feet of fine thread (8108), hanging the substances in little slender cradles of copper wire hanging by a hook extension of it from the thread, and even used no shading glass in most of the experiments, being alone and in a quiet place.



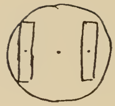
8192. *Bismuth.* The bar (8027) of two inches by 0·2 and 0·33 of an inch.

When the bar was over the center of the end of the core, i.e. over the center of the magnetic pole (the pole being the flat end

[1] i.e. par. 8180.

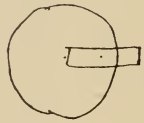
of a cylinder of soft iron, $2\frac{1}{2}$ inches in diameter) and $\frac{1}{8}$ of an inch from it, laying on the magnetism did *nothing*.

8193. When the bismuth bar was in either of these positions, the magnetism made it move towards the center of the pole, i.e. towards the axis of the magnetic pole, and when the contact was broken the bar swung out again (8196). A swinging, irregular motion also came on by successive contacts (8264).



8194. When the bar was in either of the above positions or in any similar position as respected the center of the magnetic pole, and the magnetic power was continued, then the bar swung round until it had taken a radial position and there stopped.

8195. But on taking off the magnetism, the bar passed in towards the axis of the magnet. So when bar first of all in this radial position, making the pole magnetic caused the bar to move outwards from the axis.

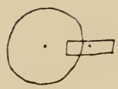


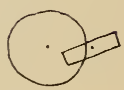
8196. Repeated the former experiment (8193) and found that, in reality, in that position the center of gravity moved *outwards* on making contact and inwards on breaking contact (8195). The apparent motion inwards was only the tendency of the near end, when there was a little inclination, to set the bar round into the radial position.

8197. A cube or sphere of bismuth is probably the best fitted to shew the simple motions of a particle of such matter in these different places.

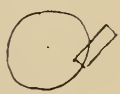
8198. So Bismuth bar set first of all radially. At the same time it is repelled outwards.

8199. If the bar be thus, magnetism makes it go *outwards*—if a little inclined to radial line as thus, the magnetism brings it into *line of radius* and makes it go *outwards*. If the bar be thus*, the magnetism sends it *outwards*—but if it be a little inclined to the tangent as thus†, the magnetism then causes it to set in the line of *the tangent* and *not in radial line*, and it goes outwards.





8200. There is a position, about this, which is quiescent as to setting round, and there the bar only goes outwards: but the least deviation to one side or the other of it causes the bar to go round into either the radial or the tangential position, at the same time that it is sent outwards.

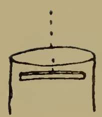


8201. All this depends upon the different lines of magnetic force

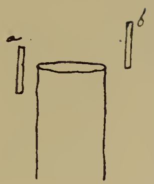
* [8199]

† [8199]

in which different parts of the bar are situated. So a smaller piece of bismuth, and that globular, would be better ( ).

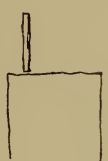


8202. If the bar, having its center of suspension on one side of the magnetic pole as above (8199), were lowered so as to be just below its level, it then always set in the tangential line and was always repelled by the magnet when magnetic—and if lowered until an inch below the top edge, the effect was the same and the repulsions very good and striking.



8203. Now slung the same bar of bismuth so that it should hang perpendicularly. When as at *a*, the magnetism caused good repulsion; when at the same distance from the axis of magnet but raised into position *b*, there was the same repulsion *but less*.

8204. When from position *b* it was approached nearer to the axis of the magnet, the repulsion or force was outwards and very good again.



8205. When placed just over the magnetic pole and at a distance of about $\frac{2}{5}$ of the radius from the side as thus, the bar seemed to have a tendency *to* the center on making contact with the battery and not outwards (8204). When a little nearer to the axis and about $\frac{3}{5}$ of the radius from the circumference, there was less tendency to the center and it was hardly sensible—further in and at the center, there was no motion of the bismuth, i.e. no motion of the center of gravity.

8206. The bar has motion if it is not in the line of the axis, but the motion is that which belongs to the bar to set its width in the radial position (8199): so if its projection on the plane of the pole happen to be as *a*, it turns and by successive vibration sets itself as *b* (8266).

8207. I am not quite sure whether, when at the $\frac{2}{5}$ distance (8205), the apparent tendency inwards is not this endeavour to turn into radial position. Find that out by a globe ( ).

8208. Whilst using the vertical and the horizontal bar of bismuth as above, I placed close to it, on the top of the pole or by its side, cubes and prisms of bismuth, to see if there were any attraction or repulsion between two pieces of bismuth in similar magnetic conditions; but I could not find that the moveable bar was in the least degree affected by the vicinity of these bodies.

8209. *Heavy glass* (7807, 7902): the bar was in the various positions affected exactly as bismuth, having all the same motions, but in a much smaller degree, and not those which depended on the thinness of the bismuth bar (8206).

8210. *Phosphorus* was affected exactly as bismuth and heavy glass and better than heavy glass.

8211. *Antimony* (8027[1]). The bar affected exactly in the manner of bismuth and its compeers.

8212. *Arsenic* (8030, 8040). All the pieces of Arsenic shewed when hung near this single pole considerable traces of magnetism. It seemed located in spots as if accidental impurity. But it prevented my having the repulsions of bismuth (8198) though I could in some positions obtain the set in certain directions.

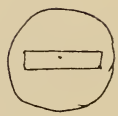
8213. A single pole will be a much better test for local magnetism in a substance and perhaps for general magnetism than two poles (8038, 7981½)—and the distinction is *more striking* because the effect is not the mere difference between the magnetism and the set but between attraction and repulsion, for bodies not magnetic are bodily repelled, i.e. in air, and of course subject to the principles before set down (8175, etc.).

8214. *Water* (   ) in a tube (   ) at the side of the pole (8202) had just the same tangential set and repulsion as bismuth (8198), but small in degree.

8215. *Bismuth*, triturated and in powder in a tube—acted in these positions very well.

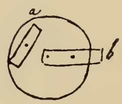
8216. *Bar of bismuth* wrapped up in three thicknesses of copper foil acted very well, as the simple bar of bismuth.

8217. *Copper* and the single vertical magnetic pole (8191). The bar of copper was of the same size as the bismuth bar (8192). When it was over the Magnetic pole, the magnetism *did not* affect it.



8218. If it were slowly revolving by torsion or momentum, the magnetism did not affect or retard it. There was no appearance of the tenacity of position or sluggishness to move which it shewed on a former occasion in another position (7963, 8102).

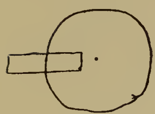
8219. If the bar were thus *a*, then the magnetism caused it to pass inwards or towards the line of the axis of the magnet, and breaking contact made it pass back again (8265). But if with



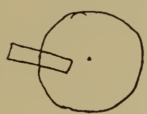
[1] Should presumably be 8028.

the same center of suspension the bar were as *b* (it then a little overlapped the center of the pole), then the magnetism repelled or sent it outwards from the axis of the magnet; but only feebly, for the action was not so strong as the previous attraction inwards towds. the axial line.

8220. When the copper bar was oblique to the radial line, then on making electric contact there is a pull towards the radial position, and on breaking contact there is an effect of revulsion.



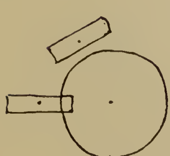
8221. If the point of suspension is just over the edge of the magnetic core and the bar radial in position; making contact sends it outwards and breaking contact brings it inwards.



8222. If the bar moves by torsion so as to be a little oblique to the radius, and contact is then made, the bar is caught (8101) and held in that position; and on breaking contact there is *revulsion.* If it move further on, clock fashion, it is again caught by the magnetism, and on taking off the magnetism there is revulsion. So it swings on clock fashion by successive steps as often as it is caught and revulsed, advancing by these successive steps (as it perhaps would have done by its own slow swing or by continued contact for a good while (8136)) until it is tangential. If caught at the tangent and kept there a moment or two, on breaking contact there is little or no revulsion. But if instead of that, the bar, after having been under the magnetic action in a position oblique to the tangent, is then allowed by swing to come up to and pass the tangential position (contact meanwhile being continued), it then rapidly moves on after passing the tangential position, clock fashion (by force of that state of charge it has

received which was before referred to (7976, 7, 8136)), until it come up *at once* to a position on the other side the radial line as before (8222), and then on breaking contact there is revulsion (left handed) and all the phenomena in a reverse direction just as before.

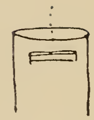
8223. The state of charge which produces revulsion or a part of it on the one side, and the apetite it has for the direction of force it finds on the other side of the radius (or its own position), are apparently one and the same thing.



8224. When the *copper* bar is carried out beyond the edge of the iron core, whether it be placed radially or tangentially, it is thrown outwards on making electric contact and falls inwards again on breaking contact.

8225. If it be oblique to these positions, there are then the same actions as before (8222), i.e. the tendency to go to the radial position, the bringing up of the bar to its place and the revulsion on breaking contact. Only when the bar is near the tangential position there is little of this effect and the general tendency seems to pass into the effect of repulsion or going outwards bodily.

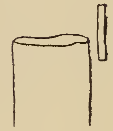
8226. The copper bar placed at the side and below the edge of the magnetic pole in a tangential position is, I think, *more strongly repelled* even than bismuth and falls back as the current ceases (8269).



8227. So copper is as bismuth in these respects. Only it has certain results in addition, probably dependant on its excellent conducting power.

8228. It falls back (8270) I think *very readily*; perhaps more readily than is due to gravity alone?

8229. The *copper* bar was now hung perpendicularly about $\frac{1}{4}$ of an inch from the core and having about $\frac{2}{5}$ of its length below the edge of the cylinder. The rod is 0·2 of inch thick and 0·33 wide, so that it may hang in several positions at the side of the core, as for instance either with radius parallel to its width or to its breadth or oblique to both.

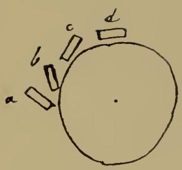


8230. Now comes the curiosity, or rather the old action of revulsion, and in a striking form. If the rod be as *a* or *b*, it is repelled on making contact and falls too again on breaking contact.

8231. But if it be oblique as *c*, the center of Gravity is repelled but the angle nearest the magnet is pulled in with a tendency to go back towds. the line of the radius; it quickly stops *stiff* in an oblique position until on breaking contact there is a strong revulsion, which will spin the bar round 2, 3, or even 5 or 6 times.

8231 a. Yet if contact be made at any moment, the bar instantly comes up to a position and then on breaking contact is again revulsed.

8231 b. The bar, in being drawn up, is never drawn up fully out of the oblique position, unless indeed it be very nearly out of it at the moment of making contact (8267).



8231 c. There is also the same charging on one side and the same favourable condition for the other as before (8222). Thus if the bar were as *a*, and contact be made and broken in a moment, the bar first moves left handed and then on revulsion right handed: but the right hand motion will always gain on the left: and though the touches be ever so quick that are made at the voltaic battery, the bar will at last become in its width tangent to the magnetic cylinder, as at *d*, and will keep that position in all after quick touches. In fact, quick touches brings it up to that position and there it settles as respect motion on its own axis. On whichever side of the radius the first inclination be, the effect is the same, the direction of the motion and revulsion only being altered if the side be altered.

8231 d. This will make a good shape of experiment for the lecture room.

8231 e. When the copper bar was raised to the level of the top of the magnetic core, the motions were all of the same kind but not so strong.

8231 f. When the bar was over the magnetic pole as at *c*, the motions were weak but apparently of the same kind.

8231 g. The tendency of the bar is evidently to set its chief plane perpendicular to the magnetic line of force, but the *drawing up* and *set* and *revulsion* seem to be something different?

8231 h. A card was placed on the top of the Magnetic core and bismuth in fine powder sprinkled upon it; the card was then tapped lightly to make the particles dance, and they did dance, but nothing particular occurred, the core being *not magnetic.* Then the core was magnetized and the process repeated and now the particles all moved away from a line exactly formed over the circumference of the end of the cylinder, some moving inwards and some outwards but leaving the line clear in the center.

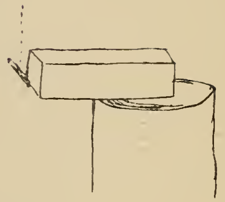
8231 i. There was no signs of arrangement among the particles inside or outside of the line—no signs of *an attraction* or *repulsion amongst themselves.*

8231 k. From the particles passing some inwards and some outwards, it is probable that a small bar or cube or globe of metal would pass inwards or outwards from that edge, i.e. if it did not rise too high above it; and hence probably the explanation of many of the movements near the edge.

8231 l. The edge, in fact, forms the apex and concentration of the pole and from it the curves must most rapidly decrease in force. Parts of metal therefore *near* the metal of the core are nearer to the true apex of power at the circumference than at the center of the end of the core.

8231 m. So a conical termination would be best to shew all the motions and would shew the travelling of the bismuth from that point all round.

8231 n. Arranged a horizontal magnetic pole for a few experiments allowing motion in a plane parallel to the axis of the magnet. Did this by putting a short bar of iron, about 1¼ inches square, on to the top of the Woolwich core whilst vertical, and then worked there. It is of course just like one end of a horizontal magnet.



8231 o. The *bismuth* bar (8027) in front of the magnetic pole took up position perpendicular or across the magnetic line of force, and was repelled bodily—returning as the magnetic power was removed.

8232. If the bar be end on, it is repelled on making the electric contact and returns when that is broken. This position is a position of unstable equilibrium as regards the motion of the bar round the point of suspension. For if the bar be inclined on either side, it tends to go on and take up position across the magnetic curve or line of force.

8233. At the corner of the pole the effects were the same.

8234. At the side of the pole the effects were the same.

8235. *Water* in a tube ( ); same actions as bismuth but much weaker.

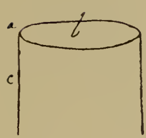
8236. *Phosphorus*: the same actions and better than water.

8237. *Copper*, hung perpendicularly at the end of this pole, was moved just as before by the side of the vertical pole (8229); there was the repulsion and the fixing and the revulsion, etc. etc. etc.

8238. The copper bar being hung horizontal at the end of the bar of course projected beyond it, for it is 2 inches long and the pole only about 1¼ inch wide. *Now nearly all the odd motions, revulsions, etc. gone.* There is the repulsion on making electric contact and falling to again on breaking contact, both in front and on all sides of the pole, both when the bar is parallel to or perpendicular to the line of magnetic force. All that is as before and as bismuth.

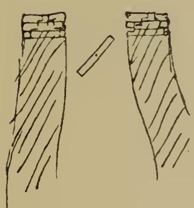
8239. But if the bar be oblique to the magnetic line, there are very small signs of the attractive pull, the fix, and the revulsion, though there are traces.

8240. But if the face of the pole be large and the dimensions of the copper bar before it be small, these rise up to a very high degree (8231). So they seem to depend upon the full enclosure of the bar by the magnetic curves and would probably disappear with a conical termination or with a hemispherical termination and a small bar; and almost certainly has relation to the excellent conducting power of copper.



8241. As to the cylindrical core, the iron round the circumferential angle *a* is probably more highly magnetic [than] that at *b* or *c*, and hence a complication of forces on a bar in different positions in front.

8242. Ampère's motions of copper ring probably have reference to these peculiar actions of copper, but cannot say as yet. Have not the dimensions or expressions of force of his apparatus, helix and cylinder, etc.



8243. *Bismuth* bar (8027) suspended horizontally, and when at rest, a *common horse shoe magnet* of considerable power was brought up and advanced so as to include the bismuth bar between its poles in an oblique position. Immediately the bar moved and vibrated to the other side and back, taking up its position in a line across the magnetic curves going from pole to pole.

8244. When the bar was still and out of magnets, i.e. free in the air, by approaching and withdrawing one pole of the magnet, I could easily obtain the repulsion and the swing.

8245. *Heavy glass.* Shewed the set across and also the repulsion, but with difficulty by this *common magnet.*

8246. *Phosphorous* also shewed the phenomena–require great precautions in these experiments.

8247. Employed the *Woolwich Helix* (7801) alone *without its core* and a bar of bismuth hung before it. The power to repel it is exceedingly small, hardly sensible. The voltaic battery is no doubt much reduced, but still the helix is exceedingly feeble in power, not better if so good as the common magnet.

8248. Consider the settling power of copper and its quelling power in Arago's phenomenon and on compasses.

8249. Can there be a convection of Magnetic power by particles—it does not seem impossible. Still, not likely.

8250. Consider the action of heat on Iron and extend the view to all bodies.

8251. Now we find *all* matter subject to the dominion of Magnetic forces, as they before were known to be to Gravitation, Electricity, cohesion.

8252. All bodies do not seem magnetic after the manner of Iron.

8253. Magnets and Iron, Nickel, cobalt, etc. seem always to have the power, i.e. it or the currents appear to be circulating about their particles and employing their forces one on another; but when the electric current is circulated around them, it seems to arrange these forces, polarizing them as it were, and giving them a constant direction, or giving that direction to a certain amount of that internal force.

8254. In the list given (8180) or to be formed, Currents may easily be formed round the particles of those at the one end of the list and only a *tendency* to currents round those at the other end. These latter it is that act most on light.

8255. A solution of sulphate of Iron forms a transparent, liquid magnet, which by dilution may be adjusted to any degree of strength within certain limits. Hence it may be used to present all the phenomena with magnetic matter which are presented to us by Air, Ether, Alcohol, Oil, Heavy Glass, etc. etc. A solution that sets axially in a weaker ought to set equatorially in a stronger solution.

8256. Is Bismuth and Air quite like iron in the character of action? Will an Electro helix having *air* and *bismuth cores*, yet the same current, act differently on a distant magnetic needle, as in Jones' measuring apparatus?

8257. What ought a vacuum to do? This important as regards air, gases and indeed the whole subject.

8258. A cube of copper, to examine the particular action at the edges and center of the end of core—or at the point of a cone. Perhaps be good in a helix.

8259. Cube of copper in different bottles of gas and in vacuo.

8260. The powder of Bismuth (8231 h) shews no signs of attraction amongst its particles. If they are not magnetic like iron filings and so do not form concentrations of the forces like these, then there is no reason to expect their attraction.

8261. Place an inch cube of bismuth at the end of the magnet, repel it and ascertain the power in grains required to bring it back again to its place.

8262. If air be rarefied, ought not different bodies suspended in it to set round in succession into the axial position—Water, Heavy glass, Bismuth, etc.?

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8263. The Woolwich magnet ( ) and core placed vertically and with the flat end in order to repeat certain results.

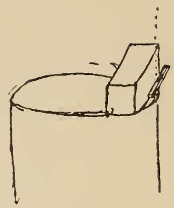
8264. Bismuth bar (8027) slung horizontal and over the pole, near to it and about $\frac{2}{5}$ of radius from the edge (8193); on putting on the magnetism, the bar is really pulled inwards, i.e. towards the axis of the magnet, the effect being not merely an endeavour to set in line of radius.

8265. The copper bar (7962) in the same position was also drawn inwards and affected exactly in the same manner (8219).

The set radially (8194) seems due to the pull inwards of that end of a bar which is in a position to go inwards, and the passing outwards of that end of the bar which is in a position to pass outwards, and so is compounded of the two tendencies and motions in the ends of the bar; and it is easy to see that that ought to be the case.

8266. *Bismuth bar* (8027) perpendicular and hanging near to and over the flat face of the pole as thus (8206); there is very little tendency to set either way—it is very nearly neutral in that respect.

8267. *Copper bar* (7962) *perpendicular* and hanging by the side of the pole as (8229, etc.). The tendency on making magnetism is to place the bar *edgeways* to the cylinder, i.e. the broad faces are parallel to the line joining the magnet and bar. The copper tends as it were to become polar, and so sets edge on to the surface of the core. This is as respects the effect of the first contact and before revulsion.



8268. The *copper bar slung horizontally* and brought near the face of a short inch thick iron bar laid on the top of the pole (to increase the size of the face). It did not set and become revulsed nearly so well as between the two poles (7964).

8269. Copper bar horizontal by the side of the core (8226). It set tangentially to the side of the core, but not very strongly, and just as bismuth, not as preceeding act of revulsion.

8270. When the copper is held off by the magnetism and then the latter removed, I do not think the copper presents any thing particular on the falling back (8228).

8271. Tried by this single pole the magnetism of the different bars, etc. (8023, etc.) by placing them horizontally and near the side below the level of the top, as thus—

8272. *Silver bar* (8029) considerably magnetic.

8273. *Platina* (8043) Do.

8274. *Lead bar* (8024) strongly attracted.

8275. *Tin bar* (8025) does not seem to be magnetic—is repelled like bismuth but feebly—is probably a little magnetic and, though able to shew bismuth action, has it in part masked.

8276. *Zinc bar* (8026) is well repelled as bismuth; does not appear to be magnetic.

8277. *Antimony bar* (8028) repelled like bismuth, better than Tin—as good as zinc.

8278. *Cadmium* (8049). Repelled very well—not much if at all magnetic.

8279. *Gold* (8046) two sovereigns; magnetic, for they went inwards on making contact independant of the copper action to be mentioned presently.

8280. *Zinc*: four plates cut from a drawn plate (8153). The last was a cast plate. These were cut so as to have the draught across two and along the other two. They all acted alike and were well repelled after the manner of bismuth—and better in that respect than the cast plate, which is a little magnetic (8026).

8281. A piece of red Sealing wax (8001) not sensibly magnetic here (8271).

8282. Solutions of Iron (8008, 57) well magnetic by this single pole and point to it—(in contrast with the tangential position).

8283. Hung up the bars of the different metals perpendicularly so as to compare them with copper in its peculiar action (8229).

8284. *Zinc* acted well in the manner of copper.

8285. *Silver* acted exceedingly well, notwithstanding its magnetism.

8286. *Tin* acts in the same manner very fairly.

8287. *Antimony* gave a trace of the action.

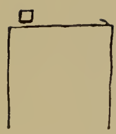
8288. *Bismuth* the least possible trace, if any.

8289. *Lead* was too magnetic to allow of an observation.

8290. *Gold.* The two sovereigns (8279) shewed the peculiar copper action very well.

8291. It may well be that some of these metals, as Gold, Platinum, Lead, Tin, Silver, Palladium, Rhodium, etc. may be magnetic of themselves, i.e. when in air. It is hardly likely that after Iron, nickel, cobalt and their derivatives, that air is the next on the list. Must ascertain this point.

8292. A single particle (a small filing) of iron placed between two flat pieces of bismuth greatly interfered with the particular movements of the latter metal.



8293. Now worked with a *cube* of bismuth half an inch in the side so as to obtain simpler results of motion. When in from the edge about $\frac{2}{5}$ of radius, *it went inwards* on making the pole magnetic.

8294. Even when close over the edge and low down, i.e. near the face of the pole, it went inwards on making electric contact.

8295. When a little further out, it was very wavering in its movements—and a little further outwards still caused it to go outwards on making contact.

8296. There is evidently a neutral line all round the edge where doubtless it tends to go upwards or against gravity along the line of the magnetic curve there. Inside of this it goes inwds.—outside of it it goes outwards. The line will of course partly depend upon the plane in which motion is allowed to the cube. If the pole were inclined, then this point would probably be just over the angle *near* to the magnet.

8297. A *bismuth sphere* about 0·2 of an inch in diameter used in the same manner and gave the same results. Near to the edge there was no tendency to motion, and at the center or axis of the magnet there was no motion. If it were at first a little within the bounds of the edge, then the magnetism made it move inwards —if it were a little outside the edge, it made it go outwards.

8298. Being lowered to the side about $\frac{1}{3}$ of an inch down or below the level of the face and just touching the core, the magnetism would repel it, sending it outwards until even half an inch off—looking just like an *electric repulsion* in effect.

8299. Have had an *iron cone*, 2 inches in diameter at the base, made, to put on the flat face of the core and so form a conical termination to the pole.

8300. The *Bismuth half inch cube*, when hung ove[r] the apex, did not move on making the pole magnetic; but when a little on one side, it then swung outwards (as expected) on putting on the magnetism.

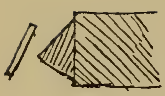
8301. The *copper* $\frac{1}{4}$ *of an inch cube* was acted on in the same manner exactly—but certainly not to the same extent as the bismuth.

8302. The *bismuth sphere* (8297) gave the same movements as the cube and very well.

8303. The *copper* $\frac{1}{4}$ *of an inch cube* (8301) at the side of the core gave some *very good indications*. First it is *repelled* bodily as the bismuth is. Next, it does not, on making the magnet active, turn on its own axis, nor does it, on taking off the magnetism, suffer revulsion. Yet it sets and is held whilst contact lasts, i.e. *it sets in relation to itself*. For if it be rotating on its axis and also swinging on its suspending thread, the electric contact instantly stops the first but the second is not affected—for the cube is repelled outwards and continues to vibrate, but *allways parallel to itself*. This is at the side of the core.

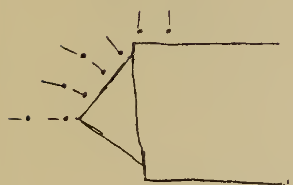
8304. Is the copper polar at this moment—or is it merely that any motion cutting the magnetic curves instantly sets up currents that tend to make it keep its *present* condition—that being a *stable* position or direction? Or is there actually counter currents accounting for the repulsion—or is that entirely due to the inwds. determination of the axis.

8305. Have now placed the Woolwich Magnet horizontal and affixed the conical termination to it.



8306. The *horizontal* BISMUTH *bar* (8027), being near the apex, set beautifully across the Magnetic curves—and was repelled as before. The same effect was produced also at the sides of the end of the core and in every other position all round.

8307. The *horizontal* COPPER *bar* (7962) in the same positions is also *repelled* by the magnetic force. But it does not set perpendicular to the magnetic lines, except by long contact of electric battery—for it is *caught* by the first contact and held in *a position* until contact is broken. There is not much *revulsion* on breaking contact with this form and size of pole and bar—still, there are traces.



8308. The ½ *inch cube of* BISMUTH was placed in the various positions indicated by the dots, and on making the pole magnetic moved in the direction indicated by the little lines. When the little *bismuth ball* (8297) was used in the same manner, it indicated the same directions of motion.

8309. The *copper cube* 0·33 *of inch* (8301) indicated exactly the same motions and the return when the electric current was taken off—but there was also the strong set (8303)—for if the cube be vibrating and rotating, the latter is instantly stopped, the former goes on undisturbed as before.

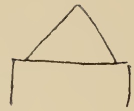
8310. The copper bar (7962) was now slung perpendicularly and placed in different positions to the pole as indicated by the dots; the same horizontal plane passing through the middle of the magnet and of its length. Wherever it was, and even if with a strong torsion twist on it, the magnetism caught it at once and held it almost stiff—it scarcely moved (on its own axis) after. It looks just as if it were polar of a sudden—but still, I suspect the effect is due almost entirely to the currents produced by the different moving parts cutting the magnetic curves. It is like my revolving brass globe of old times—and this accords with the fact that the best conductors shew this effect best.

8311. The copper bar is repelled as a mass as well as the former smaller pieces.

8312. The stop and set (8303) is best at the side of the core, where the magnetic lines throng most or diverge least rapidly.

8313. In considering the revulsion, the falling back of the center of gravity will help the action of the then existing currents.

8314. Must examine this action by a little ring or helix of copper wire in this same position, connecting it with a galvanometer.



8315. Made the magnetic pole vertical and worked with powders over it.

8316. Bismuth in fine powder on paper held over the cone and in contact with it. Being tapped, the powder left the spot above the cone, but there was no indication of arrangement in the particles generally.

8317. If the paper with the particles on it be drawn over the apex of the cone, rubbing on it, a line is cleared from the particles wherever the cone point has passed, shewing the passage of the bismuth particles right and left from the cone point. Characters may in this way be easily written in cleared lines.

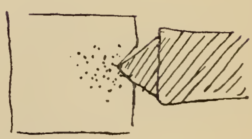
8318. The cone was removed and paper with particles put on the flat end of the core; being tapped, the particles moved *in* and *out* from the line representing the edge of the cylinder.

8319. A Glass dish was put on the top of the core; water into the dish and fine particles sprinkled on to the water. On making the pole magnetic, many of the parcels of the particles moved inwds. towards the line of the axis of the magnet.

8320. When the glass dish was put over the cone, several particles ran up towards the point, but the action looked rather like the magnetism of iron.

8321. Copper bronze dropped on the water shewed irregular action, but the surface of the water was soon embrassed[1] by the films of metal.

8322. A ball of bismuth floated on a cork—did not move very well here—perhaps do better on spirit of wine, for water soon becomes filmy on the surface and then sluggish.



8323. When the magnet was horizontal and the cone on, a piece of paper with bismuth powder on was brought up to the cone and the magnetism put on. There was repulsion just at the point, but no signs of arrangement farther in. Probably with 2 conical poles and only a small space between them, curved forms might appear.

8324. *Iron, hot and cold.*

A piece of clean iron wire slung by platina wire and hung as

[1] Presumably a variation of spelling of *embraced*. *O.E.D.* admits *embrase* as an obsolete form of the verb.

before was then within half an inch of the apex of the horizontal cone pole. It was then heated by a spirit lamp until red hot and it had lost every signs of attractive force to a horse shoe magnet brought near it. Then making the great core a magnet, the iron was instantly attracted by it.

8325. The attraction, though a true magnetic attraction, was very feeble by comparison, until the contact of the core had cooled it, when it not quite suddenly but very quickly took on it its full magnetic force, trembling with energy as it adhered by one end to the cone.

8326. The first magnetism was very clear and distinct though weak, and the contrast between it and the second very beautiful. It was very easy then to think the iron just in the state of the ordinary dimagnetics.

8327. *Nickel.* A piece of pure nickel was dealt with in a similar manner. Long before it was visibly red hot, it had lost all power to or upon the horseshoe magnet (8324). Still when quite red hot, as hot as could be, it was attracted by the great Woolwich core. The attraction was only feeble, and it was beautiful to watch the piece of metal sticking or clinging side ways to the core, as it cooled; it would have fallen off but for the wire suspension. The magnetism did not come on all at once, i.e. as respects the mass—but it gradually and slowly increased. Then by degrees and after some time the increase became very rapid, and then the attraction and holding on was with tremors, just like convulsive movements, they were so strong.

8328. The experiments are *good*—they shew that the power is never entirely lost—not when they are and act as dimagnetics; and that the change from the weak to the strong state, though quick, is not instantaneous, but progressive.

8329. Quite like other dimagnetics when hot—and so why should not other dimagnetics be like them, according to the view given (8175, etc.) and the list (8180).

8330. Have prepared certain tubes, about 0·5 of an inch external diameter and 1·8 inches long, of thin flint glass, and have nearly filled them with certain substances and sealed them up hermetically. The following are the marks and contents:

o Air.
I Ether.
II Camphine.
III Absolute Alcohol, or nearly so.
IIII Water distilled.
IIIII Solution of proto sul. Iron (standard 40 grains anhydrous [to 1 ounce).
IIIIII „ „ 1 vol. of v and 3 vols. water.
IIIIIII „ „ 1 vol. of v and 15 vols. water.

Sul. iron anhydrous	76
7 Waters	63
Crystals	139

8331. Had also portions of the same fluids and solutions to put into glasses, forming media in which the tubes could be immersed and then their set observed. The tubes were held in a little frame of fine copper wire, and when necessary, held down by a cube of bismuth hung from them exactly in the line of suspension.

8332. Then these different substances were compared by the set of a tube of one immersed in a glass of another, and so data gained as to their relation one to another.

First

8333. The *tube of air*, o, in the air–set very feebly across the magnetic lines of force, i.e. in the equatorial direction. This is no doubt a result of the action of the glass tube, and has to be considered as to its influence in any of the experiments to come.

8334. Tube of Air in Water.

The air and tube clearly set axially: and from both sides of the axis into its direction. The effect was weak but very distinct, and the more so as the effect of the glass would be to set equatorially in water.

8335. *Tube of* Air *in* Alcohol.

Also a tendency for the air to set axially, but weaker I think than with water (8334).

8336. *Tube of* AIR *in* OIL OF TURPENTINE. Air set axially and I think better than in water.

8337. *Tube of* WATER *in* Alcohol. I think the water tends to set axially in the spirit, but the effect if any is very small and it is difficult to say.

8338. *Tube of Alcohol in water*–tends to set axially–the effect clear but the difference small.

8339. So the apparent result of these two experiment[s] would be that Alcohol is axial to water equatorial.

8340. *Tube of water in Camphine.* I cannot say what is the set. Also tube of *Camphine in water*: I could not make out the set.

8341. *Alcohol tube in camphine*—sets axially—but though distinct, the effect is very small. *Camphine tube in Alcohol.* Very doubtful if it sets any way. So perhaps Alcohol axial to Camphine equatorial.

8342. The *iron solution* v (8330) easily pointed axially either in *Alcohol* or in *water*. The weaker *solution* vi easily pointed axially in *water*, and the *weakest solution* vii pointed axially in *water*, but very feebly, not better perhaps than *air in water*.

8343. Now *tube of* vii in a bath of vi—the tube pointed equatorially.

8344. Then *tube of* v in a bath of vi—the tube pointed axially.

8345. So that in the same bath of vi, a stronger solution points axially and a weaker equatorially.

8346. When a tube *of* vi was put into a *bath of* vi—there was perhaps a trace of axial power, but I doubt it.

8347. A mixture was made of 3 vols. of v or the standard solution and 1 vol. of water, so that it was not much weaker than v and considerably stronger than vi and vii.

8348. Tube vi points well equatorially in this fluid and also tube vii the same. But tube v points well axially—so that it is quite clear the weaker solutions point equatorially, or in fact are driven outwds. from the magnetic field.

8349. Then the tubes were suspended perpendicularly in this fluid of 3 + 1 water and their movements observed when the pole was made magnetic. Tube vii was repelled from the pole exactly as bismuth is in air. Tube vi also was repelled, but with less force. Tube v was attracted in the same fluid.

8350. Hence the power of the surrounding medium to cause the vertical attraction or repulsion of other bodies is very manifest—and as yet every thing seems to shew that these actions are exactly of the same nature as those with bismuth, etc. in air.

8351. It would be very curious if, after Iron, nickel and cobalt, the air were to prove the next most magnetic body. But perhaps some of the metals, as Platinum or silver, may come in.

8352. Will bodies that come axial attract and those that go equatorially not do so? Try same solution of Iron?
8353. Will the fact of attraction shew whether bodies are magnetical as Iron—Air for instance?
8354. Is a tube of solution of Iron a magnet and can it attract another tube?
8355. Must quickly try bismuth in a vacuum—and by letting in other gases, also in them. Air is becoming very important.
8356. Oxygen and Nitrogen may have great differences between them—the magnetic power of one neutralizing the deficiency of power in the other. As it is, Air is the most magnetic of all earthly bodies, except Iron, Nickel and cobalt.
8357. Not unlikely that the *Earth's magnetism* may reside essentially in *the air*.
8358. All the gases require careful examination and consideration.
8359. The magnetic condition and relation of the air, gases and vapours probably a very fine separate subject.
8359*a*. Very curious that the most and the least magnetic of substances comes out of the metallic list, and that ordinary bodies are between them.
8360. What relation will mercury have to vapour of mercury?
8361. What relation will water have to vapour of water—can easily try that.

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8362. Have now the large Enderby Magnet (7874, 8408), but with 443 feet more of the same wire on it, so that now there is 964 feet of wire on it in one length (by junctions). Battery, 10 pair of Grove's Plates.
8363. Arranged an air pump and its Jar so that the latter, about 3 inches in diameter, was between the poles of the magnet. It was furnished with a brass sliding wire and a cocoon silk suspension (7902) so that bars of glass, bismuth, etc. etc. could be submitted to the action of the magnetic forces in vacuo. The interval of the poles was 4 *inches*.
8364. *Bismuth bar* (8027) *in vacuo*. The bar was suspended and the air at first left in the Jar. On laying on the magnetism, the bar pointed equatorially very well.

8365. Then exhausted the Jar *very well.* The bar again pointed *equatorially*, and as far as I could perceive quite as well as before. This experiment was repeated several times.

8366. Let *Carbonic acid gas* into the Jar, so that the bar of *bismuth* should be surrounded by it. The bar set equatorially, and just as in Air or vacuo.

8367. Made a vacuum after the Carbonic acid, so that very rare carbonic acid should remain in the jar; still the effect was presisely the same.

8368. Repeated the experiment with the same result.

8369. Filled the Receiver with *hydrogen.* The bismuth bar pointed equatorially exactly as before.

8370. Made a vacuum after the *hydrogen*—still the same result, both in kind and degree. All were alike.

8371. So whether bismuth bar be in

Air, or vacuum after it,
Carbonic acid, or vacuum after it,
Hydrogen, or vacuum after it,

still it always points equatorially and apparently with the same degree of force.

8372. The tube of *Air* (8330) was now suspended in Air in the Jar and the magnetism laid on. It did not point *either equatorially or axially.*

8373. The receiver was exhausted—but the *tube of air* did not point either way in this vacuum.

8374. The Jar was filled with hydrogen gas—but the tube of air was as indifferent as before.

8375. It was then filled with carbonic acid gas, but still it remained indifferent.

8376. So that *tube of air*, in Air or hydrogen or carbonic acid or vacuo, is not affected by any of them, and does not point either equatorially or axially.

8377. The half inch *cube of bismuth* was suspended by a longer suspension in a taller Jar and brought nigh to one of the poles. When air was in the Jar, it was repelled on laying on the magnetism. When the air was removed and the best vacuum produced, the repulsion took place exactly in the same manner.

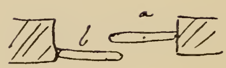
8378. The *copper bar* was suspended perpendicularly (8108) in the Jar near the face of the pole, and then its *set and revulsion* (   ) observed whilst air was in the Jar—and also after the air was removed. It was affected precisely in the same manner, both in *air and in vacuo*.

8379*. The air pump and Jar were removed and the two moveable pole terminations put in place, each armed with an iron cone which could be brought near to and opposite each other at pleasure. A slip of paper and finely powdered bismuth sprinkled on it were placed between the points, and then the magnetism laid on. By tapping the paper the bismuth was driven away a little from opposite each point, but nothing else occurred. It was not striking.

8380. With respect to currents produced in uniform fluids, they are not likely to occur, but still experimented. A little glass tube having coloured water up to a certain line and colourless water above, was held with the line of junction level with and between the points above (   ). On laying on the magnetism there were no signs of current or any change within the tube.

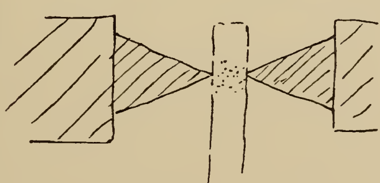
8381. Put a layer of ether on water and made the same experiment, but obtained no effect of alteration. Repeated the experiment with Alcohol and water, Chloride or Arsenic and hydrated chloride of Arsenic, and other arrangements, but obtained no signs of motion in the fluids.

8382. Are *solutions of Iron* magnets when magnetic? The two tubes V and VI (8330) of Iron were taken and the distance of the Magnetic poles so adjusted that the tubes, placed against the poles, would overlap a little. Then the tube *a* was suspended in such a position that, not quite touching its pole, still when the magnetism was laid on, its round end was drawn to the pole, and the tube took and kept the position it has in the figure. Then, retaining the magnetism, the tube *b* was brought in contact with the other pole (being held in the hand) and became of course as magnetic as *a*. In that state its end was approached to the free end of *a*, to see if there were any power to draw aside that end a little; but I could perceive *no attraction* between these two ends.



8383. Perhaps it was too much to expect, for after all, it must have been very small and the intensity of the magnetic curves passing through the air must be very great.

* [8379]



Magnetism of Metals.

8384. *Bar of lead* made from chinese lead selected and of the size of the former, being examd. for magnetism between the two poles, set axially as if magnetic, but nothing like in degree with the former bar of lead (8289, 8274). So that probably its magnetism is due to iron.

8384 *a*. *Bar of fine* TIN. Is not magnetic. Is caught and shews revulsion like copper, but not very strongly.

8385. As to strength of the magnet in its present state—the ½ inch cube of bismuth is well repelled and by either pole.

8386. *Revulsion of copper*, etc. The bar of copper slung perpendicularly and placed so as to be opposite a broad surface of the pole. It was then well *brought up and set* and well *revulsed*. If it were kept in an oblique position for a short time and then the magnetism taken off, there was strong revulsion. But if it were kept in the same oblique position for the same time, then *set by hand equatorially*, and then the magnetism withdrawn, there was *no revulsion*. The experiment was made in reference to any effect of a state of charge in the copper (   ).

8387. A plate of copper ⅔ of an inch in thickness was placed between the magnetic pole and the ½ inch cube of bismuth. The repulsions of the latter occurred just as well, and I think with the same degree of strength, whether the copper *were there or away*.

8388. Now wrought with tubes of *different substances* in masses of *media* (fluid).

8389. The tube of Air (8330) sets well axially in *water*—and in *Alcohol*—and in *oil of turpentine*.

8390. The *alcohol* tube (8330) sets axially and distinctly in *water*, but also the *water* tube sets axially in *Alcohol*.

8391. Again, the *oil of turpentine* tube—sets axially and distinctly in *water*—but the *water* tube also sets axially in the *oil of turpentine*.

8392. I believe that in these cases it is the bubble of air which is left in the tube with the fluid that determines the tubes to set axially (8390, 1); its influence predominating over any influence that the difference of the fluids might produce.

8393. The *oil of turpentine* tube in the *Alcohol*—was doubtful in its movements.

8394. In some of these expts., I could just perceive the effect of the copper wire suspending holder in producing set and revulsion, and when it alone was examined in the air, it was found capable of producing the set and a little revulsion. This would not interfere with the effects above, which belong to a *continuation* of the magnetic force.

8395. It is remarkable to consider the way in which powers seem at times to be attached to or to move freely among and through matter, and also the relation of action and reaction at such times. This illustrated by wire fixed to magnet yet revolving round it—also the revolving magnet—the power which causes the revolution being in its own mass.

8396. From experiments above (8364–71), it would seem that bismuth is not driven away from axial position by the tendency of air to occupy that place, for when air withdrawn, still it goes away. It appears rather to be absolutely repelled.

8397. But this effect might happen just as well as it does if a vacuum were more fitted to transmit magnetic influence than bismuth, and then it would still be the result of a difference in degree of quality.

8398. Is air a *normal point of condition*, or is it only intermediate in degree in one order of things, i.e. are bodies in two classes, air being the zero—or are bodies in one class, air being in the middle?

8399. If in two classes, then there is a *positive* and a *negative* in relation to magnetism.

8400. As to the degree of this power in the great mass of natural bodies, the indication we get of it is far greater in amount than any indication we can obtain of gravitating force on the same or a much larger scale of masses; yet how does gravity affect the earth and our system?

8401. It is impossible to think that this power can be in the earth and air, etc. etc. as generally as gravitation is, and yet have no relation of utility to the earth and to its magnetism.

8402. If there were no iron in the earth, or not enough to make it magnetic as a whole, still the powers now described would make it a magnet and would cause magnets on its surface to point.

8403. But whether this magnetism would reside in the air does not yet appear. Or that bismuth and Glass would go east and west because of power of air is not proved ( ).

8404. Still, the great mass of the earth ought to go equatorially, because it is as their bodies.

8405. So Saturn's ring is in the position it would take, revolving or not revolving, if the magnetism of Saturn were arranged according to its poles of rotation. Our moon also.

8406. The mass of air will tend to point in the direction at right angles to these heavier masses of matter.

8407. In the expt. of to-day, the bar of Heavy Glass was tried in air and vacuo (8363), and pointed equatorially and alike in both.

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8408. The great magnet has now (7874) six coils of wire about it, five on the ends of the two arms and one at the bend, which I will call the sixth. The wire is copper and 0·175 or rather more than $\frac{1}{6}$ of an inch in diameter, No. 7 of commerce. The first coil,

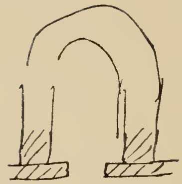
		feet	inches
beginning to count from the iron upwards, contains		166	5
The second coil	,,	172	5
third coil	,,	182	7
fourth coil	,,	192	0
fifth coil	,,	195	0
and the sixth coil at the bend	,,	56	0
so that the whole length of wire in one succession is		964	5

8409. The whole weight of the magnet and stand is about 281 lbs. and of this about 93½ lbs. are copper, for 443 feet of the wire weigh 43 lbs.

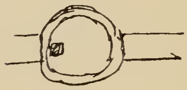
8410. When, using ten pair of Grove's battery, but not in good excitation, the force of the whole was sent through the *first coil*, the power of the magnet was good but not great. On including the second coil, it increased considerably—on including the third, it increased again well, and so on to the fifth and even the sixth coil, shewing that all these as one series, and the 10 pr. of plates were wanted to bring out the force of the *iron core*. Was very good at last and would be better still with a better battery.

8411. The magnetic poles were terminated by the two cones

(8379) and these brought close together, i.e. within the $\frac{1}{16}$ of an inch—and all being dark, the magnetic force was superinduced; but *no light* was visible either when *air*, or *heavy glass*, or *chlorophane* was the dimagnetic between the poles.



8412. The plane poles were no[w] experimented with in various ways. An iron tube about $2\frac{1}{2}$ inches in diameter and $\frac{1}{6}$ of an inch in thickness in the side, was placed between the poles and the $\frac{1}{2}$ inch bismuth cube suspended within it, near to one of the poles. When the magnetism was laid on, the cube was repelled and obeyed apparently as well as if no iron were there.



8413. Doubtless if there had been sufficient iron, it would have carried on all the force, and so either diminished the action on the bismuth or made it cease altogether. In the present case, the amount of iron is not enough, and so there is action across the space from one side of the tube within to the other.

8414. The bismuth cube was placed about $1\frac{1}{4}$ inch from the pole on the outside (the iron tube being now removed), and its repulsion was observed when that space was occupied by *air*; or a plate of lead an inch thick intervened, or a plate of copper $\frac{3}{4}$ of an inch thick. The bismuth appeared to be equally repelled in all cases.

8415. Three plates of bismuth amounting to 1 inch in thickness were introduced, but the effect was the same.

8416. The *lead* seemed in a very slight degree to favour the repulsion, but as it is probably magnetic from iron (8044), it may probably do so by virtually diminishing the distance between the bismuth cube and the pole.

8417. The perpendicular copper bar (8229) is at a *given distance* affected exactly in the same manner, whether *air* or *bismuth* or *copper* intervene.

8418. When the thick copper plate (8414) was standing near the face of the magnetic pole on a wooden block, it was a little repelled on making the apparatus magnetic. When the electric current was stopped, the copper fell to or rather was pulled towards the pole with a force that was remarkable, for it gave a sudden tap or pressure to the finger held against it. There is evidently a little more power than the falling back by gravity; there is a *pull back* (   ).

8419. A Galvanometer has been placed in the froggery, about

25 feet from the magnet. It is not disturbed on rendering the apparatus magnetic. Wires have been attached to it, and as a record, note here that the wire which is uppermost *at the door*, when a current goes through it *to* the galvanometer and returns by the under wire at the door—then the *North end* of the indicating needle goes *West*.

8420. Now helices and wires were connected with the galvanometer and taken between the poles of the magnet, and to understand the results the annexed is a plan*, shewing the state of the upper surface of the magnet and helices, etc.; thus as regards the two poles, the wires and arrow heads shew how the current from the battery was passing round them *above* when contact with it was made. *h* is a little helix of covered wire and the arrow by it shews how the induced current passed through its upper part under circumstances to be described. *a* and *b* indicate the sections of a *vertical wire* which was carried across the curves either from *a* to *b* or *b* to *a*.

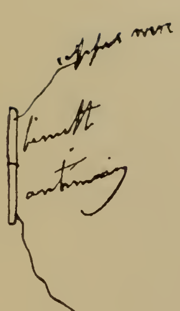
8421. Now the helix *h* being placed as marked, near one pole but between both, on making the magnet active, a current was induced in the helix in a *contrary* direction to that passing round the magnet, in accordance with my old researches (Exp. Resh.). When the electric current was stopped—a current the reverse of the former, or the same as that which had moved round the magnet, was induced in the helix.

8422. Whether this helix was only a quarter of an inch in diameter, or 4 or 5 inches, or a foot in diameter, the direction of the currents was the same as that described.

8423. When the Magnetic force was continued and a perpendicular wire carried from *a* to *b* across the curves, the induced current in it was upwards. When the wire was carried from *b* to *a*, the induced current was downwards.

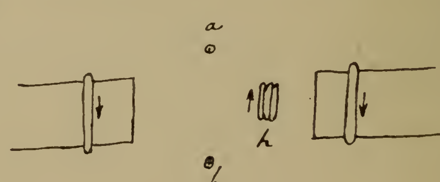
8424. All this is in accordance with the old results.

8425. When instead of a copper wire, a bar of *bismuth* or of *antimony* was carried across the curves (8423), the current induced in them was exactly the same as that induced in the copper wire.



8426. When a compound bar of bismuth and antimony was employed, the effects were exactly the same. The differences of metal or arrangement made no alteration (10328, 9).

* [8420]



8427. The *stationary compound antimony bismuth* bar did *nothing*.
8428. Worked with bars of copper and bismuth in a bath of mercury between the poles, keeping the lighter metals down and beneath the surface by fixing them to wires of copper *weighted* higher up.
8429. *Copper* in *mercury* was stopped and revulsed, just as in air or in water.
8430. Bar of *bismuth* in *mercury* sets equatorially and very well.
8431. A *tube of air* in *mercury*—sets axially—but not very strongly or distinctly. It however had a great weight to carry of equipoise above and that would embarras its movements. Repeat the expt.
8432. Worked on the magnetism of certain metals and the probable effects of heat on others.
8433. A piece of thick *Platinum wire*—was magnetic, and the same when as *hot* as I could make it by a spirit lamp.
8434. *Copper*—not magnetic—when hot almost to melting, nothing additional or particular.
8435. *Gold*—another piece than before (8046, 8279). Magnetic—and continued so when heated until it fuzed to the sustaining platinum wire (8444).
8436. *Silver*—the piece magnetic and rather strong—but when very hot and just melting, it appeared to lose its magnetic force.
8437. I precipitated some silver from the nitrate by copper, washed the metal, squeezed it into a cake and dried it. In this adhering spongy state, it was *not magnetic*, but was repelled en masse as bismuth or tin. Hence silver is not a magnetic metal.
8438. When moving, it could be caught and stopped in its vibration and could be revulsed just like copper—but the effects are feeble, doubtless because the silver has very slight adherence as a mass—very poor contact for currents of electricity.
8439. When a piece of the cake was broken off so as to represent a long bar (8229) and hung up perpendicularly near the pole, the bar could be stopped and revulsed very fairly indeed, in the manner of copper (8231).
8440. I have received from Mr Askin of Birmingham, two bottles of solution which he has prepared for me perfectly pure, of Nickel and cobalt. They are

8441. The *solution of Nickel*: was *strongly magnetic*—more so perhaps than Iron.

8442. The *solution of Cobalt*: was *strongly magnetic*.

8443. A cylinder of *Ivory* sets well *equatorially*.

8444. The gold pieces (8435) have copper action—but are too small and also are magnetic.

8445. Tried to clear some copper filings of iron by the great magnet, but could not deprive them of magnetism altogether. I wanted to see if copper finely divided would set as the bar does. The question is much answered by the precipitated silver.

8446. The motion of the bismuth or heavy glass is not a motion which regards the curves as to their direction along or across: the substance is in the curves just as if they were *not there*, if the curves are every where of uniform force. But it is a motion tending to remove them out of their sphere, and away from their influence.

8447. Such also is the case with a helix or wire in which currents are induced by making a magnet or approaching one, for the currents are in the contrary direction, and that occasions repulsion.

8448. The setting of the copper is in reality a repulsion, but a repulsion combined with an oblique polarity from the momentary induced currents formed. It is not the attraction of the approaching side but the repulsion of the other side and the center of gravity. Then when contact is broken, the corresponding attraction of these parts comes on, as is easy to be understood by consideration of the old phenomena.

8449. If the air be in a normal condition, then these other bodies must surely be in an electrotonic state. If the air does not act magnetically, or a vacuum, then these must act by their own special and temporary condition.

8450. In that case, the Electronic state must be the condition which carries on the magnetic chain of action from one thing to another.

8451. As the setting up of the currents repels the bodies, so the state they acquire may perhaps continue that repulsion. But then the currents set up cannot be those of the whole mass, for that ceases instantly; but currents round particles. If there be such currents, it must be round the particles; also for another reason, namely, that they must exist in insulators, as heavy glass, as well as in conductors, like bismuth and copper.

8452. Ought to be some contrast between bismuth and copper acting as screens.

8453. In the joint magnetization and permeation of iron, it exhibits the double character of a Magnetic and a dimagnetic.

8454. Remarkable that air and gases are those bodies which at the same time shew no action on light, and are at the zero condition in relation to dynamic magnetism.

8455. Tried a multitude of bodies as to their magnetism, either at one pole or between the two poles of the large horseshoe magnet (   ). Tried them either in the laboratory bottles or in tubes. I will group them under the metal from which they are derived, and first for Artificial preparations of Iron. The substances are solid except expressed as solution—the preparations are often as duplicate, i.e. in two bottles being different preparations.

Oxide of Iron	Tinct. Sesqui chloride
Prot. oxide of Iron	Iodide of Iron
Micaceous ox. Iron	Do.
Sesqui oxide	Do. solution
Per oxide	Nit. Iron—decomposing
Rouge or Colcothar	Prot. sul. Iron
Carbonate (so called)	„ „ dry
Deposit from Wiesbaden Water	Per sul. Iron
Ferrate of Potassa (impure)	„ „ dry
Chloride of Iron	„ „ solution
Proto chloride	Sulphuret of Iron
Per chloride	Proto phos. Iron
Oxy. Mur. Iron	Sesqui Phos. „
Ammonia chlo. iron	Do.
Proto mur.—solution	Prussian blue
Per muriate—solution	

All these were magnetic.

8456. Crystals of *Ferro pruss. potassa* were repelled and set equatorially. The Red ferro pruss. I think would have done so also but for adhering blue matter. Prussian blue in some such state.

8457. Here the iron in the acid *not magnetic*, or if so overpowered by the other force.

8458. Native substances ferrugenous.

Bog iron ore	Octoedral iron
Carbonate of Iron	Chromate of Iron

Brown ball of Iron pyrites	Bi sulphuret of Iron
Radial fragment of yellow iron pyrites	Arsenical iron pyrites
	Yellow copper pyrites

All these more or less magnetic—the sulphurets presented some specimens which were the least so.

8459. *Green bottle glass tubes* considerably magnetic—so as to be quite useless in these investigations. Flint glass not so, sets equatorially.

8460. So compounds of iron *all magnetic* except the Ferro prussiate—this curious and is a distinct relation of magnetism to the chemical relation of the body.

8461. *Nickel compounds.*

Native nickel	Sulphuret of nickel
Ore of nickel	Mur. ammonia and Nickel
Oxide of Nickel	Phos. Nickel
Do.	,, ,, dissolved in M.A.
Sulphate of Nickel	Nitrate of nickel
,, ,, solution	,, ,, solution
Sulphate of Potash and Nickel	Ammoniuret of Nickel in solution

All magnetic.

8462. *Cobalt.*

Oxide of Cobalt	All magnetic.
Mur. cobalt	
,, ,, solution	
Sulphate cobalt	
Oxalate of Cobalt	
Do.	

Cobalt blue glass appeared to be magnetic.
Borax coloured blue by cobalt, doubtful.

8463. *Manganese* (See 7414, 7416).

Pure green protoxide Manganese in a tube—magnetic enough.

Protoxide Manganese by hydrogen	Sulphate manganese
Pure prot. oxide manganese	Ammonia sulphate Manganese
Deut. oxide manganese	Phosphate
Chloride manganese	Carbonate
,, ,, in solution	Borate
Nitrate manganese solution	

All Magnetic.

8464. As all the compounds of a magnetic metal are magnetic, so from the characters of compounds of a metal may refer back to probable condition of the metal. As all the compounds of manganese tried are magnetic, and as in the solutions and several of them, as the Ammon. sulphate, chloride, etc., it is hardly possible that iron can account for the power—for if there in minute quantities, it would not produce the effect observed—so it may be deduced that Manganese is magnetic.

8465. It also appears to me that the compounds retain much more of the original force belonging to the metal than the corresponding compounds of iron do in proportion.

8466. Lead ( ). A solution of the Acetate of lead free from iron has been precipitated by zinc, so as to form a metallic tree. The precipitated lead has been washed and pressed together into the form of bars to try its magnetic condition. The metal is still magnetic and I almost think independant of Iron, but must try more carefully still.

8467. *Arsenic* ( ).

The metal was sublimed in two tubes and the cylinders formed examined. They were clearly magnetic and I think of themselves, i.e. without iron—I think nothing could be in the Arsenic except what rose by sublimation.

8468. Three different cylinders were magnetic together, and they were also each magnetic when tried separately.

8469. Platina ( ) precipitated by copper itself free from magnetism. The platina powder well washed with muriatic acid several times—then with water—dried and put into a flint glass tube. *Was Magnetic*. The tube and support was not. Think that platina is magnetic per se.

8470. *Palladium*.

Some Brazillian Palladium dissolved in N.M.Acid and this precipitated by zinc, and then washed clean in dilute Mur. acid and water. The powder in a flint glass tube *was magnetic*. Still the process of purification not very good.

8471. *Sulphuret of Palladium*.

Some crystals of *Ammonio-muriate of Palladium* from Wollaston were very well repelled and pointed equatorially.

Hence expect that palladium is not a magnetic metal, but must ascertain.

8472. *Rhodium* ( ).

Muriate of Rhodium, Wollaston—not Magnetic—is repelled.

Muriate of Soda and Rhodium, Wollaston—sets well equatorially.

8473. *Titanium.*

Oxide of Titanium. Magnetic rather—strong. Wollaston found Titanium magnetic and this helps to confirm the opinion that the metal is truly magnetic.

8474. *Chromium.*

Dry green oxide of chromium is magnetic.

Carbonate of Chromium—Magnetic.

Warrington's chromic acid. The least trace attracted magnetically.

Bi chromate of Potassa. Commercial—but good crystals: pointed magnetically, i.e. axially.

8475. *Molybdenum.*

Native *sulphuret*—not Magnetic—repelled.

Native Molybdate of Lead. Contents of the bottle as a whole magnetic—but two good large clean pieces, being tried separately, were not magnetic.

8476. *Uranium.*

Oxide of Uranium—repelled.

8477. *Cerium.*

Hydrated prot. oxide of cerium—Magnetic.
Carbonate of cerium—Magnetic.
Sulphate of cerium and potassa—Magnetic.
Muriate cerium—solution—Magnetic.

I think cerium is a magnetic metal.

8478. *Sodium* in a bottle—black and carbonaceous—Magnetic.
8479. *Potassium* Do. Do.
but the salts of Potassium and sodium are repelled and set equatorially. Probably impurities in the metals.

8480. *Tungsten.*

Tungstic acid—not magnetic—repelled.
Per oxide Tungsten—Do. Do.

In reference to the metals just mentioned and to others, experimented with the following bodies:

8481. *Arsenic*—White Arsenic
8482. *Chromium*—Chromate of lead
Bi chromate of potassa
8483. *Platinum*—Perchloride Platinum
8484. *Lead*—Nitrate of Lead
8485. *Silver*—Nitrate silver
Chloride silver
8486. *Magnesia*—Chloride
Sulphate
8487. *Ammonia*—Nitrate
8488. *Potassa*—Nitrate
8489. *Strontium*—Chloride
8490. *Antimony*—Oxide
8491. *Bismuth*—Nitrate

} none magnetic—all repelled and pointing equatorially.

8492. Now tried the effects of heat on a few of the compounds of the Magnetic metals.

8493. *Oxide of cobalt*—in a flint glass tube. Tried cold and heated by a spirit lamp—as magnetic when hot as when cold.

8494. *Oxide of Nickel*—heated in the same manner—tempted to think it more magnetic hot than cold. The heat applied was far higher than that which causes the change in the magnetic force of the metal nickel. So it would seem as if heat and chemical change affected in nearly an equal degree the force of the metal, i.e. that the magnetic force of nickel is about the same whether it be heated above boiling oil or whether it enter into chemical combination.

8495. Askin's solution of Nickel, heated to the boiling point. No change in the degree of magnetic force.

8496. Askin's solution of cobalt heated to the boiling point—as magnetic as before.

8497. *Rouge* and *hæmatite* heated in tubes—were more magnetic than when cold; but then they remained extra magnetic when cold. The vapour of the alcohol and the heat had no doubt reduced the oxidation of certain particles. Sol. Sulphate—not altered.

8498. *Magnetic curves.* I cannot see any difference in the forms of the curves indicated by iron filings on paper or a frame over magnetic poles, by putting a plate of bismuth or of copper beneath.

8499. Neither can I find any effect of bismuth or copper when employed in various ways as screens between a magnet and a magnetic needle.
8500. The heat of the center of the earth does not annihilate all the magnetic force of iron, nickel and cobalt; but will as respects the power still retained make all ferruginous and other matters like it as very soft iron.
8501. Nothing magnetic of itself can be there; i.e. no magnet would remain a magnet there except through the influence of external induction.
8502. Hence all ferruginous and magnetic matter is free for the process of induction from such external causes or from currents in the crust of the earth. *No central magnet.*
8503. Heat does not alter the magnetic relation of copper or metal free ordinarily from common magnetism.
8504. Any metal not yielding magnetic salts is probably not magnetic per se, or at least only slightly so. So Manganese and Cerium and Titanium most probably magnetic metals.
8505. Query the condition of the Vapour of a magnetic body, as Chloride of Iron.
8506. Write Sabine as to the effect of Sun and Moon in position on the earth's magnetism.
8507. Air and space cannot go for nothing in magnetic phenomena of our globe—because they are midway in the great list of bodies.
8508. If light or rays a cause of magnetism, they then are as transparent bodies eminently fitted for its action.
8509. The iron of the earth will act as a core, and it must be powerful.
8510. Meteoric stones bring us Iron, Nickel and Cobalt. They almost surely indicate external magnetic relations of the earth. As surely almost as the gravitation which brought the stones to us and connects us with the masses from whence they come.

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8511. The new state is in striking *contrast* with the old.
8512. Trace a body through *change of form*—first solid—then liquid—then gaseous.

8513. So also with a magnetic body, as *chloride of Iron*—solid, liquid and gaseous.

8514. I incline, by my view of induction through particles, to think that all bodies are in *one* magnetic list—but the facts as yet rather sustain the view of *two*.

8515. Must consider the dual state first and with it the application of Ampère's theory.

8516. Afterwards the theory of one state and air intermediate.

8517. Then the use of this power in nature—first the communication of magnetic force and afterwards terrestrial effects and conditions.

8518. *Mr Askin's solution.*

That of *Nickel* is a muriate and nearly neutral—is soluble entirely in ammonia and contains no trace of Iron.

8519. The *Cobalt solution* is almost a pure muriate but contains a trace of Sulphate—it is also nearly neutral. It is entirely soluble in ammonia, and contains a little Nickel, trace, but *no iron*.

8520. The Ammonio sulphate of Manganese (8463) is quite free from Iron, Nickel or Cobalt.

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8521. Have had ten pieces of Wollaston's Palladium and Platinum from the Royal Society, namely—

Platinum, an ingot		No.
Palladium, ingot		No. 1
,,	ingot	2
,,	rolled	8
,,	,,	12
,,	,,	13
,,	,,	14
,,	,,	15
,,	,,	16
,,	,,	17

All were magnetic. No. 15 the least so. Took No. 13, weighing 2 oz. 10 gr., to work up.

8522. Worked with metals and metallic preparations to ascertain their real relation to the Magnetic or Diamagnetic class.

8523. *Lead* (   )

Nitrate of lead has been crystallized carefully thrice.

8524. *Thin glass tubes* (flint glass) which, having a small mass, did not point sensibly equatorially or very feebly so, were used to contain this and other substances.

8525. Crystals of the *Nitrate lead* in such a tube pointed well equatorially.

8526. *Chloride of lead*, prepared from these crystals by pure Mur. Acid, washed, dried and inclosed in the tube (being in powdery crystals) pointed well equatorially. When the tube was suspended by the side of the pole it was well repelled.

8527. A cylinder of this chloride fuzed would give a very good case of the Diamagnetic class of bodies—perhaps as good as phosphorous.

8528. *Sulphate of lead* prepared from the same nitrate (8523) was well repelled by the Magnetic pole.

8529. A portion of the Nitrate in solution was precipitated by a piece of pure zinc, and the precipitated lead washed with dilute Nitric acid to remove subnitrate, etc. This specimen of lead, in a glass tube, pointed *equatorially*, and was repelled by the side of the pole, but not as bismuth.

8530. So lead is a *diamagnetic*, but comes near zero.

8531. Another specimen of same lead pressed together into a flat porous cake—same result.

8532. *Acetate of lead*, crystallized twice: crystals in a tube well repelled.

8533. *Fluoride of lead*—repelled.

8534. *Fuzed litharge*—prepared from pure nitrate by heat—repelled.

8535. So all agree as to character of lead and its compounds—non magnetic.

8536. *Ordinary carbonate of lead*—repelled.

8537. *Arsenic.*

Have sublimed the metal a third time, and have now two hollow cylinders. These seem very near the 0° point. They seem to have the least tendency to axial position. Still, when hung perpendicularly by side of the pole, they were not attracted. There was the least signs of repulsion. Being broken down and put into a glass tube (8524), there was again the least signs of repulsion, but even the glass tube might perhaps do as much.

8538. This tube and fragments would set equatorially, but very weak.

8539. Believe Arsenic not to be a Magnetic metal, but a Diamagnetic, the least degree removed from 0°.

8540. On using the short cylinders of Arsenic slung horizontally between the two poles, they pointed axially. This was because, being short, if they were found by the magnetic force in this position thus, the ends went inwards according to the phenomena before described and explained (8293–6).

8541. *White Arsenic* of commerce, crystallized. Very slightly magnetic, but not far from 0°.

8542. *Crystallized Sulphuret of Arsenic.* Woulfe's specimen—repelled fairly at the side of the pole—weakly. Between the poles points well equatorially.

8543. *Dry fluid chloride of Arsenic* in a tube between the poles points well *equatorially.*

8544. *Arseniate of Potassa*—a large crystal points well equatorially.

8545. *Platinum.*

A solution of Platina cuttings in N.M.Acid was precipitated by acid pure Muriate of Ammonia, and the triple Muriate washed and dried carefully.

8546. This *ammonio chloride platinum*, pressed into a glass tube, is distinctly repelled when hanging perpendicularly by the side of the pole.

Two lumps of it arranged bar fashion horizontally on the paper stirrup pointed well and fairly *equatorially.*

8547. These two pieces were reduced by heat in a flint glass tube, and the spongy platina pressed into a thin flint glass tube. The tube had been previously hung perpendicularly by the side of the pole, and I could not perceive that it was affected by the magnetism either way. But when in the same position and containing the reduced platina, it was distinctly *attracted* and *magnetic.*

8548. Other portions of the *Ammonio chlo. platinum* (8546) were reduced in a clean platinum crucible. Part of the sponge was pressed into a flat long cake—other part was pressed into tubes—but all proved in every way to be magnetic.

8549. Begin to believe that Platinum is Magnetic of itself and a little way removed from 0°.

8550. The solution of *chloride of platinum* (8545) from which these preparations were obtained, set well *equatorially*. In solutions the water must act by its diamagnetic force and go for a good deal.

8551. A piece of dry *chloride of platinum* set well *equatorially*.

8552. *Titanium.*

Our Oxide of Titanium—have acted on it by carbonate of Alkali and heat, and redissolved by pure Muriatic acid—filtered—diluted and boiled and collected the first part which was thrown down. This washed and dried very carefully. When in a tube it pointed axially—and when hanging perpendicularly by the side of the pole, it is attracted. Believe it to be weakly magnetic per se.

8553. *Manganese.*

Sol. *Sulphate of Manganese*, pure—well magnetic.

Sol. *Chloride of Manganese*, pure—well magnetic.

Dry crystals of *chloride manganese* in a tube—strongly magnetic.

8554. *Sulphate of Ammonia and Manganese*—crystallized thrice and quite pure. The crystals in a tube point well axially and are well attracted by the nearest pole. A solution of this salt in a tube—also the same.

8555. Manganese and its compounds are magnetic.

8556. *Cobalt.*

Glass coloured blue by cobalt points axially and is well magnetic.

8557. *Iron.*

Ordinary crown glass—is magnetic—it points well axially and is attracted.

8558. A flat crystal or plate of yellow ferro prussiate potassa sets well equatorially—no signs of magnetism.

8559. Solution of the same salt—sets well equatorially.

8560. Some of the solution with pure Mur. acid and a little N.A. digested until green in colour—still it set well equatorially and was not magnetic.

8561. *Rhodium* (8591).

8562. Wollaston's crystals of Muriate of Rhodium—contained in a tube—set equatorially very distinctly.

8563. Wollaston's Muriate of Soda and Rhodium (   ), being a piece of the salt, also set equatorially and fairly.

8564. *Cyanide of Mercury*, in crystals—points equatorially but only feebly—as if there might be a little iron in it.

8565. *Palladium.*

8566. A part of No. 13 of the Wollaston Palladium from the Royal Society (8521) was dissolved in pure N.M.Acid.

8567. Some of the acid solution precipitated by a solution of the cyanide of Mercury (8564) and the cyanide of Palladium washed and dried. It became a hydrated green coloured body. The green colour appears to belong to the palladium compound.

8568. This cyanide in a tube is almost 0°, but it does point distinctly equatorially and is not magnetic.

8569. Part of the cyanide decomposed by heat in a close vessel and the palladium put into a thin flint glass tube. Then points well axially—is attracted at the side of the pole, and is well magnetic—more so than platina.

8570. Another portion of the same cyanide heated in an open platina crucible until all appearance of burning over—is also magnetic.

8571. *Sulphuret of Palladium*—black powder—from the shelves—is almost neutral, but has slight trace of magnetic force.

8572. *Red Ammonio mur. Palladium* from the shelves—when in a tube, points very fairly equatorially and is *not magnetic.*

8573. *Chromium.*

8574. *Yellow chromate of potassa*—crystallized three times. The crystals in a tube point equatorially but not strongly—distinct though weak.

The solution of this salt points equatorially powerfully by comparison and shews how much the *water* adds to the effect of the salts.

8575. *Bichromate of potassa*—crystallised three times. These crystals in a tube give the least possible trace of magnetism. They do not set equatorially but axially.

A solution of the salt sets well equatorially and so the effect of the water is well shewn here—and in the former part, the greater abundance of chromic acid in the bichromate as compared to the chromate, well shew its magnetic influence.

8576. When a tube of *bichromate* was hung perpendicularly by the side of the pole, it was not sensibly attracted or repelled, but as nearly neutral as may be. When the yellow chromate was hung up in the same way, it was distinctly repelled.

8577. A portion of the solution of bichromate had pure Muriatic acid and a little alcohol added to it, and was heated until green from the formation of Chloride of chromium. This solution in a tube was strongly magnetic.

8578. Another portion treated with pure sulphuric acid and alcohol in the same way, was even more strongly magnetic than the former. It is beautiful to see the magnetic force developed as the chromium is reduced in its state of oxidation.

8579. A portion of the Crystals of bichromate (8575) were heated per se in a platina crucible to reduce one part of the acid to oxide. The mass was then carefully washed and the crystallized oxide of chromium collected and dried. Being put into a tube, it was strongly magnetic.

8580. Doubtless Chromium is a magnetic metal.

8581. *Cadmium* ( ).

A cylinder of cadmium, upright by the side of the pole, was well repelled.

8582. *Zinc* ( ).

Pure zinc is well repelled.

8583. *Sulphuret of copper, Artificial* ( )–is repelled distinctly though only slightly–not far from 0°.

8584. *Gold* ( ). Wire and foil from Mr Johnson, believed to be pure.

8585. *Not Magnetic*: is repelled distinctly but not strongly. The wire was in a coil, and if hung up with plane of coil vertical, it was a helix in which the induced currents on making and breaking contact were easily shewn by the motion in the two directions, giving on a large scale of current the copper phenomena. Hence to obtain correct indications with the coil, it was suspended in a horizontal position and brought near the pole in a part where the magnetic lines of force were also horizontal, and then no effect of induced currents could interfere. The repulsion was then seen alone and distinct.

8586. This motion of the coil is doubtless the phenomenon that Ampère obtained.

8587. *Palladium*. Wire and foil from Mr Johnson (8590).

Both were clearly magnetic. The foil pointed edgeways to the edge of the pole and was attracted. The coil also attracted. If pure, then surely Palladium Magnetic.

8588. *Platinum* ( ). Wire and foil from Mr Johnson.

Both clearly attracted and magnetic.

Also in bottle from Mr Johnson.

Black platina pure
Spongy platina
Prot. oxide Platina
Oxide platina
} all were magnetic in the bottles.

8589. The black platina was taken out of the bottle and put into a thin flint glass tube. Then the bottle alone was not magnetic or attracted, but the platina in the tube was. So the bottle will not shew magnetism as belonging to the body within, and in such cases the trial *in* the bottle is good.

8590. *Palladium* (8587).

Chloride (from Mr Johnson) was repelled whilst in the bottle and also when taken out and put into a thin flint glass tube—is therefore not magnetic.

8591. *Rhodium* (8561).

Oxide (from Mr Johnson) attracted whether in the bottle or the tube, and therefore magnetic.

8592. A very fine crystal of the Sodio chloride of Rhodium from Mr Johnson—pointed well equatorially and was not magnetic—was just as Wollaston's Crystal.

8593. *Iridium* ( ) from Mr Johnson.

The metal in powder—very magnetic indeed—looks much like iron action.

8594. *Oxide of Iridium*—a little magnetic—more as prot. oxide of Iron than per oxide—i.e. think it magnetic of itself.

8595. *Chloride of Iridium*—in good crystals. Strongly magnetic—more so than the oxide.

8596. *Osmium* ( ) from Mr Johnson.

Metal—very slightly attracted—but clearly.

Prot. oxide of Osmium—black and in scales—very slightly attracted—Do.

Osmic acid in transparent crystals—*repelled*, not magnetic.

8597. *Titanium, oxide of* ( ) Mr Johnson. Slightly magnetic.

8598. As to Polarity of bar of Bismuth. Hung up a horizontal bar—brought it into a quiet condition as in fig.—bent a large soft iron nail as shewn and put it against the pole. The head of the nail would of course be also N., but I could not find that it exerted any attraction on the further end of the bismuth bar; which if the bismuth had been polar it might perhaps have done.

8599. A cube of bismuth 1 inch in side, swings very well; as well as a smaller one and shews repulsion. Suspend it by two threads to avoid turning.

8600. Have filled five tubes with fluids and marked them thus:

× Water
++ Alcohol
|' Oil of turpentine
+ Ether
— Olive oil

8601. *Sodium.* My piece of Sodium in the bottle—*well repelled*—not magnetic. Naphtha would of course add its effect.

8602. The following metals shew action of copper—i.e. if rotating, they stop, being in the mag. curves. Those at the bottom of the list are the weakest:

Gold
Zinc
Cadmium
Platinum
Palladium
Lead
Antimony
Bismuth

} The lead small—Antimony less and Bismuth still less, but the power was there.

8603. Probable list of the metals not far from their order in the list:

Iron
Nickel
Cobalt
Manganese
Chromium
Cerium
Titanium
Palladium
Platinum
Osmium

Tungsten
Iridium
Rhodium
Uranium
Arsenic
Gold
Copper
Silver
Lead
Mercury
Sodium
Cadmium
Tin
Zinc
Antimony
Bismuth

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8604. Worked with the large horse shoe magnet (   ) and ten pair of Grove's Plates.

8605. Tried the following liquids in Glass tubes surrounded by *Air*.

Water—stood equatorially—very well.
Alcohol—Do. . . . —well.
Ether—Do. . . . . —well.
Oil Turpentine—Do. . —exceeding well.
Olive oil—Do. . . . —very well.
Nitrous acid (*liquid*)—Do.—moderately.
Suls. acid (*liquid*)—Do. . —moderately.
Chloride Arsenic—Do. . —well.
A Tube alone—Do. . —very weakly.

8606. In comparing such things as these, it is important to remember what great advantage is given one over another by a little greater length of the tube—and also more weight of substance.

8607. *Palladium*.

Some good crimson amm.-muriate has been digested in Nitro muriatic acid with heat and then washed and dried. This salt in a tube vertical by the side of the magnetic pole is *repelled*.

8608. Some of this Amm.-muriate decomposed in a clean Berlin basin by heat and the metal obtained. This *Palladium* in a vertical tube was *attracted and magnetic*—more than platinum, I think.

8609. Some of Wollaston's Palladium (   ) dissolved in N.M. Acid and the solution filtered, evaporated, concentrated, diluted, etc., and then precipitated by a piece of pure zinc, itself not magnetic. The precipitate was separated at five different times, washed with dilute Mur. acid, then with water, and dried. Some was in powder and was put into tubes. Other specimens were in bright metallic crusts, and this was suspended by a fine copper wire, no tube being used. All were magnetic. I believe *Palladium is magnetic.*

8610. *Iridium.*

Have received from Mr Johnson a specimen of the metal believed to be quite pure and also a specimen of the *Ammonio chloride of Iridium.* The latter is attracted and fairly magnetic, but the metal is hardly so. It is the least trace magnetic, but far too little to correspond with the Ammonio chloride, and therefore probably both are magnetic from Iron or some other body.

8611. *Tungsten.*

Have prepared carefully a specimen of the black oxide of Tungsten and believe it free from Iron. It is quite indifferent and is not magnetic or repelled.

Have another specimen of the same oxide from the shelves—it is a little magnetic. Believe that the pure oxide is indifferent.

8612. *Molybdenum.*

Oxide from off the shelves—is magnetic—cannot trust it.

8613. *Cerium.*

Have prepared a sulphate of cerium and from it the sulphate of potash and cerium. The sulphate of potash crystals used were perfectly pure and not magnetic, but repelled. But the Sul. pot. and cerium crystals *were magnetic.*

Two other specimens of the triple salt also magnetic.

A large piece of crust of triple salt formed on the Sul. potash—was well magnetic.

No doubt that *pure cerium compounds* are magnetic.

8614. *Uranium.*

The hydrated yellow oxide—the heated black oxide and the carbonate from the laboratory shelves were all magnetic and the black oxide much. But a specimen of yellow oxide from Germany

(Berlin, I believe) was quite neutral, and a specimen of the black or heated oxide from thence—only very slightly magnetic.

Hence the compounds are probably not magnetic at all or very little so.

8615. *Phosphorus* in Alcohol points equatorially—weakly but clearly.

8616. The following liquids contained in thin Glass tubes were surrounded by water—and afterwards by Alcohol, and their position or tendency to it ascertained. A tube of the same liquid as that circumambient was also used to shew the effect first of the tube alone.

8617. In *water*:

Water in tube—as nearly neutral as could be.

Alcohol—Do.—least tendency to axial p.

Camphine—distinctly axial—bubble of air in tube small.

Ether—feebly axial—bubble of air space very small indeed.

Olive oil—least trace axial—least bubble of air.

Chloride of Arsenic—Much air here, perhaps one fourth of the tube, yet was nearly neutral—so probably equatorial if full.

Sulphurous acid—liquid—about $\frac{1}{5}$ gas space—almost indifferent.

Nitrous acid—liquid—air in—feebly axial, probably due to gas space in the tube.

8618. The same water and alcohol tube were then put into *Alcohol*:

Alcohol tube. Neutral.

Water „ Neutral also.

8619. So the differences of liquids not easily obtained this way—for both the glass tube and the bubble of air interfere.

8620. Proto sulphate of Iron and also its strong solution is magnetic. Water is diamagnetic. Therefore we ought to be able to neutralize the one by the other, and have the power of making a solution which will stand by the side of air, a vacuum, etc.

8621. Took pure water and acidulated it with a little sulphuric acid. When in a tube it was repelled in air. Added a few drops of saturated solution of proto sulphate of Iron—still it was repelled in air and pointed equatorially either in air or in water, the glass tube helping.

8622. Added more sulphate of Iron, and now it was nearly neutral in water. Added more Iron—still in water nearly indifferent. Added more iron, and this time it was feebly axial in water—but equatorial in air.

8623. Added more iron and now in air was clearly equatorial and in water clearly axial. The movements were feeble but perfectly distinct (8640).

8624. There can be no doubt but that by the addition of more Iron, it might be brought to a neutral condition in air and would then point well axially in water—as air itself does.

8625. So can represent air by an actual magnetic body using water as the circumambient medium.

8626. *Air* in tube in circumambient *Mercury*—air sets axially.

8627. It was very beautiful to observe here the effect of the revulsion of the mercury. When the magnetic power was taken off there was a short sudden pull of the tube towards an axial position. This was due to the tendency of the magnet to pull round the axis of the momentary induced currents on *breaking contact* into a position parallel to the magnetic lines of the magnet as before.

8628. Placed tubes of Gases, air and a vacuum, all sealed up hermetically, in Water and Alcohol as Media.

In water.	Vacuum Air Hydrogen Carbonic acid gas Sulphurous acid gas Ether Vapour	All set axially in the water and as far as I could tell all alike.

8629.

In Alcohol.	Vacuum Air Hydrogen Carbonic acid gas Sulphurous acid gas Ether vapour	All set axially in the Alcohol. The hydrogen and the Carbonic acid more feebly than the Sulphurous acid gas—but then the latter was longer in the tube.

8630. Now put the gas tubes into air and Carbonic acid gas.

In air.	Air	sets equatorially.
	Vacuum	Do. perhaps more than air.
	Hydrogen	Do. Do. tube longer.
	Carbonic acid gas	Do. as air.
	Sulphurous acid gas	Do. less perhaps than air.
	Ether Vapour	Do. perhaps more than air.

8631. In Carbonic acid gas.

Carbonic acid gas	All set equatorially.
Vacuum	
Air	
Hydrogen	
Sulphurous acid gas	
Ether vapour	

8632. The constant equatorial set is of course due to the GLASS of the tube.

8633. Compared the same substance with itself in the liquid and gaseous state.

8634. Liquid sulphurous acid in gaseous sulphurous acid pointed well *equatorially*.

8635. Liquid Nitrous acid in gaseous nitrous acid pointed well equatorially.

8636. {Liquid Ether in vapour of Ether pointed well equatorially.
Vapour of Ether in liquid Ether pointed distinctly axially.

8637. Arranged the Woolwich helix and our largest helix end to end without the cores, sending the Electric current in the same direction through both; and hung a horizontal bar of bismuth between them, but could not perceive the least signs of set. The Magnetic forces are here by no means sufficient for the purpose.

8638. *Bismuth and Air cores.* Arranged our helix with a magnetic needle in the distance. Then sent the Electric current through the helix—but whether air alone was in the center of the helix or whether a bar of bismuth was there, made no sensible difference on the needle.

8639. Solutions of Proto sulphate of Iron—and of salts of Nickel and cobalt, being tried, were found to rotate the polarized ray in the *same direction* as the prism of heavy glass.

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8640. The solution of Sul. Iron (8623) which pointed equatorially examined—5 cubic inches gave

7	grains	of per oxide of iron; heated = to
4·9	,,	of Iron
6·3	,,	of prot. oxide
13·3	,,	of dry sulphate of Iron
24·3	,,	of crystals of sulphate of Iron

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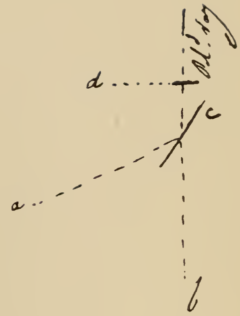

8641. When a polarized ray falls upon a single plate of glass in the right position to reflect it polarized (i.e. placed so as to reflect at the right angle in the plane of polarization), only a small portion of the light is reflected as towards *a*, for by far the largest portion goes on to *b*, passing through the glass.

8642. Still, the plane of polarization is not changed, for the light examined at *a* or *b* by a Nicol's eye piece is [found] in both places to be polarized in the *same plane*, and in the same plane as the original ray. So transmission through the glass *c* has not altered the character.

8643. In this state, a Nicol's eye piece in a certain position put[s] out or renders dark the image seen either from *a* or *b*, the position being the same for both places. If revolved 90°, it renders the image most luminous in either place, the image being then white. If revolved 90° more, it makes the image dark in both places. Thus far the two rays have the same property.

8644. A plate of Sulphate of lime was then placed at *d*, and its position and thickness was such that the Nicol's eye piece at *a*, and *in the position* which before gave maximum light, again gave the maximum light, but now that light was red. If the eye piece without further change was carried to *b*, there also the light was red, but more abundant (as before in respect of quantity) than at *a*.

8645. The eye piece being taken back to *a* and then revolved 90°, gave darkness as before; but now, being taken to *b*, it gave instead of darkness abundance of light, and that *green*, or complementary to the red. At *b*, in fact, there was no dark position, but red and green and the intermediates. At *a*, on the contrary, there was *red and dark*. So that the action of the plate *c* is not the same on the simply polarized ray, which it partly transmits and partly reflects without further alteration, and the ray altered by the sulphate of lime.

8646. Now proceeded to experiment with polarized rays sent in contrary directions along each other's course in different substances*. 1, 2, 3 are three thin plate glass reflectors, transparent, and placed at such angles as to polarize by reflexion rays sent along the line *a*; *b* is a lamp with a small flame, the light of which, polarized in a horizontal plane by reflexion at 1, then passes on along *a* to glass 2, where part is reflected to *c* and part is transmitted to *d*, both these rays being polarized in the same plane horizontal. Hence the polarized light of *b* may be observed either at *c* or at *d*. *e* is an argand lamp, the light of which is polarized by glass 3 so as to be polarized in a horizontal plane and so go on from 3 through 2 to 1, etc., and by lines on the three glasses, 1, 2 and 3, these rays are arranged so as to take exactly the same path in a reverse direction as the polarized rays from the lamp *b*. Hence it was in the line *a* that effects of opposition were expected to be exercised. The weaker ray from *b* was the one which was examd. at *c* and *d* by a Nicol's eye piece as the one to be affected, and the stronger ray from *e* was the one intended to exert such influence as it might possess.

The space between 1 and 2 was 41 inches,
b and 1 „ 44 inches,
e and 3 „ 58 inches,
2 and 3 „ 7 inches.

8647. Whether the light of *e* was allowed to flow forwd. to 3 or was shut off by a screen, still not the slightest difference appeared in the ray from *b*, when examined by the Nicol's eye piece at *c* or *d*. So there was no sign of influence in this case.

8648. Put up the plate of Sulphate of lime (8644). The effects were exactly the same whether the counter light of the lamp *e* was on or off.

* [8646]

8649*. Turned the plate 3 on the line *a* as an axis, and placed the lamp *e* below, so as to polarize its ray by glass 3 in a vertical plane, still passing the polarized ray along the line *a*. Examined the ray from *b* as before at *c* and *d*, both when light of *e* on or not on, in the counter direction. In neither case was there any change.

8650. Now placed a bar of *heavy glass* 7 inches long in the line *a*, so that both rays should pass in opposite directions through it; and examined the effects on the light of *b*, when the light of *e* was polarized either in a vertical or a horizontal plane, but could see no change. No effect of influence or interference.

8651. Filled a tube 30 inches long with *distilled water* (   ) and placed it in the line *a* (8646). Examd. the effect of light of *e* in both planes of polarization—but could perceive nothing.

8652. In reference to the effect of the sulphate of lime plate (8644)—the two plates 2 and 3 were removed and the polarized ray examd. at *d*. It was the same after the removal of the plates as before, both in position and colour, except that the red was rather more distinct. Then on putting up one plate, as 2, a portion of red was taken out and reflected, and putting up a second plate, as 3, a portion more of the red was reflected; so that probably all the red might have been taken out by a sufficient number of plates—and so on.

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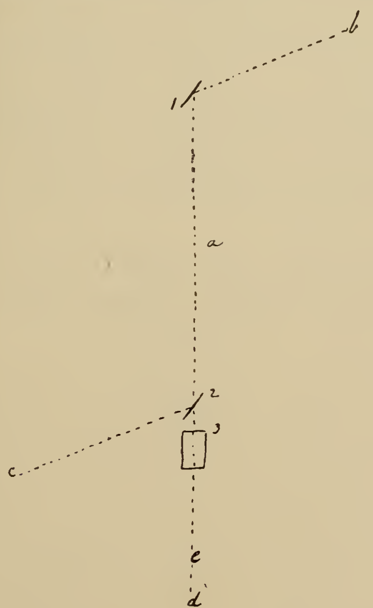
8653. *Oil of Turpentine* in a tube 28 inches in length was put into the line *a* of the counter rays (8646). From the rotating effect of the fluid, the ray examd. by the eye at *c* by a Nicol's eye piece was alternately green and dark at every successive quadrant

* [8649]



of a revolution. When the eye was at *d*, the transmitted part of the ray was red and green at the successive quadrants of revolution. When the Nicol eye piece gave green at *c*, it gave green also at *d*. When it gave dark at *c*, it gave red at *d*. When the plate of glass 2 reflected the rotated ray in a horizontal plane, it reflected the green part and not the red. When it was turned round the ray 90°, so as to reflect in a vertical plane, then it reflected only red light and not the green.

8654. Whether the counter ray of the Argand lamp placed at 4 was on or off, it made not the least difference in the effect. Neither if the counter ray were polarized in a vertical plane did it make any difference.

8655. *Proto sulphate of Iron*—nearly a saturated solution rendered slightly acid was put in a tube into the course of the opposed rays at *a*. The tube was 30 inches long. But there was no influence on the observed ray, whether examined from *c* or *d*, by the counter ray, whether in a vertical or horizontal plane of polarization.

8656. So no affection is observed of a ray passing through air, water, heavy glass, camphine, and sol. proto sulphate of iron by the passage of a strong counter ray—both being obtained from the flames of oil lamps and operating in short distances.

8657. Arranged the Woolwich helix (7538) in the line *a* and prepared a battery of ten pair of Grove's plates to throw it into action at pleasure. Then observed the effect on the rays passing through the *air* of the helix. But whether the current was on or off made not the least difference in any of the phenomena. There was no counter effect of one ray on the other.

8658. *Oil of Turpentine*—placed the tube of camphine (8653) in the helix and repeated all the observations whilst the current was passing through the coil, intermitting occasionally—but there was not the slightest effect.

8659. *Heavy Glass*. The prism 7 inches in length was put into the helix and the effects sought for in the black cross ( ), but not the slightest trace of influence of the two rays could be perceived. The rotating force of the current in the helix was evident enough.

8660. *Sol. Proto sul. Iron.* This solution in the tube (8651) was put into the helix, but it made no change in the effects (8655).

8661. *Water* in the 30 inch tube was put into the helix. It was easy to see the effect of the rotation by the current when observed at *d*, but not at *c* (8646). Still the current developed no effect of influence between the two counter rays.

8662. So Air, Water, heavy glass, camphine and sol. Sul. Iron, when in a powerful helix, still shew no trace of any influence exerted by two rays passing in opposite directions.

8663*. The short 2 inch prism of heavy glass was placed in position at the poles of the great electro magnet—Argand's lamps placed at *b* and *c*, and observations made at *d* by the Nicol eye piece. The piece of glass *a* acted well under the influence of the Electro magnet, but whether the counter rays were polarized in a horizontal or a vertical plane, still no effect of interference could be observed.

8664. Think there ought to be a difference still in the two directions of a ray—and must experiment with light of the Sun or of the stars—7808–21, 7854.

8665. Will the distance of the Sources of light make any difference?

8666. *Cobalt.* Dr Miller has lent me a piece of Mr Askin's cobalt and it appears pure and good. I took a very small piece *a*, and suspending it by a platina wire, tried to make it so hot as to loose its principal magnetism, using a spirit lamp between the poles of the large battery[1], but I could not make it hot enough. It was at every such heat magnetic to a small bar.

8667. Have received some specimens from Mr Mason and tried them as to their Magnetic or Diamagnetic state.

8668. *Pure Gold.* Diamagnetic and repelled.

8669. *Biniodide of Mercury.* Diamagnetic.

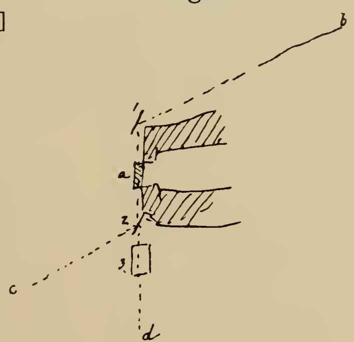
8670. *Nitrate of Tellurium.* Diamagnetic.

8671. *Oxide of Uranium*, black, and *Ammonio carbonate of Uranium* were both slightly magnetic.

8672. *Sol. Proto sul. Iron* rotates the ray in the same direction as the little prism of heavy glass.

[1] ? magnet.

* [8663]

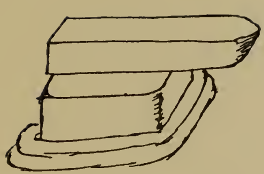


8673. Experiment for Herschell.

A cylinder of glass, 2 inches long and polished at the ends, was cut hexagonal at the ends and fitted with keys, so that torsion could be given to it. A polarized ray was passed along it and examined by a Nicol's eye piece, the latter being arranged to give darkness as to the Polarized beam. Then torsion was impressed on the glass. Immediately the beam was partially depolarized and the image became luminous, and the more luminous as the torsion was greater. But there was no rotation of the ray, for the eye piece was still in the darkest position, and whether moved to the right or the left the light increased accordingly.

8674. A prism of rock crystal cut for the same purpose was full of macles, so as to depolarize almost entirely, and was useless for the same experiment.

26 FEBY. 1846.



8675. Worked with the large horse shoe Electro magnet fixed in an upright position and excited by 10 pair of Grove's plates. Have had two pieces of iron (soft) made, flat at one end and somewhat pointed at the other, with flat under faces, and these being placed on the ends of the chief poles, can be set at any distance and give magnetic curves of most intense power in the axis of the magnetic field.

8676. Prepared our Electrical machine to give positive or negative Electricity. Prepared an insulating handle—also a wire from the machine and a series of fine and coarse points—rounded terminations—terminations of wood and balls—to give either Pos. or Neg. brushes, stars or sparks.

8677. Then sent these along and across the Magnetic field, either to the one or other magnetic pole or to receiving wires between the pole—having at the same time the Magnetic poles very near and the power intense—but could not perceive the least difference, either in the readiness of discharge or character of it, under any of the many varying circumstances.

8678. One of the Electrical terminations was a pointed steel cylinder which was held mid way between the magnetic poles, terminated as above or by cones of soft iron, bright and sharp pointed, so that the electrical point was also magnetic; but I could find no difference in effect whether the magnetism was on or off.

8679. All these trials were in air. Now placed a vessel of Camphine between the magnetic poles and passed small sparks from a pointed wire to a ball in it. Here, of course, there is either spark discharge or those rapid currents equivalent to wind in air, and which at the surface are seen to be very strong and tumultuous. But neither the sparks nor the currents were in any way affected by the superinduction of the magnetism.

8680. A voltaic discharge from 5 pr. of Grove's plates, having intensity enough to leap across a small interval, was passed across the magnetic field, and occasionally on to the magnetic poles, but it was in no way affected.

8681. *References.*

1841. Haldat on Generality of Magnetism as in Iron. Comptes Rendus, xii, p. 950.

Watt on Mag. influence of Sun's rays—Edin. Phil. Journl. 1827, April, p. 170.

Baumgartner on Morrichini and Somerville's Exp. Bull. Universelle, 1827, viii, 244.

Rotation of Mag. Needle by Electy. Ragona[1], Arch. d'l Electy., v, p. 352. Is this connected with true principles or an accident? Repeat it.

9 MARCH 1846.

8682. Christie's experiment of influence of solar rays in retarding the oscillations of a magnet are in the Philosophical Transactions for 1826, p. 219, and 1828, p. 379.

8683. A square prism of heavy Glass polished at the ends, $2\frac{1}{2}$ inches long and 0·8 of an inch in the side, has had a helix made on it of silked copper wire consisting of   feet in length. This glass was placed between the poles of the great horseshoe Electro magnet (7874)—the E. current set on and then the ends of the helix round the glass connected with a very delicate Galvanometer. Not the slightest effect of disturbance at the Galvanometer was produced so long as the electric current was continued unchanged. Hence this arrangement will do for the further experiments.

[1] Probably D. Ragona-Scinà.

8684*. The prism above was placed in position at the poles of the Electro-magnet (great horseshoe 7874), which in due time was excited by 10 pair of Grove's plates. A lime light apparatus was so arranged that rays could be collected by a condenser lens, then sent through two smaller lens and a Nichol's eye piece, so as to give a strong polarized beam which would pass through the heavy glass parallel to the lines of magnetic force. A screen was provided at the near end of the prism so as to cut off the light from the glass at pleasure, and a reflector of silvered glass was ready to apply at the other end, to send the ray back through the glass on itself when desired.

8685. A very delicate galvanometer was provided, being the one I had from Matteucci. It was placed at the other side of the room—all Iron was removed from its neighbourhood, and it was not affected by the mere magnetism of the magnet.

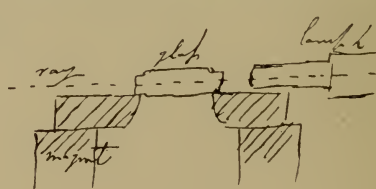
8686. First, the battery current was sent through the magnet and continued by a *screw contact*. *Next*, the helix on the glass was connected with the fine Galvanometer (8685)—there was no effect on the latter by the continuance of the magnet in action (8683). Then the lime light was set on and the polarized ray sent through the heavy glass. But whether the light passed through—or was stopped—or was intermitted—or was reflected back and then stopped or intermitted—or in any other way that I could vary the circumstances—*no effect* was produced at the galvanometer.

8687. The Galvanometer was then first separated and afterds. the electric current shut off from the magnet and the light stopped.

8688. *Oil of turpentine* in the large glass tube with the helix wire around it (8653) was placed in its tube in the Woolwich helix (7538) and both arranged before the lamp, that the polarized ray from the lime light would pass through the column of liquid. The turpentine tube helix was connected with the galvanometer (the Woolwich helix being as yet out of action) and then the lime light passed through. But whether the light was on or off or intermitted or reflected—no effect at the galvanometer occurred, i.e. the natural rotation of the Camphine caused no effect.

8689. The Galvanometer was now detached—the battery current sent through the Woolwich helix so as to cause its induction on

* [8684]



the Camphine, and the Galvanometer wire reattached to the camphine helix. All was steady at the Galvanometer. The polarized light was then passed through, intermitted for longer or shorter periods, reflected, etc. etc., but there was no effect at the Galvanometer.

8690. *Water* was submitted to the same set of experiments, being in the long large tube before described (8651). It was tried both without and with the influence of the Woolwich helix on it, but there was no effect.

8691. A nearly saturated solution of *proto sulphate of Iron*, preserved clear by the addition of a little sulphuric acid, was put into the same tube as that used with the water (8690), and submitted to the same experiments—but with only negative results. No effect was produced at the Galvanometer.

8692. So mere diamagnetics, as heavy glass and water—or one having a natural rotation, as camphine—or a magnetic, as sulphate of iron solution—all gave the same negative result under this attempt to obtain a reaction of the affected light back upon a surrounding helix.

8693. Must try these experiments with sun light condensed and polarized—are very manageable (7808–21, 7912–22, 8683–706).

8694. *References*. *Watt* on Mag. influence of Sun's rays. Edin. Phil. Jour., 1827, April, p. 170.

Baumgaertner on Morrichini and Somerville expt. Bull. Univ., 1827, viii, 244.

JUNE 29, 1846. (7808–21, 912–22, 8683–93).

8695. Worked up in Anderson's room and so arranged that a silvered glass, 18 inches square, should reflect a sun's ray (11 to 1 o'clk.) into the room. The beam was first converged by a four inch lens, then diverged by 2 concave lens near the focus so as to give a compressed dense beam of nearly parallel rays. A bar of my heavy glass surrounded by a helix of covered wire (the bar 0·7 in. width and 2·2 long) and polished at the ends was arranged, the helix being connected with my very delicate galvanometer (bought of Matteucci).

8696. The compressed beam (8695), being passed through the heavy glass, produced no effect at the galvanometer. Nor did any motion of the glass and helix in the ray produce any effect.

8697. The compressed beam was passed through a Nichol's polarizer and then through the glass—but there was no galvanometer effect.

8698. A bow string was put round the neck of the polarizer, and it was rotated on its axis whilst the ray passed through it and through the glass, but still there was no effect at the galvanometer.

8699. The heavy glass was placed between the poles of my most powerful electro magnet (the East India link magnet), but still, with any or all the above modifications and with the magnetism on and off as on the 12 March (8686), there was no galvanometer effect.

8700. Collected the sun's rays by a silvered glass reflector, concave and 12 inches in diameter—making the ray approximate to parallel by concave lenses near the focus. Still this beam treated as above produced no effects with the heavy glass. There was great heat and I had to operate with care and quickness, but if there had been any effect I must have obtained traces of it.

8701. I several times found certain small effects, but was always able to trace them to known laws and actions of the magnet.

8702. Moved a helix with heavy glass fixed in it in the sun's rays collected both by lens and reflector, but could obtain no effects on the galvanometer.

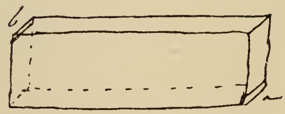
8703. Moved the same helix with a magnet *fixed* in it in the sun's rays in all directions, but obtained no results at the galvanometer.

8704. Made a small helix of clean platina wire; placed it in the Galvanometer circuit, and then sent the focus of the reflector through it and about it in various directions, and moved it in the beams; but with no results at the Galvanometer.

8705. Connected one end of the Galvanometer wire with the bell wire as a good discharger, and the other end with a ball of spongy platina. Held the latter in the focus of the reflector in all positions. No effects.

8706. For the ball of platina, put a piece of retort carbon and also the platina helix (8704)—no results.

24 JULY 1846.



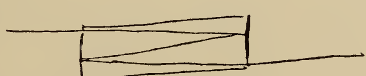
8707. I have prepared two square prisms of heavy glass for repeated reflexion of light within from end to end. One is about inches long and of an inch square. The two ends have been polished and silvered and then a portion of the silvering about of an inch broad removed nearest to the edges *a* and *b*. In this case it is evident that a ray of light may pass through directly from corner to corner*—or be reflected twice in its course and then pass out to the eye†—or it may be reflected four times within the glass and then pass out‡—in the piece of glass prepared I can make [ ] or 6 successive images of the light appear.

8708. It is manifested that where the light has been reflected twice, the ray has passed three times through the length of the glass—and when the reflexions are n, the ray has passed $n+1$ times through the glass. Still, as the rotation of the ray in the Magneto light phenomena depends upon the direction of the Mag. force and not on the direction of the ray, it will, every time that it passes through, have an additional amount of rotation impressed upon it in the same direction, which ever way it goes; and so if it pass 5 times, it will have 5 fold the rotation that it would have if passed once only.

8709. Found this to be the case. It is easy, by moving the prism more or less obliquely, to select the image due to one passage—or after one double reflexion, or two or three double reflexions—the first length ray being of course unit and the second and third

*[8707]

† [8707]



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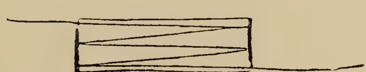


observations being on ray[s] 3 and 5 times as long. Found as expected that when the great magnet was arranged, etc., if the rotation of the direct ray was 12°, the rotation of the second image was 36°, and that of the third image 60°.

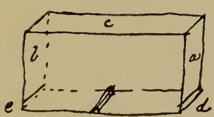
8710. Hence many good results. It shew[s] that which ever way the ray is passing, its rotation is determined by the direction of the magnetic force.

8711. It makes the distinction between this rotation and that of quartz, turpentine, etc. more striking, for in the latter, all the reflexions would neutralize each other and only an effect equivalent to one transit be obtained.

8712. It enables a short piece of glass, crystal or other substance to be used and also the poles of the Electromagnet to be nearer, and therefore the lines of magnetic force stronger.

8713. Or it enables the effect to be seen in a shorter helix and with a feebler apparatus than before.

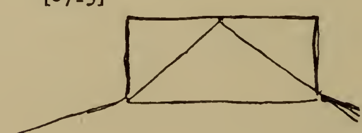
8714. It shews that the effect on the ray is proportionate to the length of glass through which the ray passes, by allowing a direct comparison of lengths, as 1, 3, 5, 7, 9, etc., without change of the apparatus or its parts.



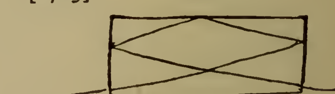
8715. A second piece of glass of an inch square and only inches long was prepared and silvered in another manner. The faces *a*, *b*, *c* were silvered and then a part of the silvering removed at *d* and *e*, where only a direct ray could pass through, but to prevent that a notch was cut in the middle of the face *d e*. In this case, light could pass once through the glass by one reflexion at the back*, or it could traverse the glass three times, as in the second figure†, by virtue of three reflexions—or five times by virtue of five reflexions‡. In this way, the eye applied at [illegible] as many as 6 and 7 images in succession, using the bright sky as the source of light, and these correspd. to 13 and 15 journeys of the ray along the bar. In this case the effect was precisely the same as with the former, and I could find no interference of the crossing rays one with the other, or any diminution of the rotating effect.

8716. The other is the best shape, as requiring least displacement of the bar between the 2 Nichol's eye pieces. If its extremities be not quite parallel—the effect is to throw the successive images which are obtained by increased reflexions more close when the

* [8715]



† [8715]



‡ [8715]

eye is applied to one end and more open when applied to the other; and this offers an advantage when it is desired to observe one image by itself, or when we wish to compare its rotation with that of another.

8717. Looked for the extinction of crossing light, but there was none evident.

8718. Can use even ordinary magnets now.

28 JULY 1846.

8719. Becquerel speaks often of substances standing across the magnetic lines of force, and so does his son and Haldat, for which see

Becquerel,	Comptes Rendus, xxii, p.	146, 26 Jany. 1846
Haldat,	Do.	267, 9 Feby. 1846
Becquerel, E.	Do.	959, 8 June 1846,

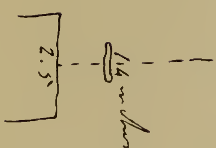
and E. Becquerel's remarks are evidently to the effect that there is but one kind of magnetism, etc. etc. So I have made a few experiments to-day in the matter—not having however any data as to measures in Becquerel's papers.

8720. A small glass tube 0·25 of inch in diameter and 1·4 inches long, was filled with the per oxide of iron called rouge, very uniform in its character. The Woolwich cylinder magnet was excited at pleasure by 5 pair of Grove's plate[s]—the magnet being horizontal and either a cone, or a square bar, or flat bar, or wedge of iron, etc. etc. being placed at the end to give different forms of termination to the pole.

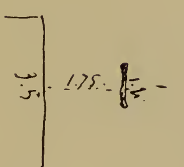
8721. The end of the magnet was made a cone. The tube of peroxide, suspended horizontally by cocoon silk, was brought up towards the pole, always keeping the center of suspension and revolution in the axis (prolonged) of the cone, but at different distances. When at distances at which the magnet action on it was sensible—it was *always* attracted. If it happened to be perpendicular to the line of magnetic force, it remained there, but if inclined either side, then the nearer end approached the pole and the tube either became axial or stopped short because its end touched the Magt. pole. At any nearer distance the same thing happened, and there was no tendency to set itself across the Mag. curves.

8722. The oxide can receive a small mag. charge and retain it and this, if it occurs, must be noticed and accounted for.

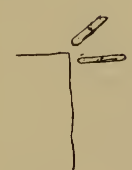
8723. Now the end of the magnetic pole was changed, a square bar of iron being used, which presented a terminal face about 1·75 inches square, facing the tube of per oxide. When at a distance of 0·3 of an inch from the face of the pole, or at any less distance, the tube of oxide could be retained in a position across the magnetic line of force, or rather across the axis of the pole *a b*, the point of suspension being in it. At greater distances, this was an unstable position and the bar tended to set obliquely, as * or † indifferently, and this increased as the distance increased as long as the action of the magnet was sensible on the tube of per oxide. The cause we shall see hereafter—but the center of gravity of the tube was always *attracted.*



8724. The end of the pole was now a short bar of iron presenting towards the oxide a vertical face about 1 inch high and 2·5 inches horizontally (the important direction). The same effects as before occurred, but the position of the tube of oxide across the direction of the axial line could be retained until the distance was an inch or more.



8725. Another termination was given to the pole, and now its vertical face was 1·75 inches high and 3·5 inches horizontal. Here, when the center of suspension was carefully adjusted, the tube of oxide was parallel to the face of the pole when 1·75 inches from it, and still more forcibly so at any smaller distance. If disturbed from this position, it returned to it, and very strikingly so when at distances less than an inch.



8726. But now must point out that in this and the last case, illustration and explanatory effects took place when the point of suspension was moved to the one side or the other of the axial line, or the magnetic pole moved so as to produce the same relative displacement. Thus, if the point of suspension were carried on one side, as in the figure, then the end on that side would approach the face of the pole and take up a permanent inclined position—and that the more inclined as the point of suspension were carried further out. Until at last the bar or tube of oxide would be perpendicular to the face or even inclined outwards—and the same would happen exactly in the same order on the

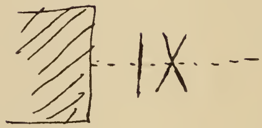
* [8723]  † [8723] 

other side. The accompanying lines give something like the different positions of the tube of oxide under the varying circumstances.

8727. In all cases the center of Gravity is *attracted.*

8728. Prepared another tube of the Per oxide of the same diameter, but 2·5 inches long, and suspended it in the same way before that face of the Magnet which was 3·5 inches in its horizontal dimension. Even this long tube could be placed with its center of suspension in the axial line as far as 1·3 inches from the face, and yet its permanent position of rest was parallel to the face or across the axial line. When the distance was more than this, the position became unstable and the stable position was inclined in one direction or the other.

8729. When within the 1·3 inches distance, then carrying the point of suspension either to one side or the other of the axial line produced just the same effects as those described above. When about half an inch from the face, it was easy to obtain all these and the other required effects.

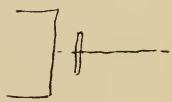


8730. *Paper*—a little roll of paper, which I knew to be ferrugenous and therefore magnetic, about $1\frac{1}{2}$ inches long and 0·4 of an inch in diameter—presented also all these effects very well.

8731. When the paper roll was presented to the conical or the wedge termination, then these effects did not occur, but either one or the other end of the roll was permanently attracted. The center of Gravity was *always* attracted.

8732. Another roll of paper did not set so well, i.e. did not give the transverse positions so accurately. It is easy to see that any irregularity in the iron of the paper—or in the state or packing of the per oxide, or in the magnetism communicated to it, may easily give disturbed results, but these are no objections to such as are regular.

8733. I then tried two tubes about 0·25 of inch in diameter and $1\frac{1}{4}$ inches long, filled respectively with solutions of muriate of nickel and sulphate of iron (proto). These did not produce the same effects as the former. It is true that they could be placed parallel to the face of the pole, but this was a place of instable equilibrium; and if the pole had power to act at all on the solution, and one end of the tube was nearer than the other to the face,



then that end approached the pole face and remained against it, just as Iron would do, but in a very much feebler degree.

8734. I incline to think that the disaggregated state of the peroxide of iron has a strong influence on the effect—for on trying several filaments of haematite, they all acted as iron and the solution acted.

8735. *Iron.* Fine iron wire being used, it always pointed end on to the face of the pole—however short the piece might be. I loaded the wire with lead, so as to prevent it from going up to the face of the pole, but still it pointed towards it. This is doubtless due to the perfection of the Magneto inductive action through it and the high reciprocal state which it acquires throughout. Still, even it manifests its tendency to go to the extremes of the face of the pole—an effect well known in iron filings, nails, etc. etc. (8755).

8736. If a fine long piece of iron wire be placed across the conical or wedge shaped Magnetic pole, then it stands *across* the polar axis, and on the tip of the cone looks very strange; but this is nothing more than the opening out of two rods there or of bunches of filings, or other such forms, and is due to the divergent condition of the lines of magnetic force carried onwd. in this case by the two parts of the wire itself (8757).

8737. The effects with the per oxide of Iron are simple results of the greater intensity of the magnetic force at the edges of the square or flat pole, than in the middle of the flat face. The consequences of this difference have been shewn already in the Researches, 2298, 2299, 2384.

8738. Hence E. Becquerel's objections or views have no force. In fact, where the per oxide of Iron will stand transversely, there diamagnetic matter will often stand axially—still shewing the contrast of the two magnetic conditions; and there is the never failing fundamental result, that where one is attracted the other is repelled.

8739. Remember all my papers were written and received at the R.S. last year, and two of them read there last year.

8740. Have procured some steel buttons and arranged two so as to make one the face of each Magnetic pole, and can arrange them so as to throw the ray to and fro, so that it shall pass 1, 3, 5 or 7 times across the magnetic field and yet allow of either air—or a crystal or other transparent body, being there (8707).

8741. *Air.* The interval was made 1·25 inches—and the direct or first and second reflected images could be very well seen—but with none of these could I obtain any effect (7878, 7864) upon the air by the large magnet (7874) excited by ten pair of Grove's plates.

8742. *Heavy Glass.* Effects as before and very good.

8743. *Quartz cube* (7882). In all three directions, but obtained no sensible effect.

8744. *Iceland spar cube* (7883). In all three directions, but with no effect.

8745. *Air again.* No effect.

8746. In all these cases then, it would appear that, as far as my means go, the Magnetic force is powerless over the ray, i.e. in air and in doubly refracting crystals.

8747. Intend to silver the cube of quartz and calc. spar so as to get a more decided result.

8748. Experimented as to power of diamagnetics to generate currents when introduced into a helix.

8749. First a fine helix (8683) was placed between the poles of the great magnet—the magnet made by completion of circuit—and last the helix connected with my fine Galvanometer. There was no motion, but any movement of the helix *now* produced a current and affected the galvanometer.

8750. The helix being at rest—a piece of iron, as a nail, was introduced into the helix, and there was then a sensible and even a strong current produced at the Galvanometer.

8751. A tube full of solution of sulphate of Iron (saturated) was then introduced, but this produced no decided current—there was motion of the slightest kind—but I think none the mere result of introducing the solution.

8752. A good thick square bar of *bismuth* 0·75 of inch square was introduced, but with no effect.

8753. *Phosphorus* and *heavy glass* were also introduced in the same manner, but with no effect.

8754. Hence I cannot find in diamagnetics any power of producing a current in a helix, like in kind but contrary in direction to that which a piece of iron can produce under the same circumstances. They are probably too weak in power to be sensible with this apparatus.

4 AUG. 1846.

8755. Thinking it possible that iron might shew the phenomena of per oxide of iron, if presented to a large face (8723, etc.) of a very weak magnet, I suspended a piece of fine iron wire half an inch long by a copper loop and cocoon silk—made it red hot to remove all ordinary magnetism—then brought the large iron faces (8725) near it, and by the application of a weak bar magnet to the other end of the pieces of iron, made them weak magnets. But the soft iron did not act as the haematite. The magnetism was so weak that the suspended wire pointed but did not adhere to the magnet. Either end would point *indifferently*, shewing that the wire was not permanently magnetic—but then it pointed end on whatever part of the face it was opposite to and at whatever distance, provided it was affected at all. In no place did it set itself parallel to the face as the tube of per oxide of Iron did.

8756. Another piece $\frac{1}{4}$ of an inch long was tried with the same results.

8757. Hence with the per oxide, much appears to be due to the disaggregation, for the Iron strong or weak as respects the opposed magnet (8735, 8755), and solution of sulphate of iron, which is weak as a magnet and weaker than the peroxide, act alike and both unlike the peroxide.

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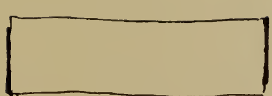
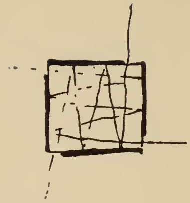
Reflections applied to crystals, etc.



8758. A crystal of quartz 2·3 inches in length had the ends polished and silvered so as to allow of repeated reflexion. It was then adjusted between the moveable mag. poles of the great horseshoe magnet (   ) excited at will by 10 pair of Grove's plates—but no effect of the magnetism could be observed on a polarized ray passed through the crystal 1, 3 or even 5 times; i.e. when the first, second or third image was observed (8707).

8759. A cube of rock crystal (   ) 0·7 of an inch in the side and cut with two of its faces perpendicular to the axis of the crystal, was silvered on four sides so as to observe by reflexion through the two faces above mentioned and other two faces. It was placed between the magnetic poles approximated as closely as they might be, i.e. to   of an inch—the position of the crystal being such that the polarized ray was not affected by it or very slightly. There was then no sensible effect of the magnetism on the ray, with either the first, second, third or fourth image. In the last case, the light had traversed the cube of crystal seven times.



8760. The crystal was now turned a quarter round and the observations now made on the other two faces. In this position it affected the polarized ray naturally, depolarizing it to a certain amount and in a certain manner. But not the slightest additional effect could be observed as due to the magnetic action, by observation of the first, second, third and fourth image, for the amount of light was not varied nor the tints in the least degree affected.

8761. A smaller cube of quartz (   ) 0·7 of an inch in the side was observed in the same manner and yielded the same negative results.

Calcareous or Iceland spar.

8762. A cube of this substance was cut with two of its faces perpendicular to the axis of the crystal, and silvered like the former cubes; it was 0·82 of an inch in the sides. Being placed so that it did not affect the polarized ray, and then subjected to magnetic action, the first, second, third, fourth and fifth image was observed, but there was no signs of any effect. The reflecting faces were very well silvered.

8763. The cube was now placed so that it might be observed in a direction perpendicular to that just described, and in which it depolarized the light, producing colours, etc.; but not the slightest additional effect due to magnetism could be observed. The reflecting surfaces in this case were not so good as the former.

8764. In the cases now described, the surfaces were silvered as a mirror is silvered, but I have had small square silvered flat reflectors made, that can be attached by a little Canada balsam to the surface of the crystal, or of glass. Perhaps in such cases it may be necessary to take note of the effect of the Canada balsam.

8765. Two silvered reflectors were attached to the opposed ends of the magnetic poles and the light reflected to and fro through the air between the poles (about 1·2 inches apart, and also 2 inches apart). The observation was made up to the fifth image (equivalent to 9 transits of the ray between the poles), but no sensible effect of the magnetism was detected in it. So no action as yet in Air.

Expts. on pointing of short bars of bodies at large end of flat magnet (8719).

8766. Arranged the Woolwich cylinder magnet as before and used as the terminal that piece of soft iron which presented a square flat face, 2 inches in horizontal dimension and 1 inch or nearly so in vertical direction.

8767. Per oxide of Iron—in a glass tube about 1·1 inch long, pointed as before described (8726), and as Becquerel has said. But sometimes, instead of standing parallel to the face of the magnet, it seemed to have a tendency to approach by one end.

8768. A piece of *hæmatite*, separated from a fibrous mass by a porcellain tube. This did not act as the per oxide, i.e. it did not tend to place itself parallel to the face of the pole, but the end which was nearest continued to approach. As it could take a magnetic state and in some degree retain it, the end which had last touched the pole had a greater tendency when removed to go to it again than the other end. Wherever it was placed, one end tended to and went *quite up* to the pole. The centre of gravity was attracted.

8769. The hæmatite was then powdered finely and put into a glass tube, but still it acted as before; the disaggregation had not been able to bring it to the peroxide of Iron state. Either one end or the other went quite up to the pole and the mass was attracted.

8770. *Pure per oxide of manganese.*

Put into a tube, acted in certain situations as the per oxide of Iron of Becquerel, but the situations required selection in respect of the face of the pole, and in the greater number of positions one end went quite up to the pole and that usually the nearest.

8771. Thus if the tube were placed in either of the positions

represented and then the magnet made active, the end *a* would go outward and the end *b* would go to the pole. But if the tube were suspended before the middle of the face, then the nearest end went up to the pole and touched it. The tube would not take and keep a point or place of rest parallel to the end of the pole.

8772. I pressed the per oxide of manganese together in a wrapper of platina foil by means of a vice, but it produced just the same effect as in the tube—even with a large pole face of four inches horizontal extent.

8773. *Platinum.*

A short piece of thick platinum wire points end on as iron wire would do, but not so strongly of course—therefore it could not keep a position parallel to the face of the pole or inclined and not touching it, and is not like the peroxide of Iron.

8774. *Spongy platina* in a glass tube pressed down shewed the phenomena above of per oxide of manganese (8771), which is a sort of half way step between those of Iron and per oxide of Iron—but not to the same extent as the manganese oxide. Still, one end of the mass always went up to the bar and the center of gravity of the mass was always attracted.

8775. *Iron glass.* A short piece of bottle glass tube pointed end on any where before the face of the magnet and touching it at one end. So also did the same glass if pulverized and put into a small glass tube. It could not keep a position parallel or inclined to the face of the pole and not touching it. No such place of rest.

8776. *Solution of Sulphate of Iron proto*—a small tube filled with saturated solution points with one end on and touching, and therefore as platina, and not as peroxide of Iron.

8777. One volume of this solution was then diluted with five volumes of water and put into a tube—it still pointed in the same manner, though more feebly. Still it could not hold a position parallel to the face or inclined to and not touching it as per oxide of Iron—even though a pole face of 4 inches in horizontal extent was opposite to it.

8778. Yet this solution had nothing like the magnetic force of the tube of peroxide of iron, as could be easily seen by the superior attraction of the center of gravity of the oxide tube and the greater amplitude of the vibrations which it made as a pendulum.

8779. *Sol. of Cobalt Muriate.*

Same as solution of Iron. The nearest end continues to advance to the magnet face and in no case does it remain parallel to it, or return towds. a parallel position.

8780. *Muriate of Nickel sol.*

Same as sol. of Iron or cobalt.

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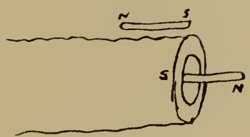
8781. As to inductive force on *inside* and *outside* of a hollow helix as manifested in its action on soft iron (8856, etc.).

8782. A copper cylindrical hollow helix with a current of 10 pr. Grove's plates was examined by short steel needles and short pieces of soft iron wire, held in various positions inside and outside; on the inside the induction was strong and powerful, but on the outside scarcely sensible. Still it was evident there and consistent in direction with the inside. Thus, if the inner piece became N S as marked, the outer piece also became N S as marked, but very feebly.



8783. When a soft iron core 4 inches long and 0·25 in diameter, which occupied the whole of the interior, was used, the same results were obtained. Placed at first outside, it indicated feeble magnetic power, almost insensible—placed inside, it became very powerful. If placed outside again, the outside influence was very evident in being able to lower the force gained on the inside but was not able to reverse the condition.

8784. Marianini's results respecting the two simultaneous conditions were in some degree manifest here.

8785. An iron tube put outside the copper helix gave the same result. It manifested a little outside action on the test needle, but would not take up any sensible portion of Iron filings. The small rod *in* the helix took up an enormous bunch.

8786. An iron tube had a copper coil helix containing 120 inches of wire placed in the *inside*, and another copper coil of only 27 inches placed on the *outside*. The two were connected together so that one current passed through both, and either could be removed from the iron or even reversed at pleasure without disturbing the electric current. The outside one made the iron very magnetic, the inside one not sensibly so, or at least in the smallest degree. Whether the inside coil were there in the same or in the *reverse* direction, or were taken away entirely, made no sensible change in the effect of the outer (8821).

8787. When the wire in the outer coil was reduced even to 6 inches in length, it appeared to be equally independant of the inner one as respects the iron.

8788. Next proceeded to examine whether an iron tube over a coil or helix would, when made a magnet, induce a current as an iron core *in* the helix would do, expecting from the above non action that it would not. Placed an iron tube over a good copper wire helix containing perhaps 300 or 400 feet of wire, and placed my delicate galvanometer in connexion with the helix. Then brought up our bar magnets to the end of the tube—but there was no trace of a current either at contact or breaking magnetic contact.

8789. If a little piece only of iron inside the coil, a great effect, but none any way outside. This quite consistent with the former action.

8790. Now proceeded to examine the condition of a helix made of *iron* wire, having for that purpose *solid* cylindrical helices, *hollow* cylindrical helices, and *flat* helices.

8791. A solid copper helix containing 20 feet of wire $\frac{1}{24}$ inch in diameter, affected the needle at a distance to a certain extent and took up a small bunch of iron filings. It conducted a good current and became hot.

8792. A solid iron wire helix of 38 feet at $\frac{1}{22}$ inch in diameter did not seem to affect the needle more than the copper wire helix, but it took up a much larger bunch of iron filings. The helix did not heat, and probably carried a much smaller current of electricity than the copper helix (8819).

8793. A solid iron helix of 97 feet of wire $\frac{1}{30}$, affected the mag. needle still more strongly and took up twice or thrice as much filing as the copper helix.

8794. I think the inner part of the iron helices acts as an iron core to the outer coils, but to make the comparison of iron and copper fair, two *equal* coils should be in the circuit at once and then compared.

8795. When the iron coils were freed from the voltaic battery, they remained magnetic as cores would have done, and this final magnetism can be changed in direction by changing the direction of the current. So it is evident that an iron helix of more than one layer of coils acts as a core to itself in some degree.
8796. The magnetism retained is small, for a very little brush of iron filings is retained by the residual magnetism. Still, the core action may be considerable (by comparison) during the time the current continues.
8797. A solid iron helix was put *as a core* into a copper helix and the electric current sent through the latter—the iron helix made a very bad core, taking up scarcely any filings. A thin continuous tube of iron being put in its place made an excellent core. The imperfection of the iron helix core was doubtless due to the direction of its fibres and the want of continuity in the direction of the axis of the inducing core of copper. Still, the iron helix did act as a core and acquired a permanent degree of magnetism, which could be changed in its direction by inverting the iron helix in the copper helix. The magnetism was also the *same* in direction as with a solid or continuous iron core (8819).
8798. A hollow *iron* helix was put over the copper helix, to represent the external iron tube before spoken of (8785); it did not appear to acquire any signs of magnetism.
8799. Flat iron helices lift iron filings well and I think more than equal helices of copper wire with the same current. The inner spirals of iron appear to act as cores to the outer.
8800. Now worked to see if there might be any peculiar inductive action between iron and copper coils. A copper helix was connected with the voltaic battery, and an iron helix connected with my galvanometer; the iron helix was put *into* the copper helix. When the battery contact was made, there was an induced current, and when it was broken there was also a current, and these currents were in the same direction as if the inner helix had been of copper.
8801. There was *no* permanent current induced in the iron helix.
8802. If the iron helix were moved in and out, a current was induced during the motion—but only as if the iron had been copper—no otherwise.

8803. Spun the iron helix a little whilst in the copper helix (taking all care of other actions), but there was no effect at the Galvanometer—apart from the first and last induction, or the moving in and out.

8804. Then placed an iron helix *outside* a copper helix, but this cause[d] no alteration of the effects, and produced no new phenomena; not even when a core of iron was put into the copper helix to exalt the inductive effects.

8805. Experiments on any possible peculiar inductive action of a bismuth core. A fine helix was connected with my galvanometer, and magnetic poles applied to the ends of the helix; the needle was affected, vibrating through an arc of 10° or 12°. Then a solid core of bismuth put into the helix and the poles applied; the deviation was in the same direction and apparently to the same extent as when *air* only in the helix.

8806. The magnetic poles were applied to the sides of the helix, but then there was no induction of a current whether *air* or *bismuth* were in the helix.

8807. Then placed this helix between the Magnetic poles of the great magnet, but *across* the lines of magnetic force. There was *no* induced current on making or unmaking the magnet, whether air or bismuth were in the helix.

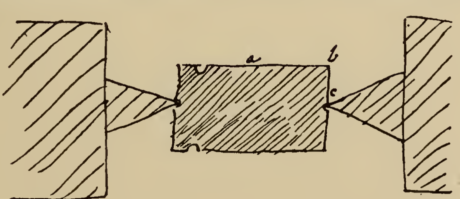
8808. Neither was there any effect by putting the bismuth *in* or *out* of the helix, the magnetic force remaining constant for the time.

8809. Much care required here to guard the galvanometer from the action of derived currents taken up by the wires touching the wood about the magnet, etc. etc. Have to insulate them.

8810*. Three cylinders were prepared of *Iron*, *copper* and *bismuth*, each about 1 inch long and 0·6 in diameter. The great Electro magnet had conical terminations and these were in succession placed between the cones, so that they could be revolved by a string passing round a groove. The object was to see what current would be induced in these cylinders by revolution under the influence of these intense magnetic forces. The ends of the Galvanometer wires could be applied at any part of the cylinders, as at *a*, *b*, *c*, etc.

8811. Much care was requisite to avoid currents by induction or

* [8810]



derivation, beside those due to the mere revolution, but these were guarded against.

8812. When the cylinder was *copper*, and the galvanometer wires applied at *a* and *c*, induced currents were formed, being in one direction for revolution of the cylinder one way and in the other direction for revolution the other way. When the wires were applied at *b* and *c*, the currents were equally produced. But when applied at *a* and *b* they were not produced.

8813. These currents were in the same direction as if the galvanometer wires had been joined and then carried across the Magnetic curves in the direction of the cylinder rotation, and are manifestly of the same kind as those described in my first series of Exp. Researches. That they do not occur when the wires are applied at *a* and *b* is because the part from *a* to *b* does not, in moving, cut the magnetic curves. That they do occur when applied at *b*, *c* is because the part between *b* and *c* in moving cuts the magnetic curves.

8814. Now used the *iron* cylinder. The induced currents were far stronger than before, but the *same in direction and the other circumstances*. They are no doubt due to the same cause, and as iron is a worse conductor than copper, might in that respect have been expected to be weaker. But there is another effect which more than compensates for this. The iron becomes itself magnetic and contains, within its space, magnetic forces of far greater power than those which occupied the same space when the copper was there, and hence the greater intensity of the magneto electric currents produced.

8815. The *bismuth* cylinder gave the same current[s], but even more weak than those of the copper—and they were mingled up with the effect of the thermo-current produced by the friction of the copper wire against the circumference of the cylinder.

8816. Have also arranged a helix of feet of wire, so that cores of Iron, copper and bismuth 4 inches long and $\frac{3}{4}$ of an inch in diameter might be placed in the center, revolved whilst there, and have currents drawn off from the equatorial and polar parts. When the iron core was revolved, the galvanometer wires applied to the equator and to the center of the end of the cylinder gave good current, varying with the direction of rotation, just as if the iron had been between the magnetic poles. The core was of course very magnetic.

8817. The copper core gave the same currents, but very feeble—not being a magnet.

8818. The bismuth core gave the same current whichever way it was revolved—and this was due to the friction of the wire rubbing against the equatorial parts. So whether the battery current was passing through the helix or not, the effect was the same. Still, when the current was on, there was a difference in the amount of effect for the two directions, which shewed that currents tended to be formed here by rotation, as in the iron and copper—but were feeble in their nature and overruled by the effects of friction and heat.

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8819. As to the action of an iron wire helix, as core to itself (8792–9). Two solid helices were made, one of 36 feet of copper wire and the other of 38 feet of iron wire of nearly equal thickness. They were both put into the circuit at once with 10 pr. of Grove's plates, so as to have the same current passing through both. The iron helix was much the strongest as a magnet in its action either on a magnetic needle or on filings. It lifted above 4 times as much filings as the copper helix.

8820. It became much the hotest through the effect of the electric current.

8821. The induction of an *inner* helix on an *outer* iron tube is scarcely sensible (8786): what is its action on an outer helix by way of Induction? Two small copper wire helices were put one in the other, and the electric current sent through one whilst the other was connected with either our large galvanometer or with my delicate one—but there was no sensible action either at the beginning or ending of the current in either helix—they were two small.

8822. Then used a large outer helix containing 190 feet of wire (about $\frac{1}{20}$ of inch), and a much smaller inner coil containing only 36 feet and not in length more than one fourth of the length of the large helix. When the Electric current was through the *outer* helix, it induced a current in the inner helix at *making* and also at *breaking* contact—of 15° or 20° by my galvanometer. But when the current was sent through the inner helix, it induced correspondent current in the *outer* helix, and these were far more powerful than before. So that the inner helix can induce on the outer helix effectually and in that respect differs from its action on *outer iron* (8786).

8823. The reason why the induced current was greater is probably because far more electricity would pass in the same time through the inner than the outer helix, because of the lesser resistance of the shorter wire. If helices alike in length, then probably the amount of induction would be the same.

8824. In considering the induction on the *outer helix* or the *outer iron tube* and comparing the difference of effect, we must remember that the currents induced in the helix run all round the inner helix, whilst those in the iron are theoretically round the particles. So in the outer helix the part outside and the part next the inner helix have the current produced in the same direction, whereas the iron would, as to its particles, have currents in these two parts moving in contrary direction.

8825. In fact the induction in the iron and the induction in the helix are different in their nature, in this respect as well as in others, for that in the helix is only momentary at the beginning and end, being in the two cases contrary; whilst that in the iron is permanent for the time.

8826. Must try and compare and co-relate these differences, for they ought to come under one principle of action.

8827. What would be the mutual action of an inside and an outside helix on each other—an iron core being between them—seeing that it would be magnetic to the outer current and not to the inner?

8828. A helix was placed vertically and a small iron core suspended in the axis; then an electric current sent through. The core was instantly and powerfully attracted to the side of the helix—to whichever side or part was nearest to it. If the core were only one third the length of the helix, so as to be entirely within, still the same result took place with equal force.

8829. So the center or axis of the helix is not the place of greatest force upon an iron core within, i.e. is not its place of rest. The core no doubt disturbes in some degree the mutual relation of the forces and their disposition.

8830. When a ring helix was used the same effect took place. When the ring was made into a triangular form, then the iron core tended with most force up into the corners presented by the angles—but always up to the wire carrying the electric current.

8831. A core of bismuth in a helix of copper wire with a current passing through it, was applied to a suspended ball of bismuth and also to a suspended bar of bismuth, but there were no sensible signs of any mutual effect.

8832. It was also applied to fine iron filings, but with no sensible effect.

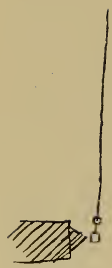
8833. Oil of turpentine (as a rotating fluid naturally) was placed between the magnetic poles of our great magnet and examined by a polarized ray passing parallel to the lines of magnetic force: it had its rotative force affected by the magnetism in the now well known way (7609). The polarized ray was then passed across the lines of magnetic force, i.e. at right angles to them, and then the oil of turpentine was examined by it—but not the slightest alteration of the rotative force in this direction occurred. So that the Magnetic rotation seems in this respect to be thoroughly independant of the natural rotative force, and the two to have no influence on each other.

8834. I have an apparatus consisting of a ring of iron made out of a bar about $1\frac{1}{2}$ inches wide and $\frac{3}{8}$ of an inch thick, the bar being welded into a ring about 2 inches in diameter and $1\frac{1}{2}$ inches high. This ring has copper wire wound round it in the fashion of a helix, the convolutions of which are of course nearly parallel to the axis of the ring; and when the electric current is sent through it, the iron ring, which is as a core to the copper helix, must become a very powerful magnet, but as it is a ring magnet, its power is all *internal*.

8835. Placed a prism of heavy glass in the axis of this arrangement—sent the electric current through—and examined the glass by a polarized ray. No effect—even though I passed the ray by reflexion three times along the glass.

8836. Put the whole arrangement of ring and glass between the poles of the great magnet, the glass being transverse to the lines of Magnetic force. No effect.

8837. Placed the ring and glass so that the glass was parallel to the lines of magnetic force, and now of course the polarized ray was rotated; but whether the ring magnet was in or out of the current of electricity, the effect was the same. Hence no effect of the ring magnet on the glass.



8838. Repulsion of bismuth—an apparatus arranged to shew it and does very well.

8839. Should do it also very well by putting the lever horizontal instead of vertical—suspending it by a few cocoon silk strands.

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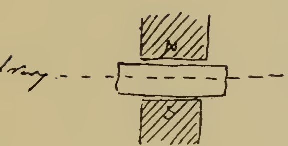

8840*. A square prism of *heavy glass* about 2 inches long and polished at the ends was mounted so that it could be rotated on its axis between the poles of the great magnet, either in a direction parallel to the lines of magnetic force or perpendicular to them—a polarized ray being at the same time transmitted through the glass parallel to the axis of rotation. The rotating apparatus was a multiplying wheel of considerable power, so that the revolutions were probably as high as 20 in a second.

8841. This rotation of the glass produced not the slightest effect on the phenomena—the polarized ray was affected or unaffected precisely in the same manner, and to the same amount, whether the glass was in rapid rotation or quite still.

8842. So nothing here came forth in the glass analogous by its phenomena or otherwise to that condition which moving conductors take up under the same circumstances, and by which currents of electricity move or tend to move in them.

8843. Dynamic electric induction produces two *inducteous* currents, at the beginning and end of the *inductric* current. When Iron, as a core or otherwise, is present and becomes magnetic by the continuing inductric current, one assumes hypothetically that it is because of the production or arrangement of *permanent* inducteous current: in which case one might perhaps be led to expect or hope for some permanent inducting action from these currents back again on to the conductors near to or surrounding

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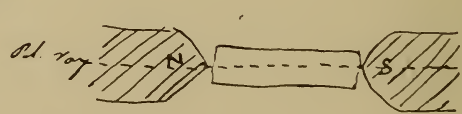

them. So without much expectation made the following experiments.

8844. Two covered copper wires were placed parallel to each other in a tube of soft iron 2 feet long and half an inch external diameter and 0·1 thick, and then a current of 10 pr. Grove's plates sent through one while the other was connected with a very delicate Galvanometer. There was *no* permanent induction of one wire on the other—or any apparent difference in the effect whether the wires were in the tube or out of it.

8845. Two helices, one of iron wire and the other of copper wire, much intermingled in their disposition but separate at the ends, were connected with the Galvanometer and the battery, the inductric current passing sometimes through the iron and sometimes through the copper. No permanent inductive effect was obtained, or any effect different to that of two copper helices.

8846. Two involved helices of copper wire, one shorter of thicker wire, and the other of a great length of very fine wire, were taken, having also a core of a bundle of *iron wire*. Whether the inductric current was on to the shorter or longer wire, or the iron core were in or out of its place—no constant inducteous current was produced, or any thing different to the former principles of inductive action. My delicate Galvanometer was used.

8847. I have a thick iron ring in the form of a short cylinder; if cut through and opened out, it would be a bar 6 inches long, $1\frac{1}{2}$ inches wide and 0·4 of an inch thick or nearly so. This ring is covered with copper wire which, passing up the inside and down the outside, forms a helix round the iron, which in its turn is as a ring core to the helix. A second copper wire helix was put over the first. When either of these helices carries a current, the iron becomes of course powerfully magnet., but also of course manifests little or no external magnetism, for it has no terminal poles; being a ring and therefore endless. When an electric current was sent through one of the helices, there was a very powerful current induced in the other at *making and breaking* contact, as was to be expected—but *no permanent* or *peculiar* effect.

8848. I made as many as fifteen small helices—bound them up in a bundle side by side and connected them so that a current should pass through them all in succession, they being all in similar and uniform position. I connected this bundle with the galvanometer—put it into the middle of a good inducing helix of copper wire, and sent the electric current through the latter. There was no permanent or peculiar inductive action—but only the effects at the beginning and ending of the inductric current, which would have been produced by *one* single internal inducteous helix.

8849. A similar bundle of small *iron* wire helices was used with exactly the same effects.

8850. The iron ring and helices were arranged with a bar of bismuth going through the hole (or axis) in the middle of the arrangement, which therefore was parallel to the general direction of the helix wire but perpendicular to the parts of the Magnetic ring core. This bismuth bar was connected with a galvanometer, and one of the helices on the ring with a Grove's battery. There was no permanent effect at the Galvanometer, but the temporary currents on making and breaking contact.

8851. A copper wire in the place of the bismuth bar produced exactly the same effects. These were not new effects, but effects of the ordinary Magneto electric induction.

8852. I have a similar arrangement in which the core is not a ring of iron but of *wood*—box wood. This shewed no effects with the bismuth bar or copper wire—illustrating the increased power given by the iron ring core.

8853. Had to be careful of accidental thermo currents with the bismuth.

8854. An ordinary hollow copper helix has been formed containing 154 feet of wire. Then 60 feet of covered Iron wire has been carried up the inside and down the outside, covering the first helix with a second, the wire of which is at right angles to the first, just as if the first helix were the iron ring described a while ago (8847). When the battery current was on to the inner copper helix, there was no induced current through the iron to the galvanometer. When the battery current was through the iron helix, there was a little induction at *making* and *breaking* contact—but no permanent or peculiar effect. The effect was no doubt of the old kind.

8855. Another arrangement consisted of a first ordinary hollow helix of 98 feet *Iron* wire—covered as just described by a second helix of 60 feet of *copper* wire—to reverse the places of the two former metals (8854). No effect at the Galvanometer except a very slight temporary result of common induction.

8856. As to action of inner and outer helices on each other, and the relation of the results to the action of an inner or an outer helice on a cylinder core of *Iron* (8781, 6), the following are results.

8857. A simple helice of 37·3 feet of copper forming one layer of spirals was put on a glass tube—a second helice of 40 feet of the same covered wire in one layer of spirals was put over the first, so the two formed an inner and an outer helix of very nearly equal quality and power. Now whether the inductric current passed through the inner helix, inducing upon the outer, or passed through the outer helix, inducing upon the inner, the *amount* of the *inductive* action was the *same*. I could perceive no difference in the effect.

8858. If the inner coil acting upon an *outer* tube of iron be compared with the outer coil acting upon an inner tube of iron, then the effects are very different (8786), and this difference is made still greater in that when the effect does occur, i.e. when the iron is *inner*, then it is also permanent.

8859. Made an arrangement of two similar helices, an inner containing 55 feet and an outer 65 feet of the same copper wire—but between the two placed a layer of 75 straight soft iron wires in a very regular uniform manner, so as to form the equivalent of an *iron tube* between the two helices, and therefore being a magnetic core to the outer helix but not to the inner.

8860. In this arrangement, the outer helix carrying a current made the iron wire core strongly magnetic. The inner helix made it magnetic in a weak degree, according to that before described (8786). Again, the outer helix made the iron strongly magnetic, whether the inner helix were included in the circuit with it or not—and also when, being included, its direction were consistent with or contrary to that of the outer helix. These variations made very little difference in the magnetic force of the iron, which seemed subject almost entirely and exclusively to the outer helix.

8861. But when the currents induced by the inner and outer helices upon each other were examined, they were just as if no *iron* were there. There were the little momentary currents at the beginning and the end, whichever helix was made *inductric*; and there was no permanent effect. There was no difference, except that when the battery was on the *outer* helix—there was a little halt and set on, etc. in the induced current, as if the iron were doing governing something for a moment; but the final result was the same, that is, the sum of force in deviating the galvanometer needle was the same.

8862. This effect was probably produced thus. When the current was in the outer helix, the iron became a magnet, but being outside the inner helix, could not induce on it to affect the inducteous current (8788); but it would for the moment induce on the outer or inductric current, retarding it ( ), and so affect the inducteous current in this indirect manner, and as described. On the other hand, when the Inductric current was in the inner helix, it would not render the iron much magnetic if at all, and so it could not act on the outer or inducteous helix—but the latter would be affected by the power of the inner helix alone.

8863. Still, the effects shew a great practical difference in the induction of a current in a helix or wire upon another helix or wire and upon Iron.

8864. I arranged a triple helix—the first or *inner* helix was a single layer of spirals consisting of 644 *inches of copper wire*. The second or *middle helix* was also a single layer of spiral[s] superposed on the first, consisting of 700 *inches of Iron wire*. The third helix or *outer* one was a single layer of spirals superposed on the iron, consisting of 704 *inches* of the same *copper wire* as the first helix.

8865. The *inner copper* was connected with the galvanometer and the *outer copper* with the battery—no new effect or constant current—nothing but the short momentary currents at making and breaking contact.

8866. The *outer copper* to the Galvanometer and the inner copper to the battery. Same results.

8867. The *middle iron* to the Galvanometer and the *inner copper* to battery. Same results—but also a little constant deviation produced by a derived current, through the coatings of the covered wire.

8868. The *middle iron* to the Galvanometer and the *outer copper* to the battery. Same as the last, in all things.

8869. The *inner copper* to the Galvanometer and the *middle iron* to the battery. The first brief currents by induction—little if any derived current—but a poor chief current through the iron helix.

8870. The *outer copper* to the galvanometer and the *middle iron* to the battery. As the last but one—in all points.

8871. The inner and middle helices joined in contrary directions, and with the battery. The out. with the Galvanometer. No results, as might be expected.

8872. The middle and outer joined in contrary directions and with the battery, the inner to the galvanometer. Nothing.

8873. The inner and out. joined in contrary directions and with the battery, the middle with the galvanometer. Nothing.

8874. The inner and middle joined in contrary direction and with the *galvanometer*—the outer with the battery—nothing except very small brief currents, at making and breaking contact.

8875. The inner and outer joined in contrary directions and with the *galvanometer*, and the middle to the battery—nothing.

8876. The middle and outer joined in contrary directions and with the *galvanometer*, and the inner with the battery—nothing.

8877. There were occasionally currents, thermo and others, produced *during* the arrangements, but the above are the true and normal results.

8878. All these effects are the results very clearly of the former principles of Induction, and there is no effect which touches closely the Magnetism of the Iron, or any thing like the currents in it shewn any where in the results.

8879. A piece of heavy glass was placed in the center of the ring core (8847), its ends being coated with pieces of tin foil. One of these was connected with the ground and the other with a very delicate Bohnenberger's Electroscope, and then the electric current sent round the ring by its helices. There were signs of Electric tension at the Electrometer, but gradually these were all traced to the inductive (static) influence of the ring, coil, etc., according as it was touched by the one or other pole first and so brought into a Pos. or Neg. state. There was no signs of static tension in the matter of the glass by reason of its relation to the Electro-magnetic forces.

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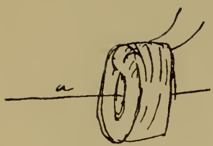


8880. My iron ring magnet—the galvanometer wire through the middle and a battery of 10 pr. Grove's on to the helix of the ring. No permanent induction, and battery contact being continued, no position of the galvanometer wire *a* produced an induced current—neither any motion of the wire. My great magnet and the poles stood as in the figure*, and when the E. current was on, the marked pole was the further in as expressed. My delicate galvanometer stood about 8 feet off and towards the left hand, as if up in the left hand top corner of this page ϕ[1]. Wires proceeded from it and where [? were] joined together into an endless wire passing between the poles, as in the figure. My object was to see if the position of the wire in respect of the magnetic axis of the magnetic field was of any consequence.

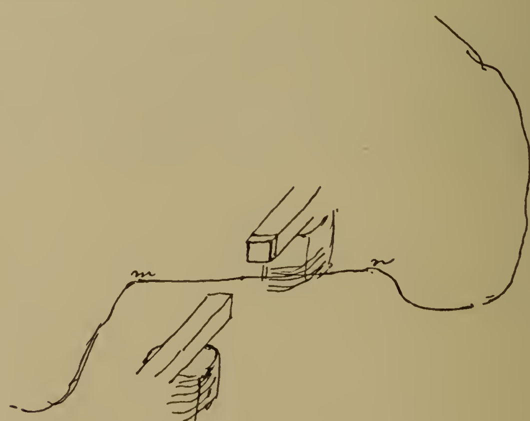
8881. When the part of the wire from *m* to *n* was directly across the middle of the magnetic field, a given end of the Galvanometer needle was *deflected left on making contact*, and deflected right on breaking contact—and there was no permanent deflection. We will call deflection *to the left* STANDARD *or* S.

8882. When the wire *m n* was brought into other positions, above or below the axial line of the magnetic field, still the deflexions were exactly the same as before in direction, but perhaps not quite so strong.

8883. If the wire were in any position parallel to *m n* above or below the middle of the Mag. field or the opening of the poles—and battery contact were made and continued with the magnet, then the galvanometer circuit closed and all found to be still, on

[1] The figure is to the right hand and a "ϕ" appears in the top left hand corner of the MS. page.

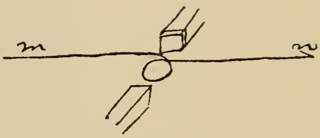
* [8880]

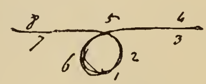


moving the wire up or down so as to cut the magnetic curves, there was deflexion. On carrying the wire down, the deflexion was S (8881), or like that of making contact, and on bringing it up, it was the reverse, or as on breaking contact.

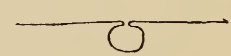
8884. In this case the galvanometer wire, i.e. the leading loose part of it, [may] be considered as a wire ring (very large), equivalent to a wire passed once round the end of the magnet, or as one coil of a helix which is about to be passed over its end to shew the effect of motion; and so far the inductions accord with those of my first series and should be as described. But when we consider *m n* by itself, it seems singular that, whether in the very middle, or on the one side or on the other side of the field of magnetic forces, it should be always affected in the same direction.

8885. I have a ring helix $1\frac{1}{2}$ inches in diameter, and containing 24 spirals or convolutions of wire. This ring was introduced into the galvanometer wire circuit, so as to be included in the part between *m* and *n*. Now however it was placed so that the magnetic axis did not pass through the circle of the ring, but rather across the places 1–8, etc.; then the deflection was S (8881); but when the magnetic axis was through the center of the ring, then the deflexion was *not sensible*.

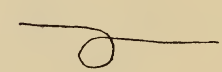




8886. On the other hand, when the helix was turned half way round either on a vertical axis or a horizontal axis, so that the direction was as in either of the two positions figured, then the positions such as 1–8 produced the same deflexion S as before; and also when the magnetic axis coincided with the center of the ring, the direction was still the same or S, but stronger than with the single wire.



8887. Put the helix in this position—then when used as a simple wire, i.e. in positions 1–8 (8885) the deflexion was always S. The mag. poles being 3 inches apart, the ring was put midway between them and across the mag. axis; the deflexion was still S, but weaker than in the former cases. The helix ring was then carried gradually nearer to one pole (pointed), but always parallel to itself; as it neared the pole, the S deflexion on making contact became less and at one place ceased—being carried to position still nearer the pole, the deflexion was *reversed* and continued so, increasing in strength for smaller distances. This happened if the



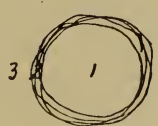
ring were carried towards the other pole exactly in the same order and manner.

8888. Then the ring was placed permanently midway as above (8887) and gave the S deflexion on making contact. But on approaching the poles (magnetic) towards each other and the ring helix, the induced current on making contact made the S deflection less strong—at a certain distance the S deflexion ceased, and at nearer distances the deflexion was *contrary* for the ring whenever the mag. axis passed through its opening.

8889. If the ring were then moved so that the mag. axis went through the wire forming its side, the deflexion was *nul*—and if still further moved so that it passed outside, it was S, in accordance with former observations (8885).

8890. These inductions were all temporary—there was no permanent inductive effect—and the reverse currents and deflexions occurred in all cases on breaking contact.

8891. Now brought the leading wires from the Galvanometer both to one side of the magnet and twisted them together, so as to obtain the effect upon the *ring helix alone*—the wires were cotton covered. When in relation to the connexion with the galvanometer the helix was in this direction, and the magnetic axis passing through its center—making contact gave the reverse to S deflexion; and when its relation in position was thus, making contact gave S or *standard* deflexion, and these were about equal in power.



8892. When the magnetic axis was in the center of the ring helix as at 1, the deflexion was strongest. When across the body of the ring as at 2, it was weaker considerably, but in the same direction: when at 3, it was weaker still, and when 2 or 3 inches off, it became *nul* or nearly so—*but it never changed in direction.*

8893. Hence in respect of the first experiments (8880, 8885), it appears that the simple galvanometer wire as first arranged produces one effect consequent upon the action of the magnetic forces diffused around and effectual on a large part of its course—and that the helix has its own effect, which may be either added to or taken from that of the general wire (8884). Still, that a single convolution (8885) should neutralize the whole of the wire is curious, and many other points about it deserve examining.

8894. In a ring helix the wire above the axis appears to have a current in one direction, as right, whilst the wire below the axis has at the same time the current induced to the left. But in a single wire, whether above or below, it is in the same direction, unless indeed the more distant parts overpower by their length the part in the magnetic field.

8895. Used the ring helix (8885) and galvanometer but introduced a little constant hydro electric current, so as to keep the galvanometer needle deflected above 40°—on purpose to see if there were any action on a current, *permanent* or *otherwise*. The temporary induction on making and breaking contact were hardly sensible because of the bad conducting power of the hydro element—but there was no new or additional effect—nor any permanent action of the magnetism on the current in the helix.

8896. Whether the helix ring were flat or perpendicular or edgeways or central—there was no *permanent effect*.

8897. Then used a constant *thermo current* in place of the hydro current (8895). Now all the temporary inductions came out well and just as before (8885, etc.); there was no change in the character of the deflexions and no permanent effect whatever, though the ring was placed in every possible position.

8898. The ring helix was used alone, i.e. without any current through it and in every possible position—but there was *no permanent current* without motion.

8899. A *solid iron helix* containing 97 feet of wire was used in place of the ring helix. The temporary inductions, or those of motion, were very powerful; but in whatever position it was placed, along, across, upright, etc. etc., *no signs* of a permanent current could be obtained from it.

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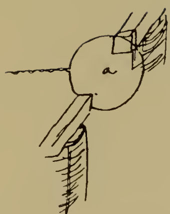
8900. Made a circuit including my Galvanometer, a voltaic pair of zinc and copper wires dipping in distilled water and a making and breaking place w[h]ere the two surfaces were, one of liquid mercury and the other the point of a needle. By ref[l]exion from the surface of the mercury, it was easy to see when the needle point was in contact and if more or less.

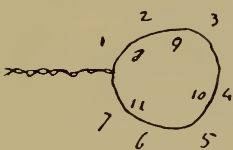
8901. Contact (to the deflexion of the galvanometer) was very soon effected. It was just possible to deflect the mercury surface in a very small degree without affecting the galvanometer, but the least degree more caused the current to pass. A very slight contact is contact enough for the current to pass.

8902. Put a layer of oil of turpentine on to the mercury and now it was much more difficult to pass the current. The needle might be dipped an inch or more in the mercury and yet the current not pass—until a shake or jar made contact and then the current passed.

8903. When a little salt water was put into the Voltaic arrgmt. so as to make a stronger electric current—the current broke through the film of oil of turpentine with much more ease than before. As if the electric condition helped it.

8904. A blunt round bodkin made contact as easily as the pointed needle upon the surface of the mercury.

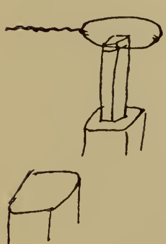




8905. Experiments with a single ring or spiral of covered wire (8891); the ring was about 8 inches diameter and the two ends twisted and carried away together, so as to remove any effect of the distant part of the wire. The Mag. poles were as before. When the ring was placed between the two horizontal poles, so that the center *a* coincided with the mag. axis, the def[l]exion of the galvanometer was left or S (8881); but when it was raised or lowered, so that the magnetic axis passed nearer to the sides of the ring or outside the ring, as indicated 1–11, still the deflexion (on making contact) was always the same or S.

8906. When the ring was carried still lower down, so as to be between the upright limbs of the great horseshoe, found a place where the effect was nul.

8907. When the helix was turned half round, then the same series of effects occurred, but the deflexions were in the contrary direction.



8908. Then set the marked pole up thus, so as to have it move simply in relation to the helix ring. Found that effect of making contact was to deflect galvanometer to the left (as before), the effect being strongest when the upright pole was concentric with middle of the ring—less as it neared the sides and *none* when outside the ring* in any part.

8909. So the ring and the wire takes not[e] of the forces all

* [8908] 

round and the effects fall into the old results of Magneto Electricity.

8910. When the poles were horizontal and near together, and the large ring of 8 inches diameter (8905) symetrically between them, there was a certain amount of deflexion on making contact or breaking. When the ring was lessened to one of 2 inches diameter, there was the same kind of effect but not nearly so much of it. This was between the two poles and the limbs of the magnet.

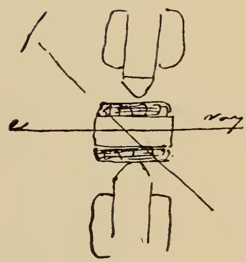
8911. When the pole vertical (8908) and current contnd. at battery, moving the ring over the pole gave a current as in the old experiments, but moving the ring outside the pole, either downwards or upwards, gave nothing.

8912. The poles were placed at a certain distance and then sometimes air, at other times oil of turpentine or strong solution of sulphate of iron, was placed between them, so as to occupy the space there; and a ring helix was then placed between the poles in different positions and moved in different ways to examine if the difference of media caused any difference in the results—but there was *no difference* either in direction or apparently in quantity.

8913. An iron helix used in these different media with the same negative result.

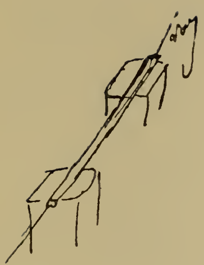
8914. The heavy glass placed in the magnetic field and examined by the polarized ray under the influence of the magnet, in different position[s], from one parallel to the lines of magnetic force, up to one perpendicular to these lines. There was no difference in the nature of the effect; the ray was rotated and to the same side, but the effect was a maximum when the ray and the lines were parallel—diminished as these became oblique and was nul when they were perpendicular. If the revolution of the glass was continued beyond the perpendicular line, the rotation gradually appeared but in the opposite direction.

8915. A piece of heavy glass surrounded by a helix and soft iron (   ) was placed between the poles of the great magnet and so connected that the current could pass through the helix, and so make the glass active on the ray passing along it; or could pass through the helix and also round the magnet, so as to add the latter effect in a transverse direction to the former. The effect of the *helix alone* was to cause rotation of the ray in a certain



direction to a certain degree. Then adding on the effect of the magnet (which was far greater in proportion than that of the helix) caused no change in the phenomena observed at *e*, i.e. the rotation produced by the helix alone continued in the same direction but was diminished in degree—only because the electric current had now to pass through a much longer circuit and was diminished therefore in force and consequently in the helix.

8916. Two magnetic induction[s] at right angles to each other did not therefore affect the results. Probably a ray crossed in the direction *f* would have shew[n] the sum or the difference of the results of their oblique actions.



8917. A small iron tube laid across from pole to pole, and a polarized ray passed through it. No effect could be observed upon the air in the tube when the magnetism was on or off.

8918. A bundle of small straight wire placed so as to reach from pole to pole and the ray passed along the minute tubular interstices—but no effect could be produced.

8919. A straight iron tube was laid as a rod from pole to pole and the polarized ray passed close along the outside of it. When a piece of heavy glass was laid in the course of this ray, it manifested rotation. When the rod was away and the glass *between* the same magnetic poles—there was still *left handed* rotation and *more* than before.

8920. Now instead of the glass, looked through the *air* by the side of the iron rod, and thought I saw a trace of *left handed rotation*—thought so even before trying with the glass as above.

8921. Still I could make out nothing distinct and suppose on the whole that I must have been mistaken.

8922. With clean iron still the same negative result.

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Static Electric induction.

8923. A cubical piece of heavy glass *d* was coated on two opposite surfaces with tin foil *e*, *f*, and raised upon a stand of shell lac *g*—two wires pressed lightly against the sides coated, the wire *a* going to the prime conductor of an electric machine and the wire *b* to the ground, so lines of *static induction* passed from one to the other through the heavy glass; *c* is a short cylinder formed of a piece of well drawn tube, thin in metal; a loop is fixed to it on opposite sides consisting of a single *cocoon thread h*; through this was passed another cocoon thread and the two ends carried upwards, and these being made fast above to a wooden pin, the whole was delicately suspended, and by turning the pin the height could be adjusted. The parts were nearly of the size represented in the drawing*[1].

8924. A little piece of cork *i* was placed inside the cylinder *c*—across it and quite within it; it was like a small cork prism fixed there. Then the machine was excited and the movements of the cylinder *c* observed under the induction. They were such as could easily be referred to the static inductive force acting on the cylinder as a regularly formed conducting body placed equidistant from the two coatings.

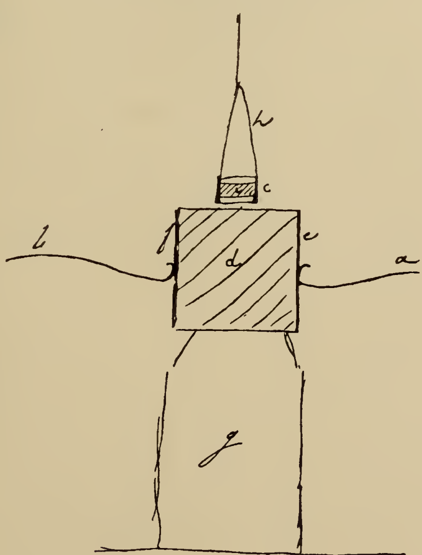
8925. Without disturbing anything else, the coated cube was turned a quarter round in the horizontal direction, simply that the phenomena might be observed in the two rightangled directions lying in the horizontal plane. The effects in both cases were alike.

8926. Then a small powerful magnet, being part of a needle, was introduced into the cork *i* so as to lie in the horizontal plane and quite within the cylinder. This made the whole cylinder point freely when left to itself.

8927. Then the static inductive force was brought on—but no peculiar effects could be observed. The direction of the needle was in no way affected by the inductive forces, and to ascertain that this was so, the cube *d* of glass was moved into different positions round a perpendicular axis, and also the cork and magnet

[1] The diagram is reduced to $\frac{3}{4}$ scale.

* [8923]



were moved into different positions in the cylinder *c*; but no peculiar effect of the magnetic and inductive forces could be observed.

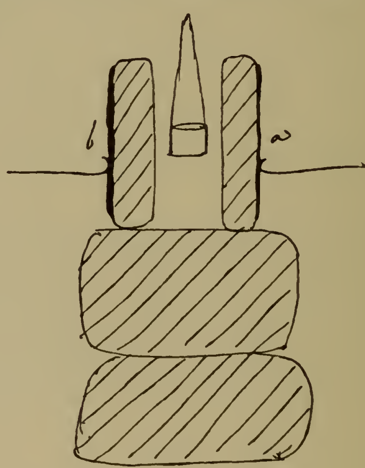
8928. Whilst the static inductive forces were on, the condition of the little magnet in relation to an approached external magnet was observed. It acted quite as if in its ordinary state.

8929. Then a prism of bismuth was made to replace the cork, but I could not see that the action or position of the cylinder under static induction was [affected] in any way by the presence of the bismuth.

8930. Nor that the bismuth had any particular relation to the magnet when approximated.

8931*. Mounted two plates of shell lac parallel to each other and coated on the outer surfaces; then suspended the little cylinder between them and repeated the former experiments, but had no new or peculiar effects, none but those of ordinary static induction.

* [8931]



8932. Last night certain weak aqueous solutions were made—put into small flasks in quantities of about 2 ounces each and placed out to freeze—the object being to ascertain whether the ice really ejected the extraneous matters added perfectly. This morning all the flasks were well frozen except in the central part, about a fifth or sixth part of the fluid there remaining in the liquid state.

8933. 1. Was common *water with* $\frac{1}{1000}$ *of oil of vitriol* added. The liquid portion and the drainings were of course very acid. Washed the piece of ice with distilled water—leaving it to soak now and then—the water contained plenty of S.A. by Mur. baryta—then after a while turned the ice out into a clean mortar—it was somewhat fissured, approaching to rotten ice—broke it up with a little distilled water, crushing the ice—poured off the water and tested it—it contained S.A. but far less than before. Again added water and washed the ice particles, dividing them from each other as well as I could—this water, poured off, contained only a trace of S.A.—repeated the washing and this water contained the least trace—a last washing gave no trace—and the remaining ice (very little) when melted gave a water containing no trace of Sulphuric acid.

8934. Hence I conclude that the true continuous ice contained no Sulphuric acid, but that what was in the ice was in the fissures and especially also in (lining the) cavities formed by the expelled air—numerous bubbles either round or long existed in every part of all the ices, from the expelled air.

8935. No. 2. *Water with* $\frac{1}{10,000}$ of oil of vitriol. The ice clearer than the former—not so many cleavages or fissures, i.e. planes of non adhesion in it. It is chiefly these places which hold the contaminating mother liquor. This ice presented the same order of appearances as to the character of the water by washing or melting of the ice, but the ice was sooner freed from the adhering sulphuric acid—and gave a larger portion of the finally pure ice.

8936. No. 3. Water with $\frac{1}{100,000}$ of oil vitriol. The ice here was very clear. The bubbles formed by the evolving air were very beautiful, being all drawn out in lines proceeding to the center of the flask. The length of a cavity was often 10 or 12 times its breadth or even more. This ice washed well as a lump, and when the washing water was poured off and then a part of the clear ice itself melted and tested, it gave scarcely a trace of sulphuric acid. Still, wherever there were air bubbles, a trace of sulphuric acid could be found. Easily washed clear in the mortar.

8937. No. 4. *Water with* $\frac{1}{100}$ of strong solution of *Muriatic acid.* The crystals seemed quite loose in the middle and the whole of the ice was very rotten and fissured—a thickness of $\frac{1}{3}$ of an inch near the flask clearer, as more solid and less air than the inner parts. It did not wash well in the flask, and I did not try it in the mortar because of the results with No. 6.

8938. No. 5. *Water with* $\frac{1}{1000}$ of *Muriatic acid.* This ice much clearer than the last, i.e. more uniform and transparent, and the air holes in long radial forms as in No. 3. It did not as a lump wash free from M.A. by water, and I did not try it in the mortar because of the results with No. 6.

8939. No. 6. *Water with* $\frac{1}{10,000}$ *of Muriatic acid.* This piece of ice more beautiful than the last and as beautiful as No. 3. It washed perfectly even in the lump, and then the water of the melted ice was found to contain no Muriatic acid.

8940. No. 7. *Water with* $\frac{1}{1000}$ *of Nitric acid.* Ice very porous, rotten and loose—can taste the acid in the portions poured off. Washed it once or twice and then tasted like pure water, and the ice also tasted so, but did not pursue the matter.

8941. No. 8. *Water with* $\frac{1}{10,000}$ *of Nitric acid.* Much finer ice than the last—more compact and clear—air bubbles long and radially disposed.

8942. No. 9. *Water with a little sul. soda.* The mother liquor very impure from Sul. Soda. The ice very rotten—the crust easily pierced by a hot platinum wire when opening a hole to the middle. The ice broke up and washed in the mortar—the presence of Sulphate of Soda rapidly disappeared and the last two or three washings gave little or no trace, and the water of the ice none.

8943. No. 10. *Water with a little Mur. soda.* Ice very rotten and easily pierced by the wire. The mother water very impure. Being washed in the mortar, the salt rapidly disappeared—and at last it disappeared altogether, pure water and pure ice being left.

8944. No. 11. *Water with Mur. Baryta.* Ice as that of No. 11[1]—but in the mortar it easily washed—perfectly pure.

8945. No. 12. *Water with Nitrate of Silver.* The contents of this flask had become coloured, especially towards the middle. The mother liquor was coloured and rich in N. of Silver. The ice was rotten and easily broken—the piece was coloured and more towds. the middle than the side—but the colour rapidly washed out, and after it was gone, the last washing water contained no nitrate of silver—nor did the water obtained by melting the ice then left give any traces of that salt.

8946. From all these experiments, I conclude that the true and compact ice had in all these experiments been pure, containing no trace of the added bodies. These remaining in the liquid portion either in the center space or in the cavities between the parts of the ice.

1 JANY. 1847.

8947. Light and its phenomena connected with my expts. and trials.

Gardner—movements of plants towards a *line* of light or to a *ray*, Indigo ray, etc.? Phil. Mag., xxiv, 7, 8, 9.

E. Becquerel believes that phosphorogenic, actinic and luminous rays (and probably those of heat) are alike, except as the action of bodies *causes* changes in them, etc.; not separate kinds of rays. Ann. de Chimie, 1843, ix, 312, 313, 321.

2 JANY. 1847.

8948. Several days ago, put into different quart bottles certain gases, and then introduced into each, portions of water. Each gas was in 4 or 5 times the volume of water with it, and these were shaken from day to day, until this morning—

No. 1. Water and Carbonic acid gas.
2. ,, Olefiant gas.
3. ,, Coal gas.
4. ,, Nitrous oxide.
5. ,, Nitrous gas.
6. ,, Carbonic oxide.

[1] ? 10.

7. Water and Hydrogen.
8. „ Air.
9. Water with a little sulphurous acid added.
10. Water through which or into which some cyanogen gas had been passed.

8949. Placed portions of these, about 1 oz. each, in glass tubes and immersed them in a mixture of ice and snow, so as to freeze them, to see how far each ice expelled the gas in previous association with its water.

8950. No. 8. Water and Air–froze into a cylinder having a certain degree of transparency, yet interrupted by divisions and also by interstices formed by the expelled air. The amount of air bubbles as usual and their appearances. Knew from former results (   ) that all the air is expelled from water when it becomes ice. When the tube with its ice warmed and a little water poured in, the ice swam as usual, but rose very little above the surface.

8951. No. 1. *Water and Carbonic acid.* The ice looked as white as marble from the multitude of air bubbles evolved in every part–bubbles rose from every place as the thawing process reached it. A little distilled water being put into the tube, the ice floated like a froth, almost $\frac{1}{4}$ of it being above the water–if held down under the water, abundance of gas rose during the melting. From the appearances, I believe that all was expelled during the freezing.

8952. No. 2. *Water and Olefiant gas.* Appearances as of No. 1, quite white and marbly. Know that Oleft. gas dissolves in water (   ) and believe that all is expelled as the water becomes ice. When partly melted the ice floated in the water in the manner of the Carb. acid ice but to a less degree. The quantity of minute bubbles of gas very striking.

8953. No. 3. *Water and Coal gas.* Very like No. 2 in all the appearances; the waters had evidently dissolved the Olefiant and other parts of coal gas.

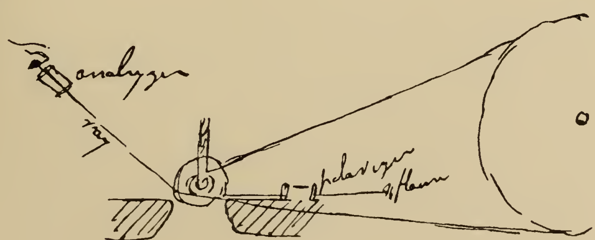
8954. No. 4. *Water and Nitrous oxide.* As the former and about as 3 in appearance. When dissolving, the ice was very buoyant like No. 1, and the gas came away in very large bubbles (fewer in number) than with Olefiant or coal gas.

8955. No. 5. *Water and Nitrous gas.* General appearances as the ice of water—but the ice more buoyant than that of water and more bubbles of gas evolved.
8956. No. 6. *Water and Carbonic oxide.* Ice with air cells about as water ice. The bubbles small but come out abundantly—very like water, or perhaps as coal gas.
8957. No. 7. *Water and Hydrogen.* The ice contains few bubbles than *air water* ice—and when melted the ice I think was more compact and gave out few bubbles than water ice or than any of the other ice—it was also the least buoyant.
8958. No. 9. *Water with a little sulphurous acid.* The ice fissured. Plenty of air bubbles, probably of common air, for it is not likely the sulphurous acid would leave the liquid portions. When the ice was melted, the bubbles came out as if of common air.
8959. No. 10. *Water and cyanogen.* Great abundance of bubbles. On dissolving the ice by warmth, it was found very buoyant, as if the cyanogen gas had formed it into a froth, but as it dissolved, not a bubble escaped, for now the water previously liquid took up the bubbles as they were set free from the ice, and all dissolved. Very good case of the solution in water and exclusion in ice.
8960. Believe that in all these cases the pure clear ice is free from the gases. That pure ice can contain neither air, gas, acids or salts, but is pure in reality.
8961. This fact is quite analogous to that or those of Silver and oxygen, Copper, Litharge, etc. etc.

19 JANUARY 1847.

8962*. Wished to *move* the diamagnetic in the direction of the polarized ray, when in the magnetic field, and that whether the lines of magnetic force were parallel or perpendicular to the direction of the ray. Made the following arrangement. A short cylinder of heavy glass was fixed between plates and mounted on an axle with a little running wheel in a forked frame, which was held by an arm rizing upwards that could be turned round in its socket. A string passed round the running wheel and also round a larger driving wheel which was moved by hand. The running wheel was 0·7 of inch in diameter and the great wheel 17 inches in diameter, so that one revolution of the latter made 24 revolu-

* [8962]



tions of the former. Two or even three revolutions of the wheel could be made in a second = 48 or 72 revolutions of the glass cylinder in that time. This cylinder was 1·1 inch in diameter. The poles of the great magnet ( ) were arranged as in the figure and then the lines of magnetic force corresponded with the direction of the moving particles of the glass between them. A polarized ray was sent over the surface of the magnetic pole on one side, then through the lower part of the heavy glass, which of course bent it upwards as a prism would do, and then out in a still higher direction to the analyzer and the eye as figured. It was easy to adjust the analyzer and the eye so as to see the light as a bright line—or to extinguish it entirely by the position of the analyzer, as in ordinary cases.

8963. When all was arranged in order, so that the polarized beam could be intercepted by the analyzer in its way to the eye, then the magnetism was put on and off, and the effect of Magneto rotation was easily brought out in perfection.

8964. Then the glass cylinder was *rotated rapidly*; this caused no disturbance of the image, or of any of the effects when the magnetism was off—nor did it have any more effect when it was *on*; i.e. the rotation of the glass in *either* direction in no way influenced the phenomena, though the parts were travelling with a velocity of about 200 inches in a second.

8965. Then the arrangement as regarded the magnetic poles was turned round 90°, so that now the direction of the travelling motion and also of the polarized ray was perpendicular to the lines of magnetic force. Now the motion produced *no* effect on the ray, and the magnetism produced *no* effect on the ray, and the two combined produced *no* effect on the ray. So no evidence of any influence of motion either here—or in the former case, when the rotation took place round the ray as axis ( ) and not parallel to it as in the present case.

8966. Arranged the magnet so as to send a polarized ray over the surfaces of the poles, and along the magnetic field, but instead of placing a piece of heavy glass in the field, put the polarizing Nicoll itself there, to see if it would shew any peculiar effect in the act or at the moment of polarization; but could find no effect whatever in addition. The ray was polarized but there was no rotation, whether the magnetism was on or off. Turned the Nicholl end for end but no difference.

8967. Then put the polarizer in its usual place and set the analyzer in the magnetic field—with the same negative results.

8968. Must remember that a piece of iceland spar in the magnetic field would *not shew* the *magneto-electric rotation.*

8969. Placed a black glass plane in the magnetic field so as to polarize by *reflexion*, and observed the ray afterwds.—no action on it by the magnet.

8970. Prepared bottles of coloured media so that they might be placed in the magnetic field. There were *blues* with *Indigo*—blue ink or *prussian blue—sulphate of copper—Greens* of Muriate of Copper—Muriate of Nickel—Reds of Lake particles—Solution of *cochineal* in water—with a little acid—with a little ammonia—solution of iron. Also *proto sulphate of iron*, a saturated solution *coloured blue* by sul. indigo.

8971. A common ray was sent into these and observed by an analyzer, but whether observed alone or with the high magnetic forces on, the lines being then parallel to the ray, no difference was seen. So that *common light* presents no trace of magnetic effect, either in colourless or coloured media.

8972. Then the ray was made blue, red, purple, etc. by being passed through coloured glasses, including glass blue from cobalt, red from copper and purple from Manganese—also green from copper; but whether the *coloured common* rays went into solution of similar or dissimilar colour, no effect could be observed in respect of the magnetism.

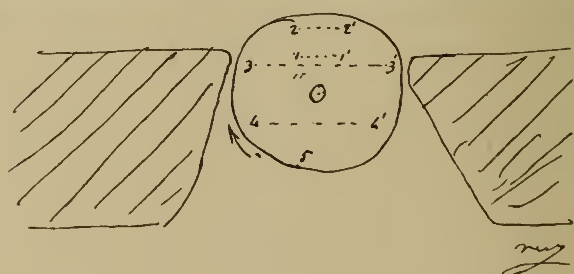
8973. Then all the above variations were made with a *polarized* ray, but still no effect beyond the former and well known effect was produced—that is, the similarity of the ray and solution in colour, or their contrast, was in no way a cause of peculiar effects dependant on or developed by the addition of magnetic forces.

8974. A metal disc was rotated in the magnetic field on an axis perpendicular to the lines of magnetic force, and then the ends of galvanometer wires were placed against its sides—so that the part between the ends should travel from one to the other—or, if the wires were placed at the axis and near the circumference, 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*—

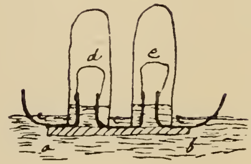
Arago—chem. action of light—Comptes Rendus, xvi, 402.

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.

Date Due
