XVIII. *On Repulsion resulting from Radiation.*—Part II.

By William Crookes, *F.R.S. &c.*

Received March 20,—Read April 22, 1875.

Contents.

81. The present paper is in continuation of one which I had the honour of reading before the Royal Society, December 11th, 1873, and which was published in the Philosophical Transactions, vol. clxiv. part 2, page 501. In that paper I described various pieces of apparatus, chiefly in the form of delicate balances suspended in glass tubes, by means of which I was enabled to show attraction or repulsion when radiation acted on a mass at one end of the beam, according as the glass tube contained air at the normal pressure, or was perfectly exhausted. At an intermediate internal pressure the action of radiation appeared *nil*. Towards the end of the paper I said (70), "I have arranged apparatus for obtaining the movements of repulsion and attraction in a horizontal instead of a vertical plane. Instead of supporting the beams on needle-points, so that they could only move up and down, I suspend them by the centre to a long fibre of cocoon-silk in such a manner that the movements would be in a horizontal plane. With apparatus of this kind, using very varied materials for the index, enclosing them in tubes and bulbs of different sizes, and experimenting in air and gases of different densities up to Sprengel and chemical vacua, I have carried out a large series of experiments, and have obtained results which, whilst they entirely corroborate those already described, carry the investigation some steps further in other directions."

82. I have introduced two important improvements into the Sprengel pump* which

---

* Philosophical Transactions, 1873, vol. clxiii. p. 295; 1874, vol. clxiv. pp. 509, 516. Phil. Mag., Aug. 1874.

The Royal Society is collaborating with JSTOR to digitize, preserve, and extend access to *Philosophical Transactions of the Royal Society of London.* www.jstor.org

enable me to work with more convenience and accuracy. Instead of trusting to the comparison between the barometric gauge and the barometer to give the internal rarefaction of my apparatus, I have joined a mercurial siphon-gauge to one arm of the pump. This is useful for measuring very high rarefactions in experiments where a difference of pressure equal to a tenth of a millimetre of mercury is important. By its side is an indicator for still higher rarefactions; it is simply a small tube having platinum wires sealed in, and intended to be attached to an induction-coil. This is more convenient than the plan formerly adopted (51), of having a separate vacuum-tube forming an integral part of each apparatus. At exhaustions beyond the indications of the siphon-gauge I can still get valuable indications of the nearness to a perfect vacuum by the electrical resistance of this tube. I have frequently carried exhaustions to such a point that an induction-spark will prefer to strike its full distance in air rather than pass across the $\frac{1}{4}$ inch separating the points of the wires in the vacuum-tube. A pump having these pieces of apparatus attached to it was exhibited in action by the writer before the Physical Society, June 20th, 1874.

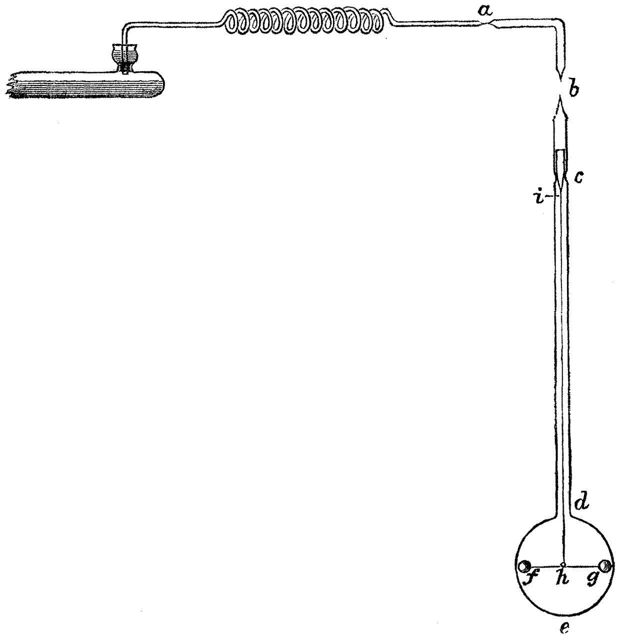
83. The cement which I have found best for keeping a vacuum is made by fusing together 8 parts by weight of resin and 3 parts of bees-wax. For a few hours this seems perfect, but at the highest exhaustions it leaks in the course of a day or two. Ordinary or vulcanized india-rubber joints are of no use in these experiments, as when the vacuum is high they allow oxygenized air to pass through as quickly as the pump will take it out. Whenever possible the glass tubes should be united by fusion, and where this is impracticable mercury joints should be used. The best way to make these is to have a well-made conical stopper, cut from plain india-rubber, fitting into the wide funnel-tube of the joint and perforated to carry the narrow tube. Before fitting the tubes in the india-rubber, the latter is to be heated in a spirit-flame until its surface is decomposed and very sticky; it is then fitted into its place, mercury is poured into the upper part of the wide tube so as to completely cover the india-rubber, and oil of vitriol is poured on the surface of the mercury. When well made this joint seems perfect; the only attention which it subsequently requires is to renew the oil of vitriol when it gets weakened by absorption of aqueous vapour. Cement has to be used when flat glass or crystal windows are to be cemented on to pieces of apparatus, as subsequently described (99, 102).

It would be of great service could I find a cement which is easily applied and removed, and will allow the joint to be subjected to the heat of boiling water for some hours without leaking under the highest rarefactions. Hitherto I have failed to find one which answers these requirements. I mention this in the hope that some one who happens to read this may be in possession of the recipe for such a cement, and will communicate it to me.

84. Before my first paper on this subject was read before the Royal Society I had discarded the balance form of apparatus there described, and commenced experimenting with bulbs and tubes in which quantitative results could be obtained. On

December 11th, 1873, when illustrating my paper, I exhibited to the Society many of these new forms of apparatus. For the purposes of simple illustration, and for experiments where quantitative determinations are not required, I find a horizontal index suspended in a glass bulb the most convenient. The apparatus, with its mode of attachment to the pump, are shown in fig. 1.

Fig. 1.



a, *b*, *c*, *d* is originally a straight piece of soft lead-glass tubing 18 inches long, $\frac{5}{8}$ of an inch external and $\frac{3}{8}$ internal diameter. At one end is blown a bulb, *d e*, about 3 inches diameter. The part *a b* of the tube is drawn out to about half its original diameter, and bent at right angles. The tube is slightly contracted at *c*, and very much contracted and thickened at *b*. At *a* it is also contracted and cemented by fusion to a narrower piece of tube bent in the form of a spiral, and fitting by a mercury-joint into the sulphuric-acid chamber of the pump. The object of the spiral is to secure ample flexibility for the purpose of levelling the apparatus, and at the same time having a fused joint. *f g* is a very fine stem of glass, drawn from glass tubing, and having a small loop (*h*) in the middle. At each end of the stem is a ball or disk, made of pith, cork, ivory, metal, or other substance. *h i* is a fine silk fibre made from split cocoon-silk ; it is cemented by shellac at the upper end to a piece of glass rod a little smaller in diameter than the bore of the tube, and drawn out to a point, as shown. The contraction (*c*) in the tube is for the purpose of keeping this glass rod in its place; when properly adjusted it is secured in its place by a small piece of hot shellac, care being taken not to cement the rod all round, and so cut off the connexion between the air in the bulb and that in the upper part of the tube. The silk fibre is tied on to the loop of the glass stem at *h*. The length of the fibre is so adjusted that the stem and disks will hang about $\frac{1}{8}$ of an inch below the centre of the bulb; that much having to be allowed for the contraction of the silk when the air is exhausted.

85. The bulb-tube is firmly clamped in a vertical position, so that the index hangs freely, and the pump is set to work, the bulb being surrounded with a vessel of water which is kept boiling all the time exhaustion goes on. The gauge soon rises to the barometric height; but the operation must be continued for several hours beyond this point, in order to get the best effects. If the bulb is not heated during the exhaustion, the index loses sensitiveness after it has been sealed up for a few days, probably owing to the evolution of vapour from the pith; when, however, the precaution is taken of heating the pith, the apparatus preserves its sensitiveness. On this account it is necessary to tie the silk on to the loop in the centre of the glass stem, instead of adopting the easier plan of cementing it with shellac. During the latter stages of the exhaustion, oil of vitriol (which has been boiled and cooled *in vacuo*) should gently leak into the pump through the funnel-stopper at the top of the fall-tube (44). This covers each globule of mercury, as it falls, with sulphuric acid, and stops mercury vapour from getting into the apparatus*. I cannot find that any vapour is evolved from oil of vitriol.

When the exhaustion is carried to the desired degree, a spirit-flame is applied to the contracted part of the tube at *a* (fig. 1), and it is sealed off. The apparatus is then unclamped and the tube is again sealed off at *b*. This double operation is necessary to secure strength at the final sealing, which can only be got by holding the tube horizontally and rotating it in the flame, watching the glass to prevent it softening too suddenly.

86. The best material of which to form the index in these bulb-tubes is pith, either in the form of a needle or bar, or as disks at the end of a glass stem. On December 11th, 1873, and again on April 22nd, 1874, I exhibited before the Royal Society a glass bulb 4 inches in diameter, having suspended in it a bar of pith $3\frac{1}{2} \times \frac{1}{2}$ inches. It had been exhausted in the manner above described; and so sensitive was it to heat, that a touch with the finger on a part of the globe near one extremity of the pith would drive the bar round 90°, whilst it followed a piece of ice as a needle follows a magnet.

To get the greatest delicacy in these apparatus there is required large surface with a minimum of weight (75, 76). Thin disks of pith answer these requirements very satisfactorily; but I have also used disks cut from the wings of butterflies and dragonflies, dried and pressed rose-leaves, very thin split mica and selenite, iridescent films of blown glass, as well as the substances mentioned in my former paper (25). Quantitative experiments to prove this law were attempted; but the bulb-apparatus was found too imperfect for accurate measurements, so another form was devised which will be described further on (102), together with the experiments tried with it.

---

[*] By adopting this precaution it is not difficult to raise the mercury in the gauge higher than that in the very perfect barometer by its side, the latter being somewhat depressed by the tension of mercury vapour.

87. With a large bulb, very well exhausted and containing a suspended bar of pith, a somewhat striking effect is produced when a lighted candle or other radiant source is brought about 2 inches from the globe. The pith bar commences to oscillate to and fro, the swing gradually increasing in amplitude until the dead centre is passed over, and then several complete revolutions are made. The torsion of the suspending fibre now offers resistance to the revolutions, and the index commences to turn in the opposite direction. This movement is kept up with great energy and regularity as long as the candle burns—producing, in fact, perpetual motion, provided only the radiation falling on the pith be perpetual *. If the candle is brought closer to the bulb, the rotation of the pith becomes more rapid; if it is moved further away the pith ceases to pass the dead centre, and at a still further distance the index sets equatorially. The explanation of the different movements of the pith index according to the distance the radiant body is off, is not difficult on the supposition that the movement is due to the direct impact of waves on the suspended body.

88. It is not at first sight obvious how ice, or a cold substance, can produce the opposite effect to heat, cold being simply negative heat (33). The law of exchanges, however, explains this perfectly. The pith index and the whole of the surrounding bodies are incessantly exchanging heat-rays; and under ordinary circumstances the income and expenditure of heat are in equilibrium. A piece of ice brought near one end of the index cuts off the influx of heat to it from that side, and therefore allows an excess of heat to fall upon it from the opposite side. Attraction by a cold body is therefore seen to be only repulsion by the radiation from the opposite side of the room.

Bearing the law of exchanges in mind, several apparent anomalies in the movements of these indices are cleared up; and it is also easy to foresee what the movement of a body will be when free to move in space under the influence of varying amounts of radiation.

The heat which all bodies radiate into space can have no influence in moving them, except there be something in the nature of a *recoil* in the act of emitting radiation. And even should there be such a recoil, if the body radiates heat equally all round, the recoil will be uniform, and will not move the body in one direction more than in another. I need therefore only consider the effect of the radiation *received* by a body. Here also the influx of radiation to a body free to move in space of a uniform temperature may be considered to be equal, and it will acquire the temperature of space without moving in any direction.

89. The case is, however, different if two bodies, each free to move, are near each other in space, and if they differ in temperature either from each other or from the limiting walls of the space. I will give here four typical cases, with experiments sufficient to prove the reasoning to be correct.

Case I. Two hot bodies, A and B, in space of a lower temperature than themselves. The body A receives heat uniformly from space, except where the body B intervenes; and on this side A receives more heat, as B is hotter than the space behind it; A will

---

* This experiment was exhibited for the first time at the Royal Society's Soirée, April 22nd, 1874.

therefore move from B. In the same manner it can be shown that B will move from A. The result will therefore be *mutual repulsion.*

CASE II. Two cold bodies, A and B, in space of a higher temperature than themselves.

Fig. 2. Case I.

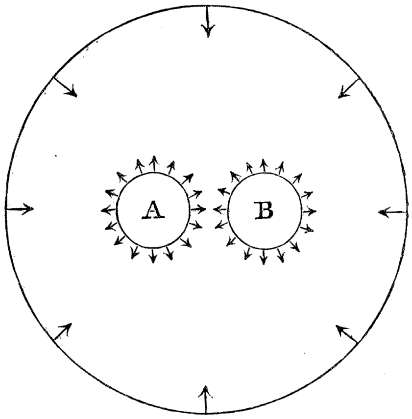
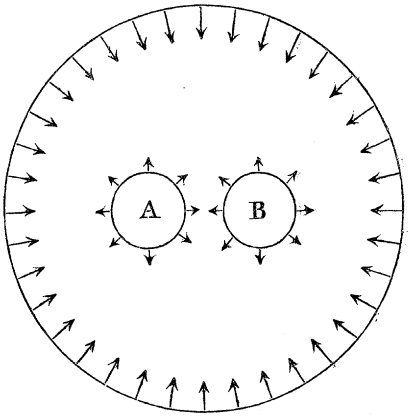


Fig. 2. Case II.



A will receive much heat from space, except where B cuts it off, and on that side it will only receive slight radiation from B. A will therefore be driven towards B. In the same manner it can be shown that B will be driven towards A; and the result will therefore be an *apparent mutual attraction.*

CASE III. Two bodies, A hot and B cold, in cold space. The body A receives heat uniformly from all sides, even from that opposite B (B being of the same temperature as space). A will therefore not move. B receives heat uniformly from all sides, except from that opposite A, on which side the influx of heat is more intense. The result will therefore be that A *remains stationary whilst* B *is repelled.*

Fig. 2. Case III.

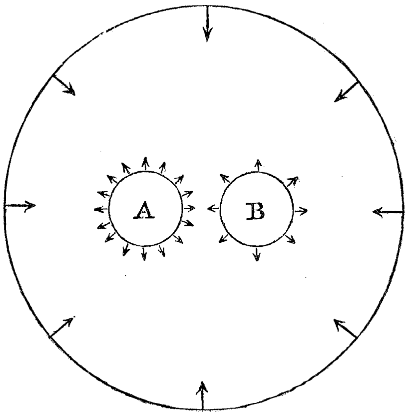
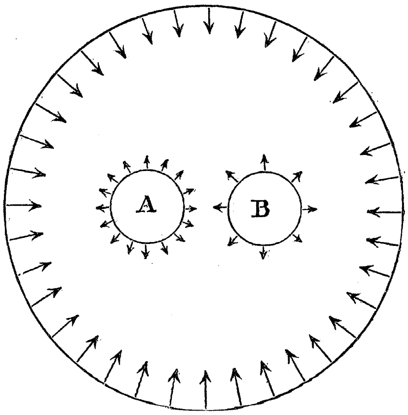


Fig. 2. Case IV.

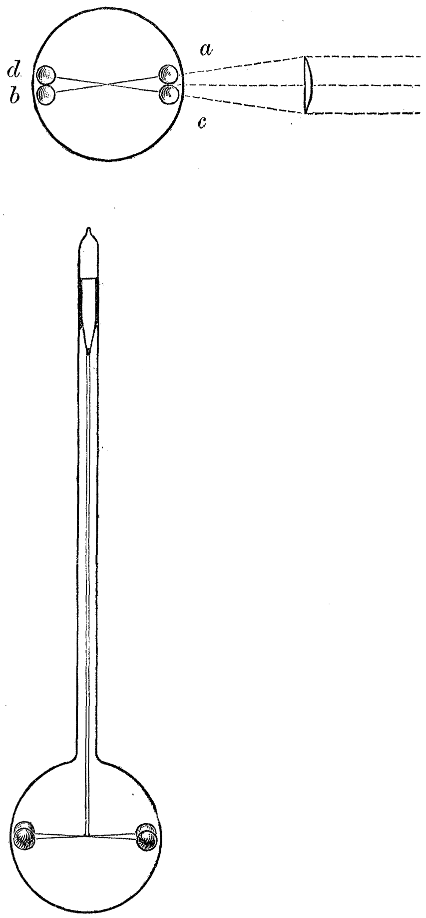


CASE IV. Two bodies, A hot and B cold, in hot space. The body A receives heat uniformly from all sides, except from that opposite B. Here the heat is less intense. A is therefore driven towards B by the extra influx of heat on the other side of A. B receives strong influx of heat from all sides, and just as much from the side opposite A as from any other. B will therefore not move. The result will be that A *will be apparently attracted towards* B, *whilst* B *will remain stationary.*

The force with which the bodies A and B in these four cases will be repelled, or apparently attracted, will vary with their distance from each other, being stronger when they are close and weaker when they are far apart. The diminution will not, however, follow the usual law of inverse squares, but a more complicated law.

90. Experiment proves the above reasoning to be correct. A bulb-tube was prepared in the manner already described (84), but in it were suspended, by separate silk fibres, two glass stems, each having pith balls at its extremity. Fig. 3 shows the elevation and plan of the apparatus. The torsion of the silk fibres was so arranged that the pith balls *a b* hung freely about a millimetre from the balls *c d*. The glass stems were looped in the middle, and bent so that they did not touch each other. After complete exhaustion the following experiments were tried.

Fig. 3.



A beam of radiant heat was concentrated on to the two balls *a c*. When applied momentarily and then removed the radiation simply drove the balls apart, and immediately allowed them to come together again. When, however, the beam was allowed to play upon the balls for about half a minute they became warm and widely separated; and upon now removing the beam of heat the balls did not fall together at once, but took several minutes to regain their original position. This experiment therefore proves Case I.

The bulb and contents being of the ordinary temperature, a spirit-flame was rapidly passed round the bulb to warm it quickly on all sides. The balls were thus in the condition imagined in Case II., being in a space warmer than themselves. They immediately came together, *a* touching *c*, and *d* touching *b*.

Many experiments were tried with the object of proving experimentally the propositions in Cases III. and IV.; but with this apparatus it was found impossible to warm one of the balls without at the same time producing repulsion of the ball by the beam of radiation concentrated upon it. There is, however, little doubt, from the experimental proof of Cases I. and II., that the reasoning is equally correct in the other cases.

91. With a highly exhausted bulb and light pith index, which was found to be exceedingly sensitive to radiation, numerous experiments were tried to see if there was any difference in action between the fingers and a tube of water of the same tempe-

rature. Many persons believe that there is a peculiar emanation or *aura* proceeding from the human hand, and Baron Von REICHENBACH[*] considered that he had proved this to be the case. Were this true it was not impossible that the emanation would affect the pith index. I have been unable, however, to detect the slightest action exerted by my own or any other person's hand which I could not entirely explain by an action of heat.

92. A similar series of experiments were tried with various large crystals, which were presented in different ways and with various precautions to the pith index. At first a decided action was observed; but in proportion as precautions were taken to eliminate the effect of heat, so was the action seen to diminish, until very little doubt was left in my mind that the slight residual action would have been entirely stopped had it been possible, with the apparatus then used, to altogether eliminate the action of heat.

93. Attempts were made to see if chemical action would attract or repel the index. I could not, however, produce chemical action close to the exhausted bulb, without at the same time liberating such an amount of heat as to mask any other action.

94. Although I most frequently speak of repulsion by *heat*, and in illustrating any of the results obtained I generally use either the fingers or the flame of a spirit-lamp as a convenient source of radiation, it must be clearly understood that these results are not confined to the heating-rays of the spectrum, but that any ray, from the ultra red to the ultra violet, will produce repulsion in a vacuum. I have already mentioned this fact in my first paper (58, 68). Experiments proving the similarity of action of all rays of the spectrum were shown before the Physical Society on June 20th, 1874[†]. They were, however, tried with a less perfect apparatus than the one I have since used for the same purpose, and need not be further alluded to till I describe the most recent results obtained with the spectrum (110, 111).

95. Some experiments were tried with the object of ascertaining whether the attraction by heat, which, commencing at the neutral point (30 *et seq.*), increased with the density of the enclosed air, would be continued in the same ratio if the apparatus were filled with air above the atmospheric pressure. Two bulbs containing ivory needles suspended by silk fibres were accordingly adjusted to show the same sensitiveness to a hot body. One was kept for comparison, and the other was attached to an apparatus whereby the internal air-pressure could be artificially increased by a column of mercury. A little increase of pressure was enough to show that the sensitiveness to radiation was greater; and under a pressure of $1\frac{1}{2}$ atmosphere the superior delicacy of the ivory in the dense air was very marked. Attempts to carry the pressure to higher points failed, owing to the bursting of the thin glass bulbs. With a little different arrangement no difficulty would be experienced in carrying the experiments to a much higher point; but hitherto the greater interest attending the vacuum experiments has prevented me from working further in this direction. My friend and pupil, Mr. C. H. GIMINGHAM,

---

[*] Researches on Magnetism &c., translated by Dr. GREGORY. London, 1850.

[†] Phil. Mag., August 1874.

succeeded in the very difficult feat of sealing up some of these tubes under an internal pressure of $1\frac{1}{2}$ atmosphere.

96. To carry this experiment a step further bulbs containing a suspended ivory or mica index were filled with carbonic acid gas, water, carbonic disulphide, ether, alcohol, and other liquids. The index in carbonic acid behaved as if it were in air of somewhat higher density than the atmosphere; movements were also obtained when the liquids were present, but they were so obviously due, in whole or in greater part, to currents, that they proved nothing of importance.

97. Two other forms of the bulb-apparatus require mentioning. A thin glass bulb was blown $2\frac{1}{2}$ inches in diameter (fig. 4). Inside this another bulb was blown 2 inches in diameter, at the end of a glass tube 12 inches long. In this a light glass index with pith terminals was suspended, and the whole was perfectly exhausted. Fig. 4 shows the complete arrangement. In the space between the two bulbs various liquids were enclosed, such as water, solutions of sulphate of copper, alum, perchloride of iron, sulphate of iron, bichromate of potash, sulphate of nickel, &c. These were selected in the hope that amongst them one would be found which would sift out the heat-rays, and so allow me to obtain an action due to light. They, however, only affect the dark or extreme red heat-rays, and do not affect the luminous rays which also have a heating-effect. By throwing a beam of sunlight on one of the pith disks powerful repulsion was obtained, whatever was the surrounding shell of liquid. That all these liquids allowed

Fig. 4.

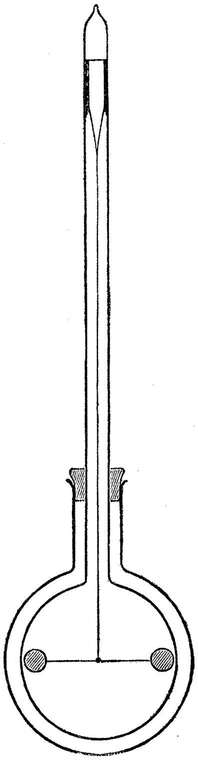
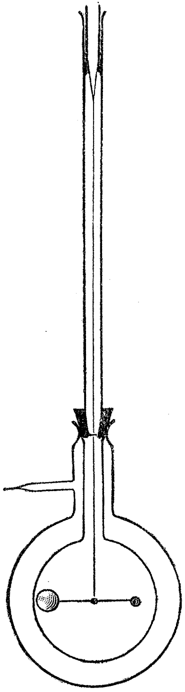


Fig. 5.

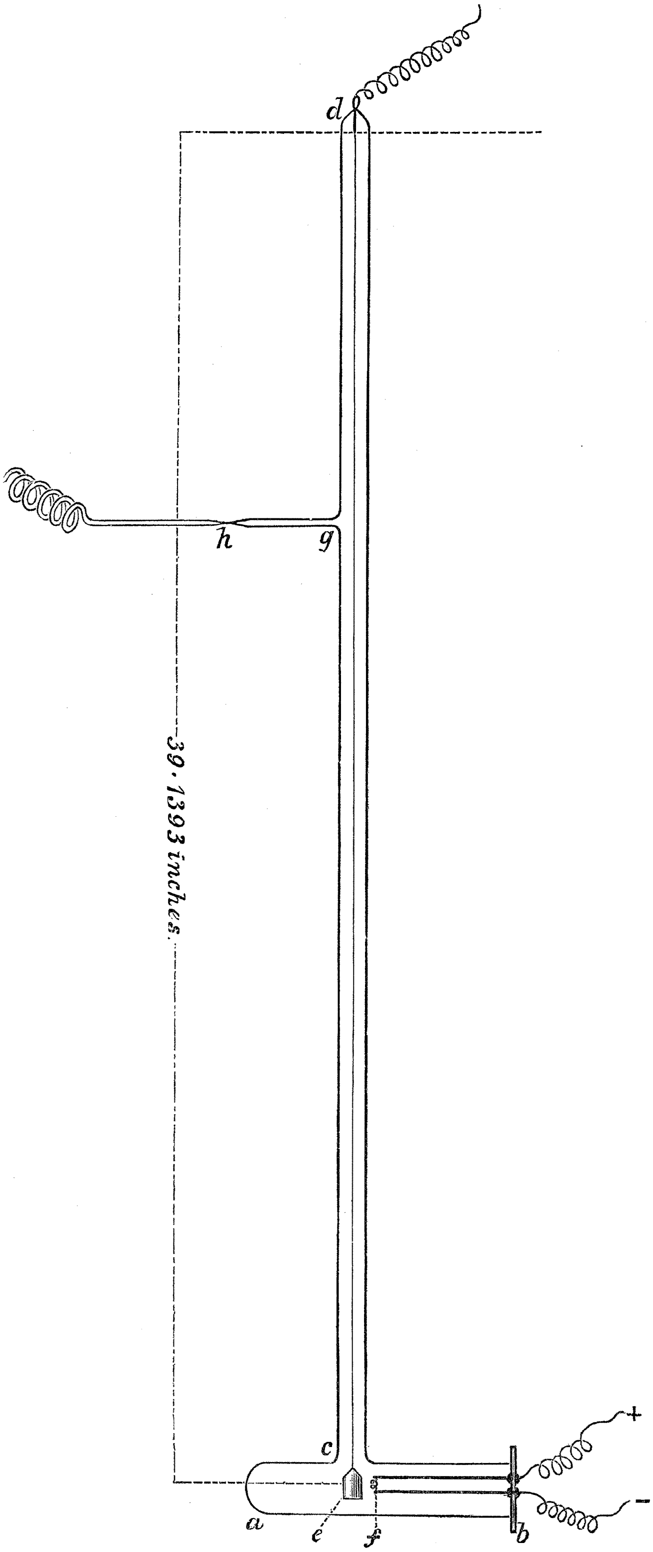


heat to pass through was proved with a thermopile. Solution of sulphate of copper was the most opaque to heat.

98. Another form of apparatus is shown in fig. 5. Two bulbs were blown one in the other, and they were fused together at the necks; to the neck a small tube was fused for connecting with the Sprengel pump. The space between the two bulbs was then perfectly exhausted, and the small tube sealed up. I thus possessed what might be called a spherical shell of vacuum surrounding a bulb open to the air. In this inner bulb was suspended a pith ball on the end of a glass arm balanced by a knob of glass on to the other end, the suspending fibre being protected by a glass tube fitting into the neck of the inner bulb with a cork. It was found that heat applied to any part of the outer bulb passed across the vacuum, and *attracted* the pith ball (suspended in air). The spherical shell of vacuum across which the heat passed, therefore, produced no change of action, but simply behaved like an extra thick glass bulb. This experiment bears upon the speculation in par. 81 of my former paper on this subject.

99. Having succeeded in proving the fact of repulsion resulting from radiation, I was desirous of getting some quantitative estimations of the forces under examination. A pendulum-apparatus was constructed as shown in fig. 6. A wide glass tube ($a\,b$) has fused to it a narrower tube ($c\,d$), about 40 inches long; e is a

Fig. 6.



turned mass of magnesium, weighing 42 grains, suspended by a very fine platinum wire, the distance between the point of suspension and the centre of gravity of the magnesium bob being 39·139 inches, so that it forms a seconds' pendulum; *f* is a spiral made of platinum plate, fastened to two stout copper wires which pass through the thick plate of glass *b*, and thence pass to a contact-key and a battery. The plate *b* is cemented (83) to the end of the tube *a b*, which is ground flat. *g* is an arm fused into the upright tube for the purpose of connecting it to the glass spiral of the pump; it is contracted at *h* for convenience of sealing off. The fine platinum wire is fastened at its upper end to a thick wire which is sealed into the glass, and passes through to the outside for electrical purposes (120). The distance between the pendulum bob and the spiral is 7 millims. To ignite the spiral the current from two GROVE's cells was used; this brought it to a bright red heat in air, and to a white heat in vacuum.

Three feet from the pendulum a telescope was firmly clamped to the bench; it was furnished with a micrometer-eyepiece, with movable spider-threads and graduated circle. The edge of the magnesium bob was brought into the same focus as the traversing cross wire. Observations were taken in the following manner:—The observer at the telescope brought the cross wire to zero, and then adjusted it to coincide with the edge of the pendulum bob. An assistant, guided by a seconds' watch, pressed the contact-key down for one second, then broke contact for a second, next made contact for the third second, and so on, alternately making and breaking contact for either 10, 20, or 40 seconds, counting the seconds aloud. At each second the swing of the pendulum increased; and the milled head of the micrometer was kept turning so as to let the cross wire keep up to the furthest point to which the pendulum vibrated. At the end of the experiment the position of the cross wire was taken and its distance from zero recorded.

100. Experiments were first tried in air of normal density. The pump was then set to work, and observations were taken at different heights of the gauge. The difference between the height of the gauge and that of the barometer gave the tension of air in the apparatus in millimetres of mercury; this is recorded in the first column of the following Tables. The second column gives the greatest amplitude of the half oscillation of the pendulum in millimetres—the sign *plus* signifying attraction, and *minus* repulsion.

Near the centre of Table I., in the second column, are five observations to which I have affixed no sign. When trying the experiments I thought that either I had mistaken the direction of impulse, or my assistant had commenced to count the make-and-break seconds wrongly, as the movement *seemed* to be repulsion. Never having had repulsion at such a pressure before, I was not prepared for it; and fearing there might be an error, left the sign queried. Another series of observations were taken to re-examine this point; they are given in Table II.

It is worthy of notice in these Tables that the attraction by the incandescent spiral is only moderate in air of ordinary density. The attraction diminishes to a

TABLE I.

Tension of enclosed air, in millims. of mercury. Temp. = 16° C. Bar. = 772·55 millims.	Amplitude of half oscillation, in millims., at end of 40″ observation.
772·55	+0·46
557·50	+0·54
472·00	+0·49
372·00	+0·39
322·00	+0·41
272·00	+0·28
242·00	+0·18
222·00	+0·15
201·00	+0·11
167·00	+0·12
140·00	0·07 ?
114·50	0·08 ?
89·50	0·12 ?
70·50	0·03 ?
54·00	0·02 ?
48·00	+0·12
37·00	+0·14
29·00	+0·14
20·00	+0·18
14·00	+0·30
9·15	+0·46
6·55	+0·66
4·65	+1·00
3·15	+1·40
2·25	+1·48
1·15	+1·72
0·75	+1·70
0·65	+1·46
0·55	+1·04
0·35	+0·64
0·25	−0·60
0·15	−1·16
−0·05	−5·90

minimum between a tension of 50 millims. and 150 millims., then rises as the pressure diminishes, until, at a tension of 1·15 millim., the attraction is nearly four times what it was in dense air. Above this exhaustion the attraction suddenly drops and changes to repulsion, which at the best vacuum I could get was nearly thirteen times stronger than the attraction in air.

The last figure in the first column requires explanation. All the others are obtained by subtracting the height of the gauge from that of the barometer, and are *positive*. At the highest rarefactions, however, I get the gauge about 0·05 millim. above the barometer (85, *note*); the sign, therefore, becomes *negative*.

Table II. agrees in the main with Table I. The sign changes to repulsion at pressures corresponding to those queried in Table I.; the repulsion, though slight, was unmistakable. At 102 millims. pressure the observation has a positive sign. This looks like an error; but as it is so recorded in my notebook, and as I was at that time specially looking for repulsions, I do not feel justified in altering it. What I have called

TABLE II.

Tension of enclosed air, in millims. of mercury. Temp.=16° C. Bar.=772 millims.	Amplitude of half oscillation, in millims., at end of 40″ observation.
772·0	+0·460
770·0	+0·540
769·5	+0·570
769·0	+0·440
769·0	+0·520
769·0	+0·440
769·0	+0·450
565·0	+0·560
557·0	+0·540
472·0	+0·490
440·0	+0·550
369·0	+0·416
213·0	+0·233
207·0	+0·130
189·0	+0·180
173·0	+0·140
164·0	+0·100
162·0	−0·100
142·0	−0·120
132·0	−0·130
127·0	−0·090
105·0	−0·140
102·0	+0·083
73·0	−0·130
60·0	−0·123
56·0	−0·136
51·0	−0·030
41·0	+0·150
33·5	+0·170
32·0	+0·106
23·0	+0·110
22·0	+0·080
16·1	+0·170
16·0	+0·140
7·1	+0·380
6·0	+0·293
3·9	+0·610
1·9	+0·880
1·2	+0·755
0·9	+0·340
0·7	−0·740
0·6	−1·700
0·3	−3·800
0·2	−5·080
0·0	−5·680
−0·05	−6·320

the neutral point, or the point where attraction changes to repulsion, is in this series lower than in the former. There it occurred at a tension of about 0·3 millim. of mercury; here at about 0·8. Neither does the previous attraction attain such strength, although the ultimate repulsion is more intense. The agreement is, however, sufficiently satisfactory, considering the faulty method of measurement.

There are many errors almost inseparable from this form of apparatus. The making and breaking contact by hand is not sufficiently certain, and hesitation for a fraction of a second would seriously affect the ultimate amplitude of arc. I tried making and breaking by clockwork, also by a seconds' pendulum, but there were difficulties in each plan.

Owing to the mode of suspension, there was uncertainty as to the length of the pendulum. I tried to make it the right length to beat seconds *in vacuo.* Assuming that I had succeeded in this, the pendulum would have executed fewer vibrations in the 40 seconds when oscillating in air, and consequently I should not have got the full benefit from the making and breaking contact, supposing these were accurately timed to seconds.

The battery-power varied, being stronger at the commencement, and gradually declining towards the end of the experiment; and even were the battery to remain constant, the spiral became much hotter, owing to the removal of the air from the apparatus, ranging from a bright red heat in air to a full white heat *in vacuo.*

Owing to the height of the centre of suspension of the pendulum from the stand of the apparatus, the slightest deviation from the perpendicular made an appreciable difference in the distance of the weight from the spiral, and thereby increased or diminished the effect of radiation. Thus the tread of a person across the floor of the laboratory, or the passage of a cart along the street, would cause the image of the edge of the magnesium weight apparently to move from the cross wires in the telescope.

Many of these sources of error could have been removed; but in the mean time having devised a form of apparatus which seemed capable of giving much more accurate results, I ceased experimenting with the pendulum.

Before proceeding to describe the apparatus subsequently employed, I may mention that a candle-flame brought within a few inches of the magnesium weight, or its image focused on the weight and alternately obscured and exposed by a piece of card at intervals of one second, will soon set the pendulum in vibration when the vacuum is very good. A ray of sunlight allowed to fall once on the pendulum immediately sets it swinging. The pendulum-apparatus above described was exhibited, and experiments shown with it, at the Royal Society, April 22nd, 1874, and also before the Physical Society*, June 20th, 1874.

101. The difficulty which attended experiments with the balances and bulb-apparatus used at first was to bring the moving part accurately back to zero, and also to measure the deflection produced. I therefore tried several plans of giving a fixed zero-direction to the movable index. Thus a piece of magnetic oxide of iron was cemented to one end of the index, and a permanent magnet was brought near it. This answered pretty well, but was inconvenient, besides not being sufficiently accurate. A bifilar suspension from cocoon-fibres seemed likely to succeed better; but the difficulty of suspending the rod in this manner, so as to get exactly the same tension on each fibre, was very great, and unless this was done there was more tendency to move in one direction than in the

---

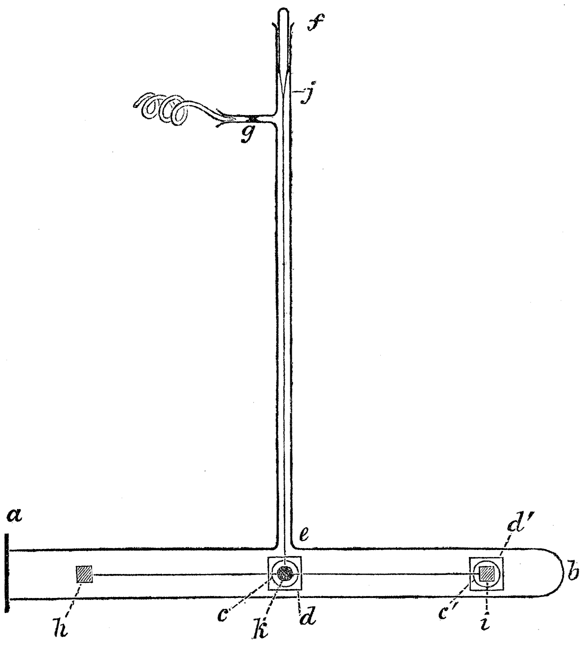
* Phil. Mag., August 1874.

other. When I had succeeded in suspending the needle with an equal tension on each silk fibre, I found their elasticity to vary; and as soon as the vacuum was approached one was sure to contract more than the other, twisting the needle out of the axis of the tube, and sometimes causing it to touch the side. This method of suspension was therefore abandoned.

By increasing the length of the needle, and also of the fibre used to suspend it, it was possible to employ fibres with a considerable amount of torsion, and still preserve the delicacy of the apparatus. Fine platinum wire was first tried; but this was soon abandoned in favour of glass fibres, which were found to answer so perfectly that I have since used nothing else.

102. Fig. 7 shows the form of apparatus which I have finally adopted, as combining the greatest delicacy with facility of obtaining accurate observations, and therefore of getting quantitative as well as qualitative results. It is a torsion-apparatus in which the beam moves in a horizontal plane, and may be called a horizontal torsion-balance. *a b* is a piece of thin glass tubing, sealed off at the end *b* and ground perfectly flat at the end *a*. In the centre a circular hole, *c*, is blown, and another one, *c'*, at the end; the edges of these holes are ground quite flat. *a*, *c*, and *c'* can therefore be sealed up by cementing flat transparent pieces of plate glass, quartz, or rock-salt, *a*, *d*, and *d'* on to them (83). To the centre of *a b* an upright tube, *e f*, is sealed, having an arm, *g*, blown on to it for the purpose of attaching the apparatus to the pump. *h i* is a glass index, drawn from circular or square (22) glass tube, and as light as possible consistent with the needful strength. A long piece of this tube is first drawn out before the blowpipe; and it is then calibrated with mercury until a piece is found having the same bore throughout; the necessary length is then cut from this portion. *j k* is a very fine glass fibre, cemented at *j* to a piece of glass rod, and terminating at *k* with a stirrup, cut from aluminium foil, in which the glass index, *h i*, rests. In front of the stirrup is a thin glass mirror, shown at *k*, silvered by LIEBIG's process, and either plane or concave as most convenient. At the ends of the glass index (*h i*) may be cemented any substance with which it is desired to experiment; for general observations I prefer to have these extremities of pith, as thin as possible, and exposing a surface of 10 millimetres square. The pith may be coated with lampblack or silver, or may retain its natural surface.

Fig. 7.



103. The preparation of the suspending thread of glass requires some care. It should be drawn from flint glass, as this gives much tougher threads than foreign glass. The diameter varies with the amount of torsion required; it may be 0·001 inch or less. I select the piece best adapted for the special experiment in the following way:—Several threads of glass are first drawn out before the blowpipe, and a certain number selected as being likely to answer the purpose. These are then suspended, side by side, to a horizontal rod and equalized as to length. A piece of glass rod, about 2 inches long, which is always kept for this purpose, is then cemented by shellac on to the end of one of the threads. Air-currents are then cut off by a glass screen, and the thread being set in movement by a slight twist, the torsion is measured by timing the oscillations. This having been done with each thread in succession, one is selected and mounted in the apparatus. If it works properly, well and good; if not, it is easy to select a thread having the requisite amount of torsion, more or less, and substitute it for the one first used.

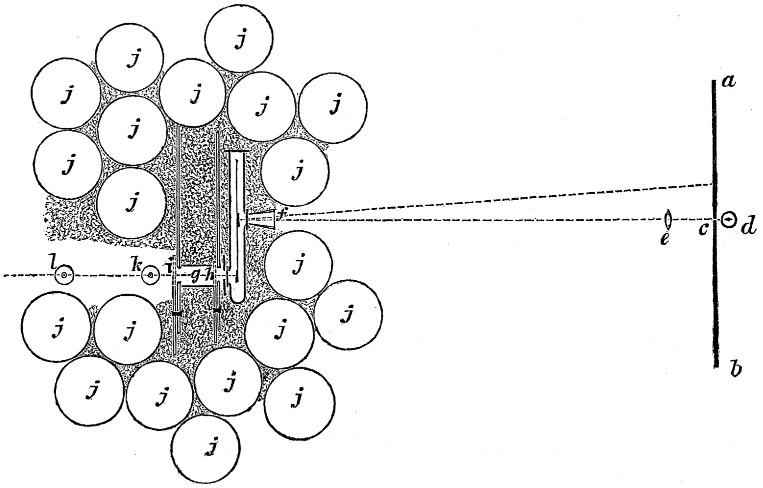
In fitting up one of these apparatus, threads were drawn out which were found to require respectively:— 44 seconds,
30 „
28 „
11 „
and $3\frac{1}{2}$ „
for a half oscillation when the glass weight was hung on to their ends. The one oscillating in 30 seconds was first used, but was found to give insufficient torsion. The one making half an oscillation in 11 seconds was then used, and was found to answer well. Before I adopted this plan days were frequently wasted in the attempt to hit upon a glass thread of the requisite degree of fineness.

104. In taking accurate observations with an apparatus of this description, it is necessary to support it on a stand firmly fastened to a main wall. When resting on a bench, or connected in any other way to the floor, there is a constant oscillation which keeps the index from zero.

The apparatus being fastened firmly to its stand, accurately levelled, and sealed on to the pump, a divided scale, *a b* (fig. 8), is placed four feet from the small mirror; and immediately beneath the scale is a narrow brass slit, *c*, illuminated by a lamp, *d*. In front is a lens, *e*, which throws the image of the slit on to the mirror, where it is reflected back again on to the divided scale. Here the angular movement of the bright line of light shows the minutest attractive or repulsive force acting on the pith at the extremity of the movable index.

In order to keep the luminous index accurately at zero, except when experiments are being tried, extreme precautions must be taken to keep all extraneous radiation from acting on the apparatus. A slightly conical paper tube, *f*, about 6 inches long, and as narrow as the angular movement of the ray of light will admit of, is cemented on to the glass window in front of the mirror; and a similar tube, *g*, is cemented on to the quartz window in front of the pith surface on which radiation is to act. The latter tube is furnished with card shutters, *h*, *i*, at each end, capable of easy movement up and down. The whole apparatus is then closely packed on all sides with a layer of cotton-wool, about 6 inches thick, and outside this is arranged a double row of Winchester quart bottles, *j*, *j*, filled with water and covered with brown paper, spaces being only left in front of the paper tubes. *k* and *l* represent the positions of the candle 140 and 280 millims. distant from the pith. The whole arrangement has the appearance shown in fig. 8.

Fig. 8.

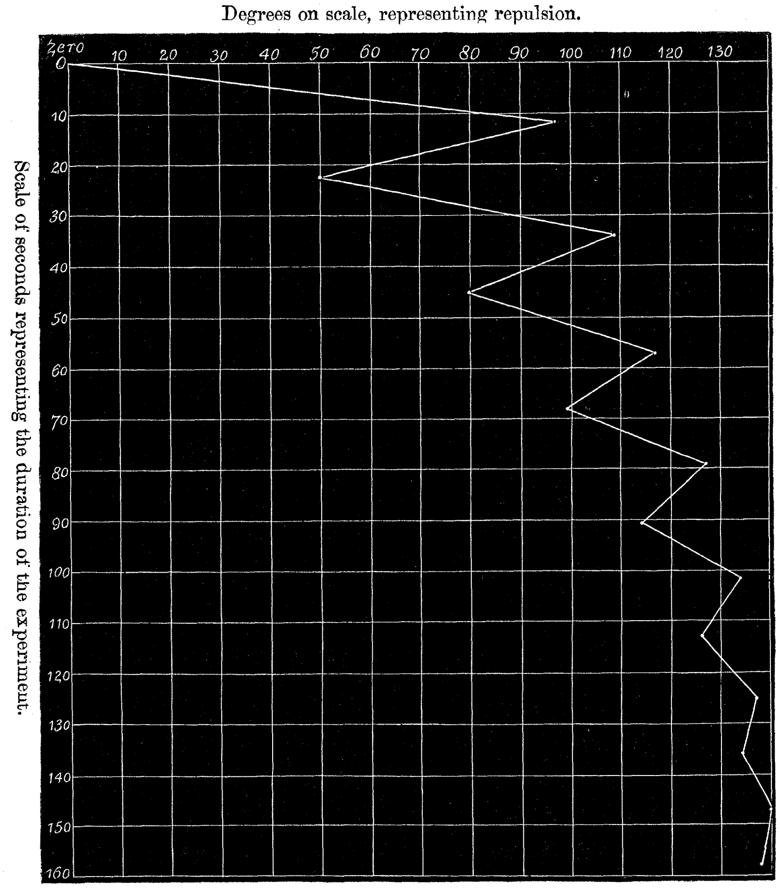


105. I will not discuss at present the phenomena presented when the apparatus is full of air, or when the vacuum is imperfect, but will proceed to the effects observed when the exhaustion has been pushed to the highest attainable degree. However much the results may vary when the vacuum is imperfect, or when the apparatus is full of air, I always find them agree amongst themselves when the residual gas is reduced to the minimum possible; and I have also ascertained that it is of no consequence what this residual gas is. Thus I have started with the apparatus filled with various vapours and gases, such as air, carbonic acid, water, iodine, hydrogen, or ammonia; and at the highest rarefaction I find no difference in the results which can be traced to the residual vapour, assuming any to be present. A hydrogen vacuum seems neither more nor less favourable to the phenomena than does a water or an iodine vacuum. If moisture be present to begin with, it is necessary to allow the vapour to be absorbed by the sulphuric acid of the pump, and to continue the exhaustion with repeated warming of the apparatus until the aqueous vapour is removed; then only do I get the best results. When pith surfaces are used at the extremities of the glass beam, they should be perfectly dry; and they are more sensitive if the apparatus has held a vacuum for some weeks, as the residual moisture in the pith will then have been absorbed by the sulphuric acid in the pump.

106. It was found that when a source of light and heat is suddenly allowed to shine on the pith surface and not removed, a deflection rapidly takes place, attaining its

maximum in about 11 seconds; the spot of light now returns a few degrees, and then proceeds in the first direction to a greater extent than at first. So it goes on, by alternate steps, advancing a little each oscillation, until, if the light be feeble, the index takes up a nearly fixed position; if, however, the light be strong, the beam is driven against the side of the tube. In illustration of this I select the following series of observations from a large number recorded in my note-book. The horizontal figures represent the degrees on the scale, starting from zero, where the spot of light normally rests. The vertical figures represent the seconds during which the experiment lasted. The zigzag line represents the oscillations of the spot of light, and shows the movement of the pith surface under the influence of a uniform source of radiation. The time was recorded by a chronograph. Starting from zero the spot of light is seen to have travelled to 97° in 11·5 seconds; at the end of 11 more seconds, or 22·5 seconds altogether, it had come back to 50°; at the end of 34 seconds the light had advanced again to 109°, and so on. The movements are tolerably uniform as to time, taking about 11·5 seconds for the half oscillation, but the amplitude of vibration is continually diminishing.

Fig. 9.



107. If, however, the light is only allowed to shine on the pith surface for 11·5 seconds (or for as long as the spot of light takes to perform its first half oscillation), and if it is then instantly cut off, the spot of light almost invariably returns to zero and stops there,

instead of swinging to the opposite side and only returning to rest after ten or a dozen oscillations, as is the case when the beam is set vibrating by mechanical means. This behaviour points to the return movement taking place under the influence of a force which remains active after the original radiation is cut off, and which is only gradually dissipated. This force is most probably from the heat which the pith has absorbed raising its temperature; and the steady return to zero seems to be due to the movement being controlled by the radiation of heat by the pith.

108. A series of observations taken with another apparatus, with the object of ascertaining the times of oscillation to and fro, showed that the first half, or the maximum deviation produced, whilst under the influence of radiation, occupied about the same time as the second half, or the return swing, when the source of radiation was cut off. The following are the observations. The source of radiation was a candle, the intensity of action being moderated by filtering the rays through glass screens.

Half oscillation, under influence of radiation.	Whole oscillation, radiation being cut off during the return swing.
8 seconds.	15 seconds.
7·5 "	15 "
7·5 "	14·5 "
7·5 "	15·5 "
7·5 "	14·5 "
7·25 "	15 "
7·5 "	15 "
7·5 "	15 "
7 "	14 "
7 "	14 "
6·75 "	14 "
7 "	14 "
7·25 "	15 "
7 "	14 "
7 "	13·25 "
8 "	16 "
8 "	16 "
7·5 "	15 "
7 "	15 "
8 "	15 "
8·5 "	15·5 "
7·5 "	15 "
8 "	15 "
8 "	15 "
7 "	14 "
Mean...7·47 "	Mean...14·77 "

The average time of the first half oscillation is therefore 7·47 seconds[*], and of the second half 7·3 seconds. This small difference is not unlikely to be due to errors of observation.

After a long series of experiments the zero gradually creeps up, showing that one side of the apparatus is becoming warmed. The conducting-power for heat and

---

[*] By referring to paragraphs 106 and 107 it will be seen that I have put the time of the first half oscillation as 11·5 seconds. This was with another apparatus, having a glass thread of different torsion.

condition of the surface (whether coated with lampblack or consisting of polished metal) of the body on which radiation falls materially influence the movements.

109. The accompanying Table gives the results of numerous experiments as to the effect of screens, tried with an exceedingly delicate apparatus, constructed as above described, the window, c' (fig. 7), being of quartz. The candle used was the kind employed in gas photometry, and defined by Act of Parliament as a "sperm candle of 6 to the pound, burning at the rate of 120 grains per hour." The distances were taken from the front surface of the pith when the luminous index stood at zero. They were in the proportion of 1 to 2 (140 to 280 millims.), to enable me to see if the action would follow the law of inverse squares and be four times as great at the half distance. No such proportion can, however, be seen in the results, the radiant source possibly being too close to allow the rays to fall as if from a point. The figures given are the means of a great many fairly concordant observations. Where a dash rule is put I have tried no experiment. The cipher 0° shows that experiments were actually tried, but with no result.

Interposed sceen.	Magnesium wire, burnt for 7·5 seconds, distant 140 millims.	Standard candle, distant 140 millims.	Standard candle, distant 280 millims.	Copper ball, 400° C., distant 140 millims.	Copper ball, 400° C., distant 280 millims.	Copper ball, 100° C., distant 140 millims.
None	° ——	° ——	54°	° ——	180°	9°
Rock-salt, 20 millims. thick, not very clear	——	148	52	220	——	6
Rock-crystal, in two pieces, 42 millims. thick altogether	——	88	32	115	——	1·5
Talc, clear but very dark, 1·25 millim. thick	——	100	28	90	——	2
Plate glass, white, 2 millims. thick, one piece	——	——	——	——	——	3·25
Ditto, two pieces	——	——	——	110	——	1·75
Ditto, three pieces	——	72	24	76	23	0·62
Ditto, two pieces, enclosing 8 millims. water	——	——	——	0	0	0
Plate glass, of a greenish colour, 10·5 millims. thick	——	55	17	——	20	0
Ditto, 20 millims. thick	——	——	8	——	——	0
Alum, a clear plate, 5 millims. thick	——	18·5	3	——	0	0
Plate glass, slightly greenish, 40½ millims., and clear alum plate, 8½ millims. thick.	30	0	0	0	0	0
Calc spar, 27 millims. thick	——	——	——	78	——	——
Very thin film of mica	——	——	off the scale.	——	——	8
Ammonio-sulphate of copper, 8 millims. thickness of solution, opaque to rays less refrangible than line F	72	7	——	0	0	0
Ditto, stronger solution, opaque below G	29	3	——	0	0	0

The sensitiveness of my apparatus to heat-rays appears to be greater than that of any ordinary thermopile and galvanometer. Thus I can detect no current in the thermopile when obscure rays from copper at 100° C. fall on it through glass; and MELLONI gives a similar result.

110. An examination of this Table shows that the action is by no means confined to the rays usually called heat, *i.e.* to the extreme- and ultra-red of the spectrum. The strong action obtained when the light is filtered through greenish glass and alum, or through ammonio-sulphate of copper, shows that luminous rays produce a similar movement of repulsion.

Unfavourable weather has prevented me from obtaining good quantitative results with the different rays of the solar spectrum; but I have tried numerous qualitative experiments which leave no doubt on my mind that any ray, from the invisible ultra-red to the invisible ultra-violet, will produce repulsion in a vacuum. The following is an experiment tried with the electric light. The spectrum was formed with a complete quartz train, no glass whatever being in the path of the rays. The purity of the spectrum was evidenced by the fact of the lines being sharp when thallium, sodium, or lithium was put between the carbon poles. The spectrum was so arranged that any desired ray could be thrown on to a lampblacked pith surface, screens being interposed to cut off the action when desired. The torsion-balance was similar to the one used in the last-named series of experiments (104), but was not quite so sensitive.

The extreme-red rays were first brought into position. On removing the screen the luminous index moved 9 divisions on the scale. The screen being replaced, the index returned to zero. A solution of iodine in disulphide of carbon was now interposed, and the screen again removed. The repulsion was almost as strong as before, showing that this liquid was transparent to the ultra-red rays.

The iodine solution was then replaced by a clear plate of alum 5 millims. thick, and the screen removed; a very slight movement only took place. The iodine solution was then put in front of the alum plate, so as to subject the extreme-red rays to a double process of sifting. No trace of action could be detected.

Whilst this double screen was in front of the pith disk, the spectrum was gradually passed along, so as to bring the rays, one after the other, into position. No effect, however, was produced, showing that alum and iodine solution practically obliterate the whole of the spectrum.

The alum plate and iodine-cell were now removed, and the green of the spectrum (the thallium line) was brought into position. The luminous index moved 6 divisions. The plate of alum cut off only a small amount of this action, but the iodine-cell brought the index to zero. This is a proof that the action in this case was not due to the heat-rays of the spectrum, for these are practically transmitted by iodine, and cut off by alum.

The indigo-rays were next brought into position. The spot of light moved three divisions on the graduated scale. Alum cut off only a very little of the action; but the iodine-cell was completely opaque to the rays, and brought the index to zero.

Finally, the invisible ultra-violet rays of the spectrum were brought into position. The train being of quartz these were abundant. Care was taken to keep any of the luminous rays away from the pith disk. I think I succeeded in this; but it was not easy, owing to the fluorescence of the card and other surfaces on which stray rays fell. The spot of light moved two divisions, which were increased to five when the invisible rays were further concentrated by a quartz lens. The interposition of the iodine-cell cut off the whole of the action. The alum plate cut off about half of the action, but scarcely more than would have been cut off had a piece of colourless glass of the same thickness been interposed, and it must be remembered that the alum plate has glass and Canada balsam on each side.

111. A similar experiment with the solar spectrum gave the following deflections, glass prisms being used:—

	°
Ultra-red	2
Extreme red	6
Orange	5
Green	4·5
Indigo	3·5
Ultra-violet	2

Although I give the number of divisions shown by the luminous index, I attach little importance to them as quantitative measurements. They are only single observations, and were taken before I had succeeded in getting any thing like the same sensitiveness I can now attain in the apparatus. As illustrations of the fact, however, that the more refrangible rays of the spectrum act as well as the lower rays, they may be taken as trustworthy*.

112. In my former paper on this subject I have already mentioned in detail that at a certain point of rarefaction there is neither attraction nor repulsion when radiation falls on the movable index (30, 43, 47, 66). I have long tried to ascertain the law governing the position of this neutral point. My results are not yet ready for publication; but they are shaping themselves in order, and will, I trust, lead to a true explanation of the cause of these phenomena.

The barometric position of the neutral point dividing attraction from repulsion varies according to circumstances; among these may be mentioned the density of the substance on which radiation falls, the ratio of its mass to its surface, its radiating- and conducting-power for heat, the physical condition of its surface, the kind of gas filling the apparatus, the intensity of radiation, and the temperature of the surrounding atmosphere.

When the surface exposed to radiation is pith, the neutral point is somewhat low. I have had it vary between 50 millims. and 7 millims (30) below a vacuum. It is, however, impossible to ascertain exactly; for a point of rarefaction can be obtained at which the warm fingers repel, and incandescent platinum attracts. With a heavy metal in the form of a sphere, so as to expose the smallest surface in proportion to the mass, I

---

* Every thing is ready to try a series of experiments with the solar spectrum, as soon as sunshine is available. The results shall be communicated in a subsequent paper.

have not attained the neutral point until the exhaustion was within a very small fraction of a millimetre (43, 47); whilst if the metal is in the form of thin foil the neutral point may easily be got lower than with pith.

I am inclined to believe that the true action of radiation is repulsion at any pressure, and that the attraction observed when the rarefaction is below the neutral point is caused by some modifying circumstance connected with the surrounding gas, not necessarily of the nature of air-currents (80). As a proof of this I have not unfrequently obtained repulsion from radiation when the apparatus was full of air at the normal pressure.

113. The following experiments are too few in number, and have not been varied sufficiently as to conditions, to enable many inferences to be drawn from them. However, they afford glimpses of a law governing the position of the neutral point.

A torsion-apparatus was fitted up similar to the one described in paragraph 102. The beam was of glass, and at one extremity was fitted with a spring clip, also of glass, so that different bodies could be experimented with. Disks of platinum foil, 1 centimetre in diameter and weighing 1·28 grain each, were prepared, and they were fixed in the clip at the end of the torsion-beam, either singly or two, three, or four together, in such a manner that while the disk exposed was always 1 centim. in diameter, the weights should be in the proportion 1, 2, 3, 4. At the other end of the beam a movable counterpoise was arranged, so that the length of beam from the platinum disk to the centre was always the same.

The neutral points were as follows:—

No. of disks.	Barometer.	Gauge.	Diff. = Neutral point.	Differences.
1.	760	682	78	8
2.	760	690	70	16
3.	760	706	54	24
4.	760	730	30	

114. Two pieces of platinum, *a* and *b*, were now cut from the same sheet, each having 1 square centim. of surface. *a* was left the full size, but *b* was carefully folded in four, so as to expose a surface of only a $\frac{1}{4}$ of a square centimetre, the weight remaining the same. The neutral points were then taken. The average of several observations (which, however, were not quite so concordant as could have been wished) were, below a vacuum,

a.	*b.*
136 millims.	70 millims.

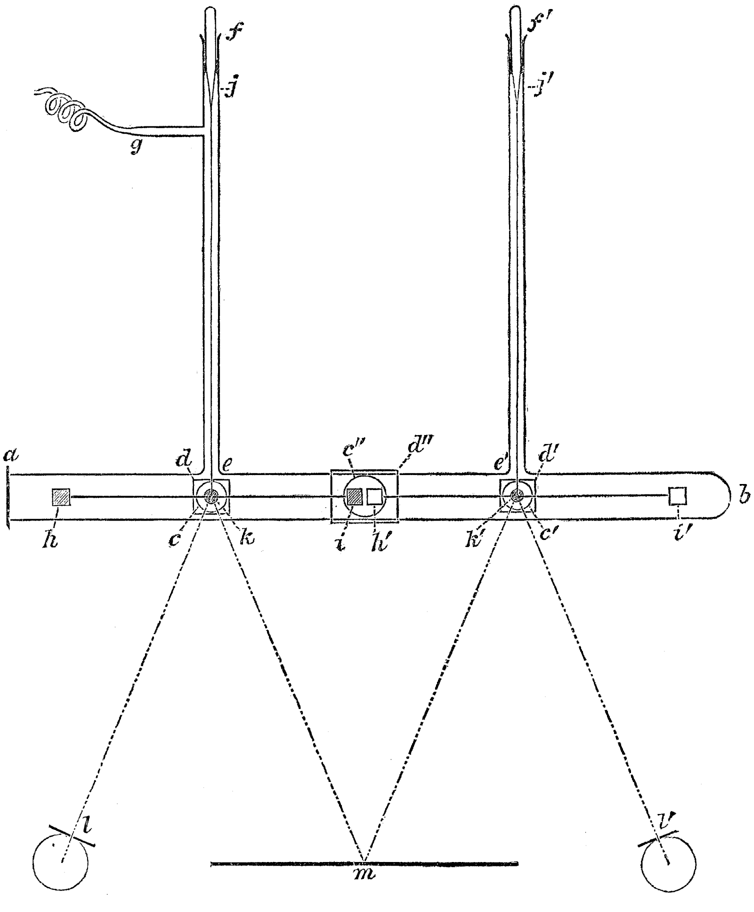
The pieces of foil were then coated with lampblack, and observations again taken. This time the neutral points came out—

a.	*b.*
66 millims.	124 millims.

An intimate connexion is thus shown to exist between the absorbing (and radiating) power of the surface on which radiation falls and the atmospheric tension at which the movement is reduced to a minimum. Further experiments on this subject are in progress.

115. It has already been said that when radiation falls on a thin surface of pith, the neutral point is low, whilst with a moderately thick piece of platinum it is generally high. I have constructed a double torsion-apparatus by means of which these actions can be easily studied. Fig. 10 shows the arrangement of apparatus. It consists of a torsion-apparatus constructed as the one shown in fig. 7 (102), with the exception of the arms being double. Similar parts in each drawing are shown by similar letters. $a\,b$ is a piece of thin glass tubing, sealed off at the end b, and ground perfectly flat at the end a. In the centre a circular hole (c'') is blown, and two others are blown at the parts c and c'; the edges of these holes are also ground perfectly flat. a, c, c', and c'' can therefore be sealed up by cementing flat transparent plates of glass, quartz, rock-salt, &c., a, d, d', d'' on to them. At right angles to $a\,b$, and at the parts e, e', upright

Fig. 10.



tubes, $f\,e$, $f'\,e'$, are sealed, one of them having an arm (g) blown into it for the purpose of attaching the apparatus to the pump. h, i, h', i' are glass beams made as light as possible consistent with the necessary stiffness. $j\,k$, $j'\,k'$ are glass fibres (103) cemented at j, j' to pieces of glass rod, and terminating at k, k' with a stirrup cut from aluminium foil, in which the glass beams h, i, h', i' rest. In front of these stirrups are thin glass mirrors (k, k'). At the ends of the beam (h, i) are cemented very thin pieces of blackened pith, each 1 centim. square; and at the ends of the other beam (h', i') are cemented pieces of platinum foil, also 1 centimetre square. At l and l' are narrow slits, with lamps behind them, so arranged that they send their rays of light respectively on to the mirrors (k, k'), whence they are reflected back to the divided scale m. When the torsion-beams are not acted on by any force, the rays of light both meet at zero (m), and there overlap, the slightest movement of either beam causing them to separate.

When the apparatus is full of air, a beam of radiation sufficiently wide to cover the whole window (c'') being thrown upon the plates i, h', the latter are instantly attracted, as shown by the movement of the reflected rays of light ($k\,m$, $k'\,m$). On exhausting the tube, and trying the effect of a hot body at the central window from time to time, it is seen that the movement of the pith surface (i) gradually diminishes, until at a certain point of exhaustion (in this apparatus at about 50 millims. below a vacuum) the neutral point for pith is obtained. On increasing the rarefaction the pith is repelled by radiation, the platinum continuing to be attracted. On exhausting the air still further (to about 28 millims.) the neutral point for the platinum surface is obtained, higher rarefactions producing repulsion of each when radiation falls on the pith and platinum surfaces (i, h').

At a rarefaction intermediate between the neutral point for pith (50 millims.) and the neutral point for platinum (28 millims.), the curious effect is produced of the same beam of radiation thrown into the window (c'') producing repulsion of the pith and attraction of the platinum, the two rays of light ($k\,m$, $k'\,m$) each moving to the right, and, if a piece of ice is presented to the central window, to the left. By adjusting the internal tension of the apparatus, a point may be reached (about 40 millims. below a vacuum) at which the repulsion of pith and the attraction of platinum are exactly equal, and then the two rays meeting at m do not separate, but together move to the right or left.

116. A series of experiments have been tried with a view to ascertain what influence the state of surface of the substance submitted to radiation has on the amount or the direction of its movement. A torsion-apparatus was prepared similar to the one shown in fig. 7 (102), and having a thin disk of ivory at each end. One was coated with lampblack, whilst the other retained its white polished surface. The average of a number of experiments showed that, under the influence of the same source of radiation acting for the same time (15 seconds), the white ivory was repelled so as to send the luminous index 41·5 divisions of the scale, whilst the blackened ivory caused the index to

move 46·8 divisions. These experiments were, however, tried in 1873*, when I had not succeeded in getting any thing like the delicacy I now obtain in the apparatus; and I propose to repeat them under varied conditions before employing the results to found any arguments upon.

117. In my former paper on this subject (74, 75, 76, 77, 78) I have discussed various explanations which may be given of attraction and repulsion resulting from radiation; and in a lecture delivered before the Physical Society† I entered more fully into the same arguments. The most obvious explanation is that the movements are due to the currents formed in the residual gas, which, theoretically, must be present to some extent even in those vacua which are most nearly absolute.

Another possible explanation is that the movements are due to electricity developed on the moving body, or on the glass apparatus, by the incident radiation.

A third explanation has been put forward by Professor OSBORNE REYNOLDS, in a paper which was read before the Royal Society on June 18th, 1874. Referring to the results of my experiments, Professor REYNOLDS says that it is the object of his paper to prove that these effects are the result of evaporation and condensation. In my exhausted tubes he assumes the presence of aqueous vapour, and then argues as follows:—"When the radiated heat from the lamp falls on the pith, its temperature will rise, and any moisture on it will begin to evaporate and to drive the pith from the lamp. The evaporation will be greatest on that ball which is nearest to the lamp; therefore this ball will be driven away until the force on the other becomes equal, after which the balls will come to rest, unless momentum carries them further. On the other hand, when a piece of ice is brought near, the temperature of the pith will be reduced, and it will condense the vapour and be drawn towards the ice."

It is not my intention to recapitulate the arguments I have already brought forward against these three explanations. They are all fully given in my above-quoted lecture before the Physical Society. I shall, however, adduce a few experiments which have been devised specially with the view of putting one or other of these theories to the test. In giving what I conceive to be reasonable arguments against the explanations which have already been proposed, I do not, however, wish to insist upon any theory of my own to take their place. *Any* theory will account for *some* facts; but only the true explanation will satisfy *all* the conditions of the problem, and this cannot be said of either of the theories I have already discussed.

118. The pendulum-apparatus, described and figured in paragraph 99, was specially devised to bear upon the air-current and the electrical theory. On referring to the description of the experiments tried with it (Tables I. & II.), it is seen that in air the ignited spiral produced attraction, whilst in a vacuum the same source of radiation gave

---

[*] The torsion-apparatus with ivory terminals was exhibited in action at the Meeting of the Royal Society, Dec. 11th, 1873.

[†] June 20, 1874 (Phil. Mag., August 1874).

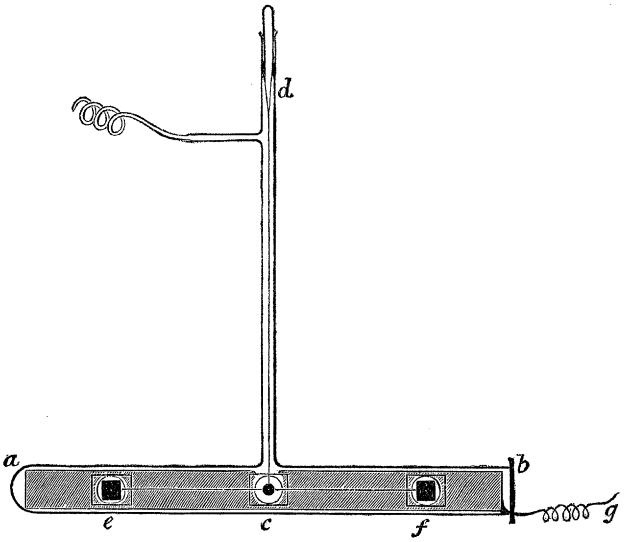
strong repulsion. Now the effect of raising a platinum spiral to whiteness in the air would be to rarefy the air all round, and the suddenness of its ignition would cause the air to be driven from it, as a centre, on all sides. Hence I was prepared to find that the pendulum would be mechanically blown on one side by what might be likened to a miniature explosion of heated gas. But the action was always one of attraction, whilst, when there was no air at all present to be expanded and driven away by the hot platinum, the action was one of violent repulsion. A possible explanation of the attraction in air in this experiment may be given by assuming that the pendulum was driven inwards by the rush of cold air supplying the place of that rising upwards from the hot spiral; but it is not likely that this action should so completely overcome the effect of expansive action; and, moreover, it will only account for half the phenomenon (that in air), and leaves the still stronger action in a vacuum entirely unexplained.

119. I have tried special experiments to put the air-current theory to a decisive test. Bulb-tubes (84) and torsion-apparatus (102) have been prepared, containing terminals of metal, ivory, glass, mica, or pith, in the form of thin flat surfaces. These surfaces have been placed at an angle with the plane passing through the index and suspending-thread, in such a manner that the action of heat would be to cause currents and drive them round like the vane of a windmill. I, however, found the action of heat *in vacuo* to be repulsion, and in air to be attraction; and the latter was even strong enough to overcome the action of the air-currents, which could not fail to be developed under the circumstances of the experiment.

120. The pendulum-apparatus has also been used to show that electricity is not the cause of the attraction and repulsion. On referring to the description (99), it is seen that the mass of magnesium forming the weight was in metallic contact with the platinum wire which supported it, and that the upper end of this platinum wire was fused into the glass tube, and passed thence to the outside. With this I have tried numerous experiments bearing on the action of electricity. I have connected the projecting end of the platinum wire with "earth," with either pole of an induction-coil (the other pole being more or less insulated), with either pole of a voltaic battery, and with a delicate electroscope; I have charged it with an electrophorus, and have submitted it to the most varied electrical conditions; and still, on allowing radiation to fall on the suspended mass, I invariably obtain attraction in air and repulsion in a vacuum. The heat has been applied from the outside, so as to pass through the glass, and also inside by means of the ignited spiral; and the results show no difference in kind, but only in degree, under electrical excitement. I often obtain troublesome electrical interference with the usual phenomena, but never of such a character as would lead me to imagine that the normal results were due to electricity. I also obtain the normal actions whether the apparatus has been standing insulated in the air, or whether it has been completely immersed in water connected electrically with "earth" or surrounded with wet blotting-paper.

121. The following experiment was suggested by Mr. CROMWELL F. VARLEY, F.R.S., who informs me that he considers the results conclusive against the electrical theory. A torsion-apparatus was prepared, as shown in fig. 11. The inside of the tube (*a b*) is lined with a cylinder of copper gauze, having holes cut in the centre (*c*) for the passage of the supporting-thread (*d c*) and the index ray of light, and holes at each end to admit of the plates (*e*, *f*) being experimented with. A hole drilled in the plate (*b*) allows a wire to pass from the copper gauze to the outside, so as to give me electrical access to the gauze lining. Under the most diverse electrical conditions, whether insulated or connected with "earth," this apparatus behaves normally when heated; neither can I detect any electricity when the plate *e* or *f* is under the influence of radiation if I connect the wire *g* with a delicate electroscope. In experimenting with this apparatus I have also completely immersed it in liquids, such as water, solutions of metallic salts, ether, disulphide of carbon, &c. The heat has been applied in these cases by introducing a glass bulb containing water at different temperatures and a thermometer (28). Under all these varied circumstances the movements took place in the regular manner, and no electrical action whatever could be detected.

Fig. 11.



122. I have already discussed Professor OSBORNE REYNOLDS'S theory of evaporation and condensation somewhat fully in the already quoted Physical Society paper*. I will, however, describe the following experiments, which I think prove that Professor REYNOLDS has not suggested a theory which accounts for all the facts of the case, and therefore has not hit upon the true explanation.

A thick and strong bulb was blown at the end of a piece of very difficultly fusible green glass, specially made for steam-boiler gauges. In it was supported a thin bar of aluminium at the end of a long platinum wire. The upper end of the wire was passed through the top of the tube and well sealed in, for electrical purposes (120). The apparatus was sealed by fusion to the Sprengel pump, and exhaustion was kept going on for two days, until an induction-spark refused to pass across the vacuum. During this time the bulb and its contents were several times raised to a dull red heat. At the end of two days' exhaustion the tube was found to behave in the same manner

---

* *Loc. cit.*; also Chemical News, July 17, 1874.

as, but in a stronger degree than, it would in a less perfectly exhausted apparatus, viz. it was repelled by light and heat of low intensity and attracted by cold.

A similar experiment was next tried, only water was placed in the bulb before exhaustion. The water was then boiled away *in vacuo*, and the exhaustion continued, with frequent heating of the apparatus to dull redness, for about forty-eight hours. At the end of this time the bar of aluminium was found to behave exactly the same as the one in the former experiment, being repelled by radiation.

Similar experiments, attended with similar results, were tried with a platinum and with a glass index; and instead of water, iodine has been put into the bulb to begin with.

It is impossible to conceive that in these experiments sufficient condensable gas or vapour was present to produce the effects Professor OSBORNE REYNOLDS ascribes to it. After the repeated heating to redness at the highest attainable exhaustion, it is difficult to imagine that sufficient vapour or gas should condense on the movable index to be instantly driven off by a ray of light or even the warmth of the finger with recoil enough to drive backwards a heavy piece of metal.

123. It seems to me that a strong argument against Professor REYNOLDS'S theory (and also against the electrical and air-current theories) may be drawn from the fact that the repulsion in a vacuum is not confined to those red and ultra-red rays of the spectrum which mainly produce dilatation of mercury in a thermometer, excite an electrical current between antimony and bismuth couples, and cause a sensation of warmth when falling on the skin, but that any ray from the ultra-red to the ultra-violet will produce a similar effect. It cannot be reasonably argued that a ray of light, filtered through plates of glass and alum (109), can instantly vaporize a film of moisture or condensable gas from a surface on which it is caused to shine, or that it can produce air-currents in the almost perfect vacuum surrounding the surface shone upon, or that it will produce electrical excitement on such a surface.

124. Facts tested and verified by numerous experiments, but scarcely more than touched upon in the present paper, are, I think, gradually shaping themselves in order, in my mind, and will, I hope, aid me in evolving a theory which will account for all the phenomena. But I wish to avoid giving any theory on the subject until I have accumulated a sufficient number of these facts. The facts will then tell their own tale; the conditions under which they invariably occur will give the laws; and the theory will follow without much difficulty. In the eloquent words of Sir HUMPHRY DAVY, "When I consider the variety of theories which may be formed on the slender foundation of one or two facts, I am convinced that it is the business of the true philosopher to avoid them altogether. It is more laborious to accumulate facts than to reason concerning them; but one good experiment is of more value than the ingenuity of a brain like NEWTON'S."