XV. *On Attraction and Repulsion resulting from Radiation* *.

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

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1. In a paper "On the Atomic Weight of Thallium," presented to the Royal Society June 18, 1872, after describing a balance with which I was enabled to perform weighings of apparatus &c. in a vacuum, I noted a peculiarity in relation to the effect of heat in diminishing the apparent weight of bodies. I said, "That a hot body should appear to be lighter than a cold one has been considered as arising from the film of air or aqueous vapour condensed upon or adhering to the surface of the colder body, or from the upward currents of air caused by the expansion of the atmosphere in the vicinity of the heated body. But neither hypothesis can be held when the variation of the force of gravitation occurs in a vacuum as perfect as the mercurial gauge will register, and under other conditions which I am now supplying, and which I purpose embodying in a paper to be submitted to the Royal Society during a subsequent session" †.

With the vacuum-balance mentioned above I carried out many experiments, but was unable to obtain results which were at all concordant; and it was soon found necessary to investigate the phenomena with smaller and less complicated apparatus.

2. Most chemical manuals warn beginners against the errors occasioned by weighing substances while hot; and, up to a moderately high degree of exhaustion, I was prepared to find a piece of glass apparatus, when hot, apparently lighter than the weights which should balance it were the whole system at the same temperature. But instead of the interfering causes diminishing as the rarefaction proceeded, they seemed rather to increase, or at all events to become irregular in their action, sometimes appearing to oppose, and at others to supplement the force of gravity. In such a vacuum as a good air-pump would produce, the actions of the ascending current of air and of the adhering film, it might be presumed, should cease to exert an influence; and I could think of no other disturbing cause except the lengthening of the beam, owing to the heat radiated from the apparatus below it. An increase in the length of the beam should make a mass suspended at its extremity appear heavier; but whilst I frequently noticed an action which might be due to this cause, I occasionally obtained results which were so anomalous as to convince me that some cause which I had not hitherto recognized was at work (49), and to lead me to hope that perhaps I might succeed in tracing a connexion between heat and the force of gravity.

* This paper was read before the Royal Society, and the abstract was published in the 'Proceedings' (vol. xxii. p. 37) under the title "On the Action of Heat on Gravitating Masses."

† Philosophical Transactions, 1873, vol. clxiii. p. 287.

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3. Many physicists have worked on the subject of repulsion by heat. I give here a brief *résumé* of the state of knowledge on this subject up to the time of my commencing these experiments, premising, however, that much of this historical information was unknown to me until some of the experiments here recorded were finished and I commenced putting my notes together. The earliest mention I can find is by the Rev. A. BENNET, F.R.S., who in the year 1792 published a paper* on "A new suspension of the magnetic needle intended for the discovery of minute quantities of magnetic attraction: also an air-vane of great sensibility; with new experiments on the magnetism of iron filings and brass." Mr. BENNET used a spider's thread as a means of suspension. This he found by experiment to be absolutely free from torsion. I quote the following experiments from his paper:—

"Experiment IV. A bristle was suspended horizontally by a spider's thread, somewhat stronger than the last, and after turning the wheel till it produced 4800 revolutions, it shortened the thread from 3 inches to 1 inch; yet either end of the bristle would move towards any warm substance which was presented to it either with or against the direction of the twist†.

"Experiment V. Several other light substances were suspended by fine spiders' threads and placed in a cylindrical glass about 2 inches in diameter, as the thinnest part of the wing of a dragon-fly, thistledown, and the down of dandelion; of these the last appeared most sensible to the influence of heat; for when this down was fastened to one end of a fine gold wire, suspended horizontally on to one end of two bits of straw joined together in the form of the letter T inverted, it would turn towards any person who approached it at the distance of 3 feet, and would move so rapidly towards wires heated by my hand, as very much to resemble magnetic attraction.

"Experiment VI. A bottle filled with cold water was brought near the glass cylinder standing in a warm room, and soon after the down of dandelion appeared to be repelled by the bottle by turning away from it. The bottle was removed to the other side, and the dandelion again moved towards the opposite side.

"Experiment VII. A piece of paper was tied over the mouth of a glass jar, about 4 inches in diameter. Two holes were made in the paper opposite to each other, and near the edge of the glass. The jar was placed upon a table, and suffered to stand a considerable time to cool in a room without fire. I then sat near it on the side where one of the holes in the paper was in the nearer and the other in the farther end of the diameter. I next filled another glass with smoke, and placed it with its mouth over the two holes in the paper. The smoke was now seen to descend through the farthest hole, and mixing with the air in the lower jar, plainly showed that the air moved slowly towards the side of the glass warmed by the heat of my body.

"Experiment X. To the end of a fine gold wire 3 inches long, and suspended by a spider's thread in a cylindrical glass, was fastened a small circular bit of writing-paper;

* Philosophical Transactions, 1792, p. 81.

† For a rediscovery of this fact, seventy-nine years after, see par. 14.

light was admitted through a small hole, and also the focus of a large lens was thrown upon the paper, with the intention of observing whether it would be moved by the impulse of light; but though these experiments were often repeated, and once with the paper suspended in an exhausted receiver, yet I could not perceive any motion distinguishable from the effects of heat. Perhaps sensible heat and light may not be caused by the influx or rectilinear projection of fine particles, but by the vibrations made in the universally diffused *caloric* or matter of heat or fluid of light. I think modern discoveries, especially those of electricity, favour the latter hypothesis."

4. In his 'Elementary Treatise on Heat'* Professor BALFOUR STEWART, F.R.S., cites this experiment of Mr. BENNET'S as one of the arguments against the emissive and in favour of the undulatory theory of light and heat. Bearing in mind the overwhelming proofs we now possess that the undulatory theory more nearly expresses the truth than does the emissive theory, it is not likely that the very different results I have succeeded in obtaining (56, 57, 58), by the employment of instruments of a delicacy unattainable eighty years ago, will have any weight in modifying the accepted theories of light and heat.

5. The next mention of the dynamic action of heat is by LAPLACE, who, in his 'Mécanique Céleste'†, speaks of the "repulsive force of heat" as subsisting among the particles of a fluid, but observes that experiment shows it has no other effect on capillary attraction than what results from its diminishing the density of the fluid.

6. In the year 1824 LIBRI‡ published some experiments on the movement of translation experienced by a drop of liquid suspended to a metallic wire, one of the ends of which is heated. This he inferred was due to repulsion produced by the heat between the wire and the particles of the liquid. The Rev. BADEN POWELL, F.R.S., says § that trying to repeat LIBRI'S experiment he has never been able to succeed, except in producing a slight apparent motion in the drop, which seems explicable from the mere effect of evaporation on the side next the heat.

7. In the 'Annales de Chimie et de Physique' for 1825 ‖ are two papers by FRESNEL, in which he gives an account of an experiment on the repulsion exerted by heat. To the two extremities of a fine magnetic needle, suspended by a cocoon-fibre, he attached vertical disks of foil and mica, so as to test with the same apparatus an opaque and a transparent body. The fixed body which was to repel the torsion-balance was another disk of foil. The whole was covered with the receiver of an air-pump, and a vacuum, up to 1 or 2 millimetres, was obtained. The whole was then taken into sunshine, and turned so that the needle was kept slightly out of the magnetic meridian by pressure of the fixed disk against one of the movable disks. On concentrating the sun's rays on either of these disks, they instantly separated, sometimes to the extent of a millimetre. On withdrawing the lens, the torsion-balance only gradually returned to its

* Oxford, at the Clarendon Press, 1866, pp. 161, 352.

† Suppl. livr. x. p. 75, A.D. 1799–1805.

‡ Mem. Accad. Torino, xxviii.

§ Philosophical Transactions, 1834, p. 485.

‖ Vol. xxix. pp. 57, 107.

original position. To see if these phenomena were due to the residual gas, air was gradually let in; and on repeating the experiment when the density of the enclosed air was fifteen or twenty times greater than at first, it was found that the repulsion had not sensibly increased, as it should have done had it been due to currents of heated air. Under some conditions, indeed, the movement was not so great as in a vacuum. Sometimes FRESNEL observed an action of attraction, the disks adhering when heated, and separating when the lens was removed. With pieces of copper suspended to the magnetic needle the attraction was very apparent; when the movable and fixed disks were near together they approached on applying heat. Reasons are given why these effects cannot be due to electricity or magnetism, but the author does not seem to have tried any further experiments.

8. M. SAIGEY in 1827 * described an experiment with a needle of lead delicately suspended at different distances from a bar of copper. He found the number of oscillations in a given time decrease with the distance. From his experiments he arrived at the following results:—All bodies exert between themselves a feeble repulsive action under ordinary circumstances. A very marked attraction may be observed between a cold and a heated body, or between two bodies of different temperatures, whether screens be interposed or not. SAIGEY concludes that in many cases results obtained without the appreciable development of magnetism or electricity have been attributed to these forces.

9. In the year 1834 Professor J. D. FORBES † published an elaborate research on the vibrations which Mr. A. TREVELYAN had found to take place between metallic masses having different temperatures. The general conclusion at which he arrived is that there is a repulsive action exercised in the transmission of heat from one body into another which has a less power of conducting it. These repulsions only take place between bodies having a certain amount of conducting-power, below which some metals fall; it must be excitable in a most minute space of time, and is energetic in proportion to the difference of conducting-power of the substances and to their difference of temperature.

10. The Rev. BADEN POWELL, in the same year ‡, published a paper "On the Repulsive Power of Heat." He employed an arrangement somewhat similar to FRESNEL's (7), the disks being two small plates of glass with truly plane surfaces. He found that if in the first instance they were pressed together so as to adhere, heat always overcame the attraction, and the movable disks sometimes receded to a sensible distance; but Professor POWELL says that this effect (and perhaps also that in FRESNEL's experiment) appeared to him in a great measure due to another cause than repulsion, viz. the slight curvature which will be given to the plate of glass by the greater expansion of the more heated surface producing a convexity towards the heat.

11. By pressing the disks closely together, the coloured rings formed would give a

* Bulletin Mathématique, tom. ix. pp. 89, 167, 239, tom. xi. no. 167; Bull. Sci. Nat. viii. p. 287.

† Trans. Roy. Soc. Edinburgh, vol. xii. p. 429. ‡ Philosophical Transactions, 1834, p. 485.

test of the interval between the disks. Professor Powell found that the tints invariably *descended* in the scale when heat was applied, showing that the interval between the disks increased, and proving the existence of a repulsive power exerted between heated surfaces at small though sensible distances—the *warping*, or change of figure, if any, in the glasses by heat being readily seen to be such as ought to cause the rings to *enlarge* at the first instant. From experiments made by the contact of a lens with different substances, Professor Powell inferred that whatever tends to increase the rapidity of communication of heat, tends to increase the observed effect. The effect is increased when water, instead of air, is introduced between two lenses.

12. In 1838 Professor Powell* gave some additional notes on the same subject, but no new form of experiment was tried.

13. Dr. Joule, F.R.S.†, gave an account in 1863 of a new and extremely sensitive thermometer. It was based upon the disturbing effect of currents of air upon finely suspended magnetic needles. By diminishing the directive force of the needle, the instrument was made sufficiently delicate to move to the heat radiated from a small pan containing a pint of water heated to 30°, placed 3 yards off, and also to give evidence of the heat of the moon. I have little doubt that these movements were not so much due to currents of air as to the mechanical effects of radiation described in this paper.

14. It is right that I should mention here that in September 1871 I received a letter from Mr. J. Reynolds, mentioning that he had constructed a little instrument which would turn to the hand, to a fire, or to any source of heat. It consisted of a thin slip of deal suspended by a filament of spider's web, and enclosed in a thin glass flask. This little instrument was more sensitive than any I had then constructed, as the spider's web was much freer than cocoon-silk from torsion, and Mr. Reynolds kindly allowed me to experiment with it.

15. I cannot do better, in bringing this historical summary to a conclusion, than draw attention to a passage written in 1868 by Professor Guthrie, F.R.S.‡, in which he distinctly points out a probable relation between heat and gravity. He says:—"If the ætherial vibrations which are supposed to constitute radiant heat resemble the aerial vibrations which constitute radiant sound, the heat which all bodies possess, and which they are all supposed to radiate in exchange, will cause all bodies to be urged towards one another."

16. Were it such a relation between heat and gravity of which I had been getting glimpses, it was evident that a much more delicate apparatus would be necessary to render it distinct, and I accordingly commenced a series of experiments with the view of ascertaining what form of apparatus would be most sensitive to the action sought.

The first requisite was to get rid of the error arising from the expansion of the beam by heat; and since, in working with hot bodies, the metallic masses used as

* Phil. Mag. vol. xii. April 1838. † Chemical News, vol. vii. p. 150.

‡ Proceedings of the Royal Society, vol. xix. p. 35.

weights would themselves become warm, and since the action I sought to establish was only likely to be due to a *difference* in temperature between the hot body on one arm of the balance and the cold weights on the other arm, both being under the influence of the same force of gravity, I endeavoured to obtain the desired results by means of a spring-balance, one in which the variations of gravity should be measured, not against gravity itself, but against the tension of a spring.

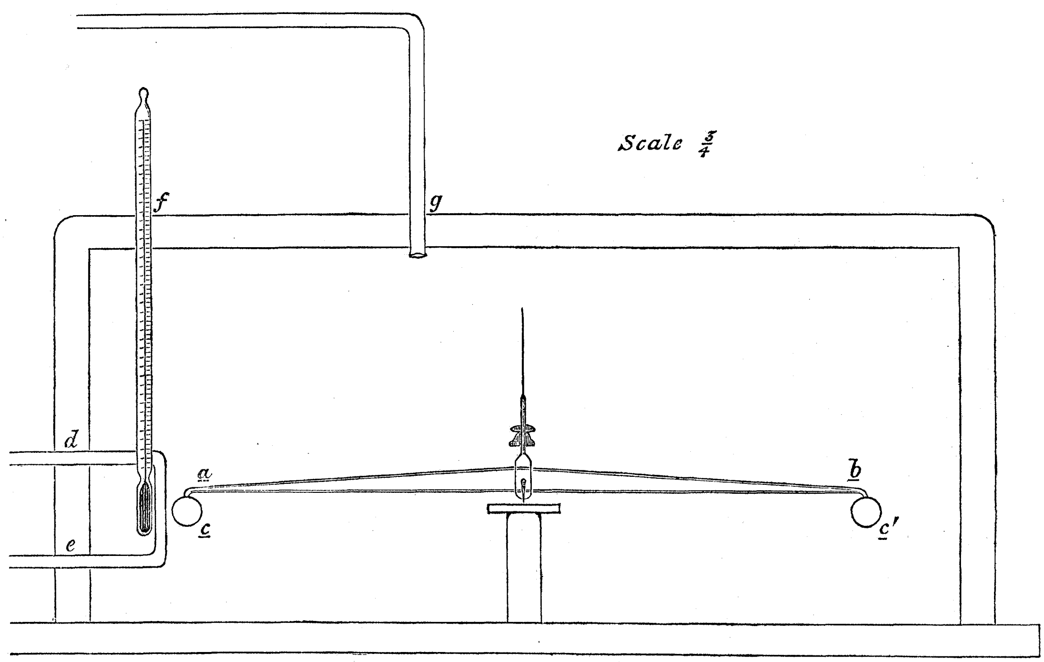
17. I tried many forms of spring-balance, and obtained with them results which, at the time, I thought sufficiently satisfactory. The sources of error were, however, so numerous, and the manipulation was so difficult, that I ultimately gave up that form of balance in favour of the one usually employed.

In order to obtain a very high degree of rarefaction, without the trouble and uncertainty attending the use of an ordinary air-pump, it was necessary to have the balance sufficiently small to enable it to be exhausted by the Sprengel pump.

Before proceeding to the forms of apparatus finally adopted and the experiments made therewith, it will, I think, be useful, for the sake of other experimentalists, if I briefly describe some of the arrangements successively tried and rejected, with the reasons for so doing.

18. A light beam (*a b*, fig. 1) was made of two pieces of fine flattened brass wire

Fig. 1.



120 millims. long, 5 millims. apart at the point of suspension, and joined together at each end. A pair of very fine needle-points, one on each side of the beam, represented knife-edges, and worked on glass plates cemented horizontally to upright pillars. The

centre of gravity could be altered by a screw, and a pointer projecting upwards enabled me to see a movement of the beam. A brass ball (c, c') 6 millims. diameter, was soldered to each end of the beam. The balance was adjusted so that it made one oscillation in about five seconds.

19. The case consisted of a rectangular box of brass, 10 mm. thick, the front of which was replaced by plate glass. At one end two holes were drilled (d, e) about 20 mm. apart, and a curved piece of glass tube was cemented in, so that both ends were outside. At the top a hole (f) was drilled to receive a thermometer, and another (g) to receive the tube attaching the instrument to the Sprengel pump. By sending a current of hot water through the bent glass tube, heat could be communicated to one of the brass balls, and the movement, if any, of the beam could be seen by a micrometer in front. Many experiments were tried with this apparatus, and the result appeared to be that warming the ball caused it to sink (49). This action might, however, be due to the expansion of the brass beam by heat; so to obviate this source of error, I sought for a material wherewith to make a beam which should be as little affected as possible in this manner.

20. A stick of fine-grained charcoal was worked up into the shape of a beam, and fitted at each end and in the centre with appropriate metallic collars for the needle-points and brass balls. To get rid of absorbed gases, which experience showed were evolved unequally in a vacuum, and thereby threw the beam out of adjustment, it was first heated strongly in an exhausted tube, and then soaked in an alcoholic solution of shellac. When quite dry the charcoal was heated till the shellac fused. After much difficulty the beam was adjusted, and being enclosed in the brass case described above, exhaustion was effected. With this charcoal beam I also found that heat generally caused the brass ball to sink (49).

21. These results were opposed to what I had before noticed, but many anomalies were observed (49). Thus the diminution of gravity did not appear to vary as the rarefaction increased; and the position of the hot body, in relation to the brass ball, seemed to have considerable influence on the direction and amount of movement. It was also difficult to make the brass case, with its numerous joints, sufficiently tight to hold a Sprengel vacuum*, even by painting it over with gold-size when partially exhausted, and I therefore decided to form the beam of some other material.

22. A mica beam was at first tried, but it was found to be liable to split across. Magnesium possesses the advantages of lightness and rigidity, but was inapplicable,

* As I shall have to speak of various kinds of vacua, it will be best to name them distinctively to avoid periphrasis. I shall call the best vacuum which my air-pump will give an *air-pump vacuum*—this is one or two millimetres below the barometer. The ordinary vacuum produced by the Sprengel pump I shall call a *Sprengel vacuum*—in this the gauge is appreciably level with the barometer. A so-called "perfect" vacuum, produced by potash and carbonic acid, as subsequently described, or by similar means, I shall call a *chemical vacuum*. I object to the term *perfect*, as applied to any vacuum at present known, as I believe that where force can travel we are not justified in assuming the absence of matter—imponderable it may be, and unaffected by ordinary forms of force—but none the less *matter*.

owing to its expanding by heat. A gridiron beam of zinc and iron or of zinc and glass, and a beam formed of two reversed thermometers, their bulbs forming the gravitating masses, were thought of, so as to have self-compensation for changes of temperature; but after a few experiments the weight of such beams was found to be an insuperable objection, even had the difficulties of adjustment been overcome. Altogether glass seemed to offer most advantages, as being sufficiently rigid, whilst its low conducting-power for heat rendered it little liable to introduce errors from expansion.

A straight glass beam was drawn from a piece of rectangular glass tube, and it was fitted with needle-point centres and brass balls at each end, similar to the plan adopted with the brass and charcoal beams. This was fitted in the small brass case, and many experiments were tried with it. The results were still anomalous, the apparent action of heat being sometimes in one direction and sometimes in another (49).

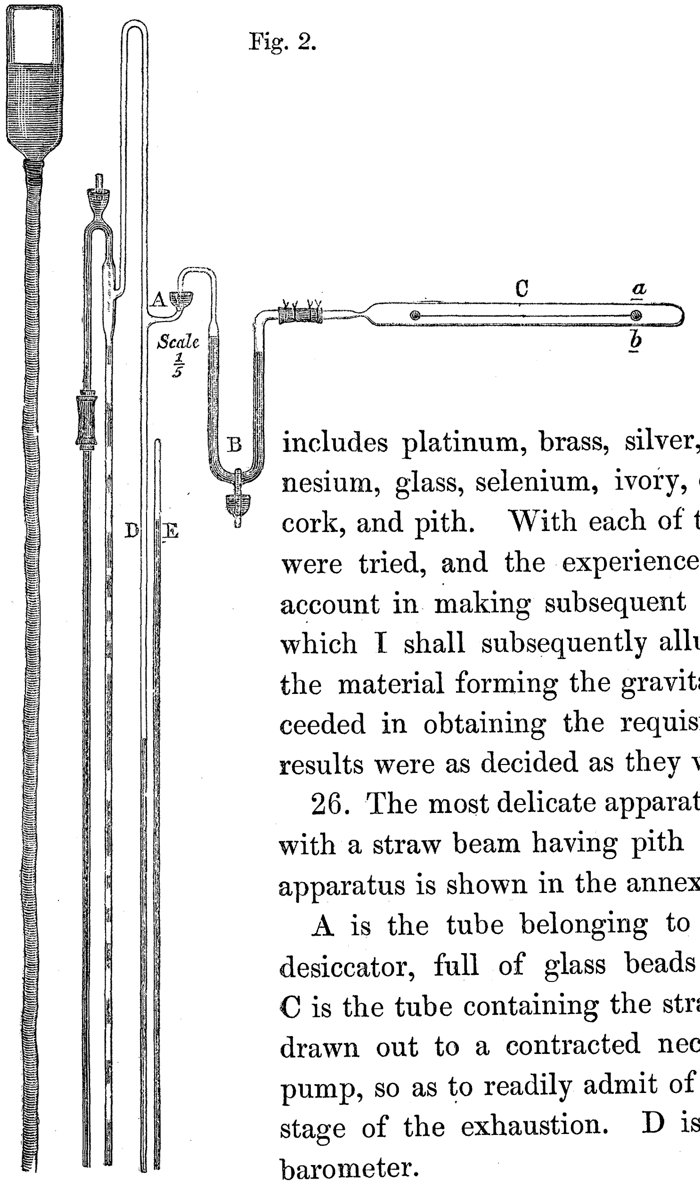
Greater delicacy was still required, and the brass balls at the end of the beam were accordingly replaced with magnesium balls; and instead of enclosing the balance in the brass case, I sealed it up, after exhaustion, in a glass tube. With this a large number of experiments were tried; but as the subsequent results, after I had discovered one of the laws governing the phenomena, were much more satisfactory, I forbear from occupying space in describing these preliminary experiments.

23. Afterwards I experimented successively with a glass beam having no special weight at the ends, and with the same beam with terminal knobs of glass fused on; this appeared to increase the delicacy somewhat, but the weight was still too great to allow of accurate results being obtained; and I finally adopted straw as the material for the beam, varying the gravitating masses at the end as experience dictated. Straw possesses many advantages: it is exceedingly light yet rigid; it dries easily, and evolves no vapour in a vacuum; moreover it is not likely to introduce errors by altering in shape under the influence of the moderate degrees of temperature to which it is subjected in these experiments.

24. The method of supporting the straw beam in the centre, so as to secure the maximum sensitiveness without the liability to get out of order, was difficult at first. After trying suspension by fibres of cocoon-silk from a glass frame, suspension on a fine glass axis resting on thin glass rods, and many other devices, I finally adopted the following mode of support.

The pointed half of a small sharp needle is broken off about half a millimetre shorter than the internal diameter of the glass tube; the blunt end is then ground very sharp on Arkansas stone. The straw, about 7 inches long, having its gravitating masses at the ends, is then balanced on a knife-edge so as to let it roll over to a stable position nd to find its centre; and the needle is then run through it at right angles, at such a distance above the horizontal centre of the straw that the centre of gravity of the whole system is a little below the centre of suspension. The beam being slipped into the glass tube (sealed at one end), the needle is supported very delicately against the sides of the glass by its points, and with the least possible amount of friction. It is best now to exhaust temporarily, heating the straw by passing a spirit-flame along the tube, so as to drive off moisture. If, as is almost certain to be the case, one end becomes heavier than the other, equilibrium can be restored, without much difficulty, by holding the spirit-flame for a few seconds under the heavier end, so as to slightly char the straw or other material. When in good adjustment and sufficiently sensitive the balance is ready for experiment.

Fig. 2.



25. The material with which I formed the masses at the ends includes platinum, brass, silver, lead, bismuth, aluminium, magnesium, glass, selenium, ivory, charcoal of different kinds, straw, cork, and pith. With each of these a large series of experiments were tried, and the experience gained with each was turned to account in making subsequent apparatus*. Certain differences, which I shall subsequently allude to, were noticed according to the material forming the gravitating mass; but as soon as I succeeded in obtaining the requisite degree of delicacy, the chief results were as decided as they were unexpected.

26. The most delicate apparatus for general experiment is made with a straw beam having pith masses at the end. The general apparatus is shown in the annexed figure (fig. 2):—

A is the tube belonging to the Sprengel pump †. B is the desiccator, full of glass beads moistened with sulphuric acid. C is the tube containing the straw balance with pith ends. It is drawn out to a contracted neck at the end connected with the pump, so as to readily admit of being sealed off if desired at any stage of the exhaustion. D is the pump-gauge, and E is the barometer.

27. The whole being fitted up as here shown, and the apparatus being full of air to begin with, I passed a spirit-flame across the lower part of the tube at *b*, observing the movement by a low-power micrometer; the pith ball (*a b*) descended slightly, and then immediately rose to considerably above its original position. It seemed as if the

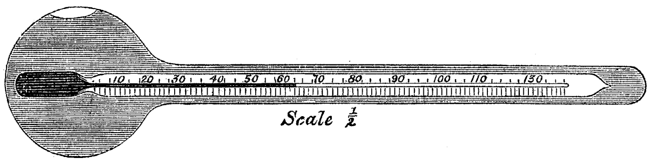
* It is only fair to acknowledge here the assistance which I have received during the progress of these experiments from my young friend and pupil, Mr. CHARLES H. GIMINGHAM. Without his skill with the blowpipe and delicacy of manipulation with complicated apparatus, it would have been difficult for me to have carried out this investigation during the limited time I am able to devote to original research.

† For a full description of this pump, with diagrams, see Phil. Trans. 1873, vol. clxiii. p. 295.

true action of the heat was one of attraction, instantly overcome by ascending currents of air. A hot metal or glass rod and a tube of hot water applied beneath the pith ball at *b* produced the same effect as the flame; when applied above at *a*, they produced a slight rising of the ball. The same effects take place when the hot body is applied to the other end of the balanced beam. In these cases air-currents are sufficient to explain the rising of the ball under the influence of heat.

28. In order to apply the heat in a more regular manner, a thermometer was inserted in a glass tube, having at its extremity a glass bulb about $1\frac{1}{2}$ inch diameter; it was filled with water and then sealed up (see fig. 3). This was arranged on a revolving stand, so that by means of a cord I could bring it to the desired position

Fig. 3.



without moving the eye from the micrometer. The water was kept heated to 70° C., the temperature of the laboratory being about 15° C.

29. The barometer being at 767 millims. and the gauge at zero, the hot bulb was placed beneath the pith ball at *b*. The ball rose rapidly; as soon as equilibrium was restored, I placed the hot-water bulb above the pith ball at *a*, when it rose again, more slowly, however, than when the heat was applied beneath it.

30. The pump was again set to work; and when the gauge was 147 millims. below the barometer, the experiment was tried again; a similar result, only more feeble, was obtained. The exhaustion was continued, stopping the pump from time to time to observe the effect of heat, when it was seen that the effect of the hot body regularly diminished as the rarefaction increased, until, when the gauge was about 12 millims. below the barometer, the action of the hot body was scarcely noticeable. At 10 millims. below it was still less; whilst when there was only a difference of 7 millims. between the barometer and the gauge, neither the hot-water bulb, the hot rod, nor the spirit-flame caused the ball to move in an appreciable degree.

The inference was almost irresistible that the rising of the pith was only due to currents of air, and that at this near approach to a vacuum the residual air was too highly rarefied to have power in its rising to overcome the inertia of the straw beam and the pith balls. A more delicate instrument would doubtless show traces of movement at a still nearer approach to a vacuum; but it seemed evident that when the last trace of air had been removed from the tube surrounding the balance—when the balance was suspended in empty space only—the pith ball would remain motionless, wherever the hot body were applied to it.

31. I continued exhausting. On next applying heat, the result showed that I was

far from having discovered the law governing these phenomena; the pith ball rose steadily, and without that hesitation which had been observed at lower rarefactions. With the gauge 3 millims. below the barometer, the ascension of the pith when a hot body was placed beneath it was equal to what it had been in air of ordinary density; whilst with the gauge and barometer level its upward movements were not only sharper than they had been in air, but they took place under the influence of far less heat—the finger, for example, instantly repelling the ball to its fullest extent.

To verify these unexpected results, air was gradually let into the apparatus, and observations were taken as the gauge sank. The same effects were produced in inverse order, the point of neutrality being when the gauge was about 7 millims. below a vacuum.

32. When the balance was in air of ordinary density, and the hot body was placed *above* the pith ball in the position *a* (see fig. 2), it will be remembered that the action was to cause the ball to rise; the rising was, however, less decided than when the heat was applied below (27, 29). On re-exhausting the balance-tube and taking a series of observations, placing the hot bulb above the pith ball in the position *a* (fig. 2) instead of below it, the ascending tendency of the pith got less and less. Several millimetres below the previously ascertained point of neutrality the hot bulb at *a* ceased to exert an action, and when the neutral point was exceeded by some millimetres I could still detect no movement. However, at 2 millims. below a vacuum, I detected a tendency of the pith to sink when the experiment was tried; and in a good Sprengel vacuum there was an unmistakable repulsion exerted between the two bodies, the pith sinking in obedience to the radiation from above almost as strongly as it rose when the heat was applied beneath.

33. I now wished to ascertain the effect of cold on the balanced pith balls, and for this purpose a lump of ice was employed.

The experiments were first tried with the balance-tube full of air, cold being applied either above or below the pith ball; a lump of ice generally produced an upward movement of the pith, but it was very faint, and sometimes the motion appeared to be in the opposite direction. It was evident that the true action of a cold body, whatever it might be, was here masked by currents of air; and I therefore exhausted the apparatus and tried the effect of ice at the previously ascertained neutral point, viz. at about 7 millims. below the vacuum. It was absolutely inert. I then carried the exhaustion to the fullest extent, testing the balance with ice during its progress. As the gauge approached nearer and nearer to the height of the barometer, the ice commenced to attract the pith; and at last, when the gauge and barometer were level, the attraction of the ice, whether applied above or below, was very marked, being exactly opposite but equal to the action of the bulb of hot water.

34. In trying some of these experiments in a Sprengel vacuum an action was noticed which led me to think that some of these movements might be due to electricity. When a hot glass rod is held motionless against the lower side of the exhausted tube, the repul-

sion of the pith ball takes place in a perfectly regular manner; but if the glass rod has been passed once or twice through the fingers, or if it is rubbed a few times sideways along the exhausted tube, the beam immediately moves about in a very irregular manner, sometimes being repelled from and at others attracted to the side of the tube, where it sticks until the electrical excitement subsides. When the finger is rubbed against the exhausted glass tube, the same electrical interference takes place; attractions and repulsions occur by fits and starts, the pith sticks to the tube, and does not regain its ordinary state for some hours. When a small spirit-flame is passed beneath the pith end of the balance in the vacuum, a similar but much fainter electrical effect is noticed. This, however, is not sufficient to interfere with the repulsion due to radiation unless the vacuum is very good and free from aqueous vapour.

35. The end of a glass beam in a Sprengel vacuum was found to be attracted by either pole of an induction-coil, when the other pole was not well insulated.

To ascertain whether electricity exerted any special action in the ordinary repulsions in vacuum, the following experiments were tried:—

36. A straw beam with pith extremities was enclosed in a tube (24) and exhausted to the full power of the Sprengel pump. After adjustment by heat, till it was in equilibrium and very delicate to slight radiation, it was re-exhausted and hermetically sealed up. The tube was then completely surrounded with wet blotting-paper, with the exception of a small aperture through which the movement of the beam could be observed; the blotting-paper was connected to earth by a wire soldered on to a gas-pipe.

On bringing a piece of warm copper beneath the pith ball, it rose as readily as if the outside of the tube had been dry and insulated. The finger moistened with warm water also repelled the pith; and when cooled with melting ice, and then applied dripping wet either above or below the pith ball, there was attraction. The same result, but more strongly marked, took place when a piece of ice was used instead of the cold finger.

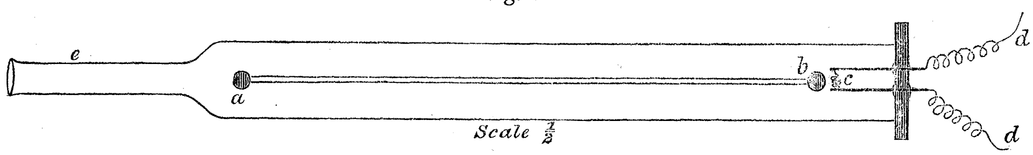
A straw beam furnished with brass balls at each end* was suspended in the usual manner on a double-pointed needle; and the brass balls and needle were placed in metallic connexion by means of a very fine platinum wire. The needle did not rest on the sides of the glass tube but in steel cups, to which was soldered a platinum wire passing through the glass tube and connected to earth. The tube was then exhausted, and the usual experiments tried with hot and cold bodies, both with and without a wet blotting-paper cover. In all cases the brass balls behaved normally, being repelled by heat and attracted by cold.

These experiments show that electricity is not a chief agent in these attractions and repulsions, however much it may sometimes interfere with and complicate the phenomena.

* Preliminary experiments showed me that the brass balls on the straw beam acted in every respect the same as the pith balls, with regard to hot and cold bodies externally applied, both at ordinary pressures and in a vacuum; but they moved in a more sluggish manner.

37. I now wished to see the effect of applying to the gravitating masses a hot body inside the exhausted balance-tube instead of outside, and accordingly constructed an apparatus shown in fig. 4.

Fig. 4.



a, *b* are two balls suspended on a straw beam; *c* is a platinum spiral fastened to two stout copper wires cemented into holes drilled through a plate of glass, which can be cemented to the end of the tube; *d*, *d* are the battery-wires; *e* is the extremity of the tube, drawn out for attachment to the Sprengel pump. A single Grove's cell served to heat the platinum spiral to redness. By careful management and turning the tube round the necessary degree, I could place the equipoised brass ball either over, under, or at the side of the source of heat. A contact-key enabled me to heat the spiral without removing the eye from the micrometer. With this apparatus I wished to learn more about the behaviour of the balance during the progress of the exhaustion, both below and above the point of no action, and also to ascertain the pressure corresponding with this critical point.

In air of ordinary density the action of the hot spiral was one of attraction both above and below. Not wishing, however, to complicate the action by air-currents, I exhausted with the Sprengel pump until the gauge stood about 40 millims. below the barometer. I then tried the following experiments:—

38. The brass ball was placed so that its position when in equilibrium was about 1 millim. above the spiral, and the latter was rendered incandescent. The ball was immediately drawn down to the spiral, sometimes touching and then rebounding with considerable force.

39. The brass ball was then arranged so that it was about 1 millim. below the spiral. On turning on the battery-current the ball rose to the hot platinum. This latter action might be due to air-currents; but it is difficult to imagine that air-currents could drag the ball down to the hot spiral when the latter was beneath it (38).

40. The ball was arranged so that the platinum spiral was opposite the end, but a little above, as shown in fig. 4 A. On igniting the spiral the movement was very slightly

Fig. 4 A.

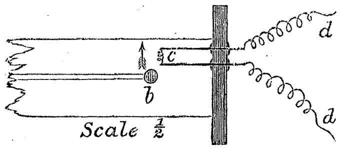
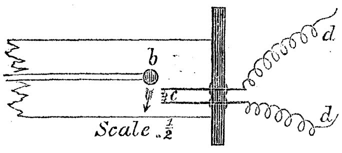


Fig. 4 B.



upwards. When the spiral was rather below the ball, as shown in fig. 4 B, the ball moved downwards when contact with the battery was made; the tendency in each of these cases was to bring the centre of gravity of the brass ball as near as possible to the source of heat.

41. The pump was then worked until the gauge had risen to 5 millims. of the barometric height. On arranging the ball above the spiral and making contact, the attraction was still strong, drawing the ball downwards a distance of 2 millims.

The pump continuing to work, the gauge rose until it was within 1 millim. of the barometer. The attraction of the hot spiral for the ball was still evident, drawing it down when placed below it, and up when placed above it. The movement was, however, much less decided than before, and in spite of previous experience (30, 31) the inference was very strong that the attraction would gradually diminish until the vacuum was absolute, and that then, and not till then, the neutral point would be reached. Within 1 millim. of a Sprengel vacuum there appeared to be no room for a change of sign.

42. The gauge rose until there was only half a millimetre between it and the barometer. The metallic hammering heard when the rarefaction is close upon a vacuum commenced, and the falling mercury only occasionally took down a bubble of air. On turning on the battery-current, there was the faintest possible movement of the brass ball in the direction of attraction.

43. The working of the pump was continued. On next making contact with the battery, no movement of the ball could be detected. The red-hot spiral neither attracted nor repelled. I had arrived at the critical point. On looking at the gauge I saw it was level with the barometer.

44. The pump was now kept at full work for an hour. The gauge did not rise perceptibly; but the metallic hammering increased in sharpness, and I could see that a bubble or two of air had been carried down. On igniting the spiral, I found that the critical point had been passed. The sign had changed, and the action was faint but unmistakable *repulsion*. The pump was still kept going, and an observation was taken from time to time during several hours. The repulsion continued to increase. The tubes of the pump were now washed out with oil of vitriol*, and the working was continued for an hour.

45. The action of the incandescent spiral was now found to be energetically repellent, whether it was placed above or below the brass ball (figs. 4 C, 4 D). The finger

Fig. 4 C.

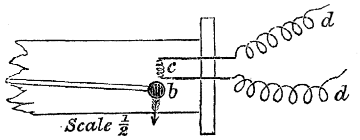
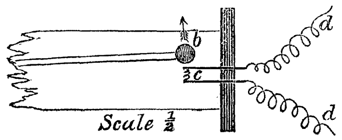


Fig. 4 D.



exerted a repellent action, as did also a warm glass rod, a spirit-flame, and a piece of hot copper.

* This can be effected without interfering with the exhaustion. (See Philosophical Transactions, 1873, vol. clxiii. p. 296.)

46. I now heated the sulphuric acid, in the U-tube desiccator attached to the pump, until it boiled. A little aqueous vapour was driven off; the sharpness of the hammering of the mercury changed to a duller sound; and on trying an experiment once more with the ignited spiral, the action was seen to have returned to one of attraction.

On leaving the pump to itself for an hour or two, the sulphuric acid, as it cooled, again took up the aqueous vapour; and as the absorption proceeded I could trace the action of the hot spiral from attraction, through the point of neutrality, up to decided repulsion.

47. The critical point, in the case of brass balls, is much higher than with pith balls. In the case of pith, applying heat outside, I obtained neutrality when the gauge was about 7 millims. below the barometer, and decided repulsion when there was still a difference of 2 millims. With the apparatus just described, however, using brass balls and an internal source of heat, the critical point is very near the Sprengel vacuum. By close observation I can just distinguish that the gauge is a fraction of a millimetre below the barometer when the hot spiral ceases to exert an attractive action on the brass ball; but my unassisted eye is not able to detect a difference on the gauge between a Sprengel vacuum in which the ignited spiral is neutral to, and one in which it strongly repels, the brass ball.

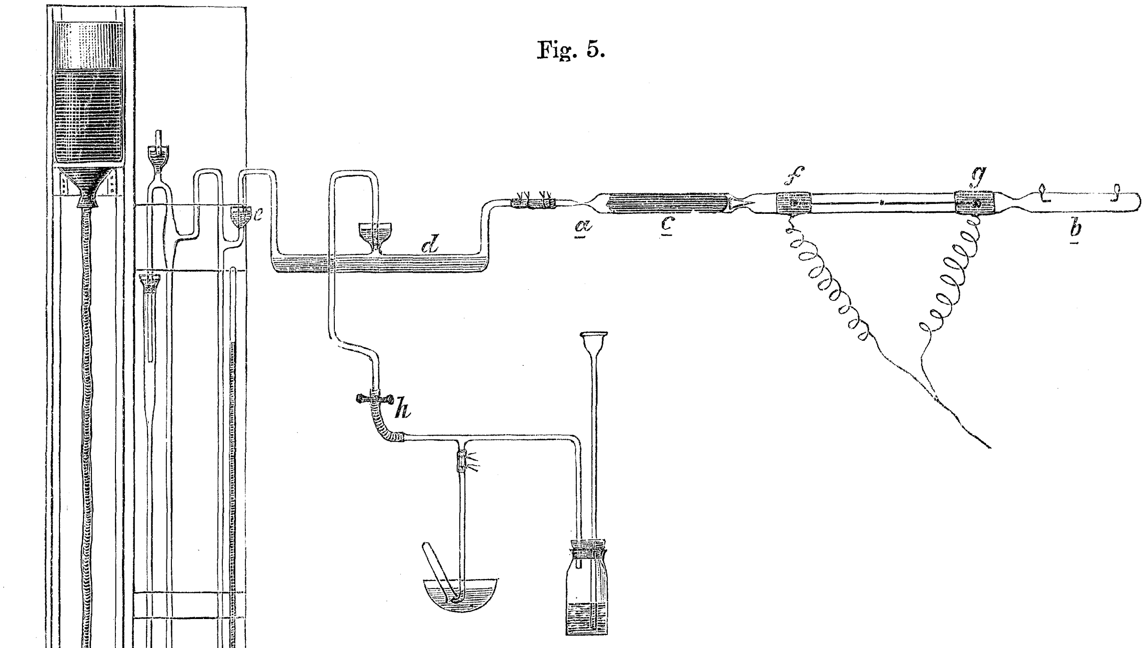
48. I have experimental grounds for believing that the position of the critical point varies with the density of the mass on which radiation falls, with the relation of its mass to its surface, and with the temperature of the apparatus (62, 63, 64, 65, 67, 75). The temperature of the body used to attract or repel the brass or pith ball also affects the critical point, but it cannot, I think, modify it much; for with the apparatus last described no difference in kind of movement, but only in degree, was noticed, whether the spiral were ignited to full redness, or whether it were merely warmed by a momentary contact of the wires. Further experiments are in progress which may throw light on this point.

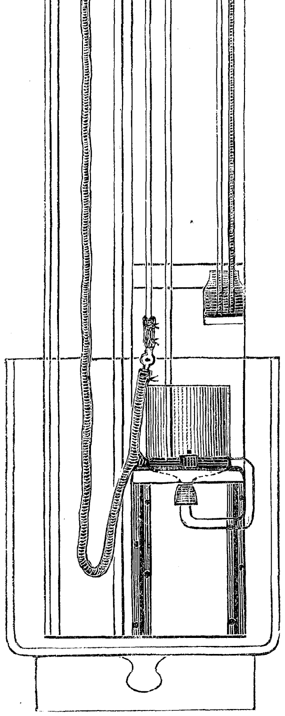
49. The very high amount of rarefaction needed before this neutral point is reached, and the change of direction of movement on applying a hot body to one arm of the balance, caused by a difference of exhaustion of a few millimetres on one side or the other of the point of neutrality, are, I think, a sufficient explanation of the anomalous results which were met with in my earlier experiments (2, 19, 20, 21, 22).

50. Although these results were sufficient to show that air-currents could not be the cause of the movements of the balance, I was anxious to decide this point once for all, by a form of experiment which, whilst it would settle the question indisputably, would be likely at the same time to afford information of much interest. Having found that the balance last experimented with had its neutral point close to an ordinary Sprengel vacuum, that the repulsive action only came on by still further pushing the exhaustion, and that, as I further exhausted, the repulsion got stronger, it was of interest to see what would take place in a vacuum so nearly perfect that it would not carry a current from a RUHMKORFF'S coil.

In small tubes, and taking certain special precautions, I could prepare a vacuum with my Sprengel pump which would hardly allow an induction-current to traverse it, or would only show a faint, cloud-like discharge; but it was impossible to effect this in the large tubes required for these experiments, and, in fact, all the Sprengel vacuum balance-

Fig. 5.





tubes which I had hitherto prepared became brightly luminous when the current from an induction-coil was passed through them. I accordingly fitted up the apparatus shown in figure 5, in which a chemical vacuum could be prepared by a method first devised by Dr. ANDREWS (Philosophical Magazine, February 1852).

51. *a b* is the tube containing a straw beam with pith-ball terminals; at *b* two platinum wires are passed through, to connect with an induction-coil; at *a* the tube is contracted to allow the apparatus to be sealed off; *c* is a portion of the tube containing a copper boat filled with freshly cast sticks of caustic potash; *d* is a tube bent as shown, and nearly full of strong sulphuric acid, which has been previously boiled for some minutes and then allowed to cool in a vacuum; *e* is a mercury joint, connecting the apparatus to the Sprengel pump. At the upper part of the tube *d* is a stopper fitting into a funnel-joint and capable of being replaced (as shown in figure) by a tube through which I could pass carbonic acid when desirable. The carbonic acid was prepared by the action of hydrochloric acid on marble; when not being passed into the exhausted tube, the gas was kept bubbling through mercury,

where a tubeful could be collected from time to time (as shown in the figure) to test with potash. It was found necessary to keep the evolution going on all the time pretty briskly, to prevent air diffusing in. The joints were made of double caoutchouc tubing, the smaller one tightly wired on and coated with glycerine before the larger tube was slipped over it. The whole was then tightly bound with wire. To prevent air creeping down between the mercury and the glass, glycerine was poured over all the mercury joints, except the one at the top of the mercury fall-tube, which was kept for oil of vitriol, with which the pump was lubricated from time to time.

52. The apparatus being exhausted of air, the balance was adjusted by heating the ends so as to slightly char the one which happened to be the lower. *f* and *g* are two collars of silver foil encircling the tube where the heat is to be applied, and connected with earth by wire. At a very high rarefaction the flame of a spirit-lamp excites so much electrical disturbance in the balance, that its adjustment becomes well nigh impossible. This arrangement was adopted in the endeavour to carry off the electricity; it is, however, only partially successful, and the electrification of the balance at the highest rarefactions is still very troublesome.

The air having been removed from the apparatus as perfectly as the Sprengel pump would effect it, carbonic acid was let into the tube by cautiously opening the tap *h*. Exhaustion was again effected, and carbonic acid passed in a second time. This was then pumped out, and the apparatus was filled a third time. This alternate filling with carbonic acid and exhaustion was continued until the gas collected at the bottom of the mercury fall-tube of the pump was entirely absorbed by potash. When this was found to be the case, the exhaustion was allowed to proceed to the highest possible point *. The pump was then stopped; an induction-current now being passed between the wires at the end *b* showed the usual white light of a carbonic-acid vacuum (a trace of red shows atmospheric nitrogen).

The sticks of potash in the copper boat in *c* were then heated to incipient fusion, and the whole was allowed to cool for some hours. The tube was then sealed off by applying a spirit-flame to the contracted part *a*; the potash was then heated again, and the whole was set aside to give the potash time to absorb the residual carbonic acid.

53. By testing from time to time with an induction-coil, the progress of the absorption could be traced; and when the current ceased to pass through the tube, but preferred to strike across in air the full length of the spark, the vacuum was considered nearly perfect. Warming the potash with a spirit-flame, at any time, will cause it to give off sufficient aqueous vapour to allow the spark to pass as a cloud-like luminosity. This will be gradually absorbed until, in the course of from a few days to a few weeks, the vacuum will again cease to conduct.

54. It is very difficult to get the vacuum in large tubes so nearly absolute as not to

* When the pump is working in a very good vacuum, the friction of the falling mercury produces a very beautiful effect in the dark. Brilliant points of light flash about wherever the mercury-drops are splashed from side to side, and the pump is frequently illuminated with a phosphorescent glow filling all the tubes.

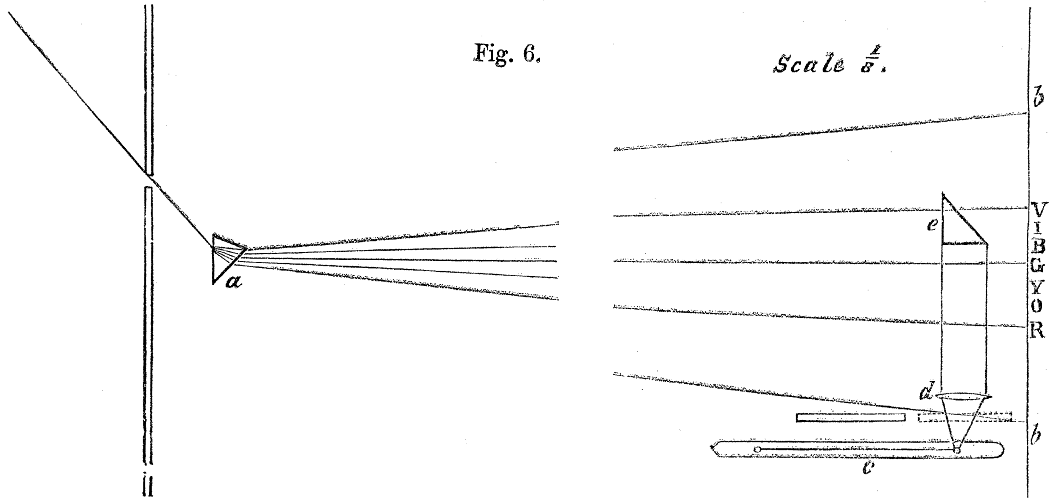
allow a white, cloud-like discharge to pass. I have tried experiments with balances sealed up in vacua of both kinds prepared in this manner, and I always find the repulsion on the approach of a hot body is very decided, although I do not think it is more energetic than with the same kind of balance enclosed in the best Sprengel vacuum I could prepare. These experiments have therefore set at rest the doubts which might have arisen had I only worked with the Sprengel vacua, that air-currents were the cause of the phenomena. They have decided the important point, that in a chemical vacuum which will not carry an induction-current the repulsion by radiation is decided and energetic.

55. Besides the straw and pith balance mentioned above (51, 52), I have prepared and sealed up in chemical vacua balances made of a glass beam and platinum terminals, and also of glass entirely, the ends being flat glass plates. They act in all respects as the straw and pith balance, only being somewhat less sensitive.

56. The delicacy of one of the straw and pith balances in a good vacuum is very great, as may be comprehended by the following experiment:—The balance-tube was supported on a stand, and immediately beneath one of the pith balls was placed the face of a bismuth-antimony thermopile. On connecting the terminal wires of the pile with a single cell-battery, so that the current flowed in one direction, heat was produced, and the pith ball was repelled. On reversing the current, cold was produced and the pith ball was attracted.

57. The rays of the sun allowed to shine on the terminal pith ball of one of these delicate balances *in vacuo* repel it strongly. If concentrated by a lens, the focal point beats the ball away, as if it were struck with a material agent. If the sun's light be filtered through coloured glasses before concentrating them on the pith ball, the action is slightly diminished. Passing the light through two very clear plates of alum with parallel sides, and having an aggregate thickness of 8 millims., had but little action.

Fig. 6.



58. A beam of sunlight was passed through a prism *a* (fig. 6), and the spectrum was projected horizontally on to a screen (*b*) 18 feet off. In front of the screen, a little

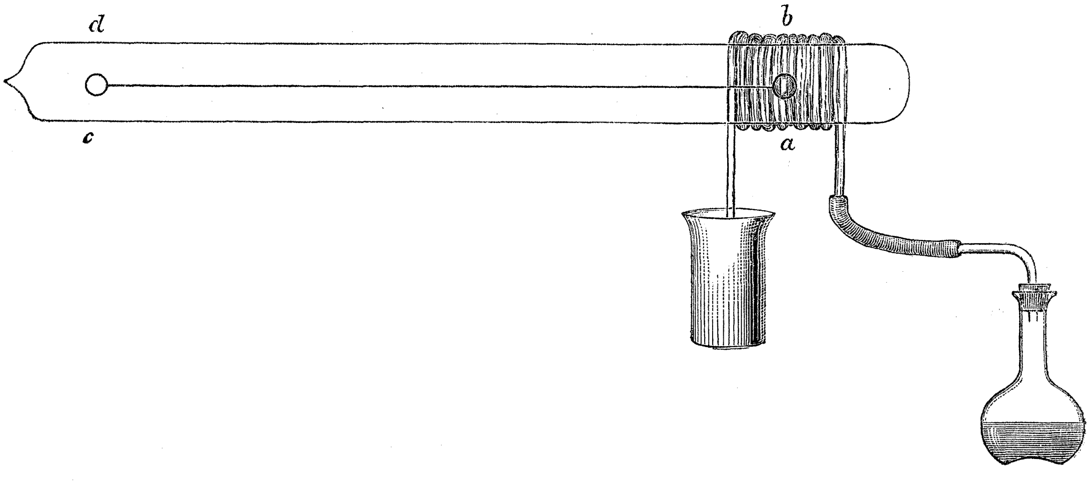
below it, was arranged the balance-tube *c*, having fixed above it a lens *d*, so that one of the pith balls was in the focus of the lens. A small right-angled prism (*e*) was then held in the different parts of the spectrum, so as to reflect any desired ray on to the lens and concentrate it on to the pith ball. By turning the right-angled prism sideways, the ray could be thrown off and on the ball, and the action readily noted. In the ultra-violet rays the repellent action was slight; the effect increased as I made the reflecting prism traverse the spectrum from the violet to the red. The maximum action was in the extreme visible red, and it gradually faded away as the invisible rays beyond the red were reflected down. An appreciable action of repulsion was, however, observed a full half spectrum in length below the least refrangible visible rays. The plates of alum, interposed in the path of the rays, cut off a small portion only of their action.

59. The following experiments were tried with a very sensitive pith-ball balance sealed up in a good Sprengel vacuum:—

The beam was turned over, so that the centre of gravity was a very little above the centre of suspension; the balance consequently set on either side. In this condition the ball touching the tube could be made to rise by placing the finger beneath it, the balance then oversetting on the other side. The higher ball could also be made to sink in the same manner by placing the finger on the top of the tube just over it.

60. A piece of glass tube, bent in the form of a spiral (fig. 7), was supported on a stand, so that it could be slipped over one end of the balance-tube, which it fitted

Fig. 7.



loosely. Steam was then passed through the glass coil, and a woollen cover was put on it, so that the end of the balance-tube could be kept at 100° C. When the temperature became uniform and steady the pith ball took up a position, as nearly as could be judged, in the axis of the spiral, and then remained stationary. On raising the hot coil, so as to bring the lower part close to the tube, the pith ball rose a little, and on lowering the spiral the pith ball sank.

On applying the warm finger to the cool extremity of the balance-tube, either above or below the pith ball, the latter was repelled by it; and by employing a lump of ice in the same way in each position the pith ball was attracted. The balance seemed to be as sensitive when one end was heated in this manner as it was when both ends were of the same temperature; but the rise and fall were not to so great an extent, owing to the controlling action of the hot spiral.

61. The following experiments were tried with a view of ascertaining the conditions of greatest sensitiveness. In all cases the balances were in a good Sprengel vacuum.

A ball of ivory was balanced against a ball of brass on a straw beam; the ivory was more sensitive to radiation than the brass.

62. A pith ball and one of platinum of the same weight were balanced on a straw beam. The pith was very sensitive, being readily repelled by the finger; but the platinum was sluggish, and required a spirit-flame to move it.

63. Two pith balls of the same weight, one gilt and the other plain, were fixed to the ends of a straw beam; they appeared equally sensitive to radiation.

Two rectangular blocks of silver and bismuth, each weighing $4\frac{1}{4}$ grains, were balanced against one another on a straw beam *; they were each repelled by a warm body applied above or below. The bismuth was a little more sensitive than the silver to the action of heat, but it exposed a little more surface for the rays to impinge upon.

64. A selenium ball was balanced against a copper ball: the selenium was more sensitive to radiation than the copper; but I do not think it was more so than would be due to its more extended surface. When I allowed luminous rays, either from the sun or from artificial sources of light, to fall on the selenium, I could detect no special action which I could correlate with the action of light on selenium lately discovered by Mr. WILLOUGHBY SMITH †.

65. Two pieces of thin mica, each having a surface of half a square centimetre, were fastened to the end of a straw beam, one being horizontal and the other vertical. On applying slight warmth, each end was found to be very sensitive. The horizontal mica was easiest affected by the heat; but it had not that great advantage over the vertical piece which might have been expected from the much greater surface exposed had the movement been due to air-currents.

* These two metals were taken as representing nearly the two extremes of metallic conductors for heat and electricity, and also being the best (silver) and the worst (bismuth) in the list of "vibrators" given by Professor FORBES in his "Experimental Researches regarding certain vibrations which take place between metallic masses having different temperatures" (Transactions of the Royal Society of Edinburgh, 1834, vol. xii. pp. 429–461).

† Telegraphic Journal, vol. i. p. 78.

Received August 18,—Read December 11, 1873.

66. A piece of flat plate-glass, 15 millims. wide and 1·5 millim. thick, was heated in the middle before the blowpipe till quite soft, and then drawn out till a long ribbon of glass was produced, the width and thickness of which retained the proportions of the original piece. From the middle of this a portion 150 millims. long, 4 millims. wide at the ends, and 3 millims. wide in the centre, was cut off. A double-pointed needle (24) was then secured to the centre by binding with platinum wire and fusing the latter to the glass. This little balance was adjusted until it was very delicate, and was then enclosed in a tube containing potash at one end and platinum wires sealed in at the other end; it was then arranged, in connexion with the Sprengel pump and carbonic-acid apparatus, so as to produce a chemical vacuum (51, 52, 53).

When the flame of a spirit-lamp was passed under one end of this balance, the apparatus being full of air at the ordinary density, the result was decided attraction (27, 37, 38, 40, 41). On exhausting and testing from time to time in the same manner *, I found the attraction, or rather the sinking of the heated end towards the spirit-flame, to keep at about the same strength until the gauge had risen about 500 millims. After this there was a gradual decline in the downward movement of the glass end of the balance when heat was applied, until the gauge stood about 100 millims. below the barometer. On testing the movement at this pressure I at first thought it was very slight; but on keeping the flame of the lamp for about half a minute below the end of the balance the latter commenced to sink, and then the downward movement was almost as great as ever. With a difference of 95 millims. the phenomena were similar, the lamp, however, requiring to be kept under the balance end longer, and the ultimate movement not being so great. At 90 millims. the same thing occurred, the time of heating having to be still longer.

When the gauge was raised to 85 millims. of the barometric height, and the lamp was applied, the first movement was one of repulsion. The glass end rose instantly, but to a very slight extent. On keeping the lamp under for about a minute, the end of the balance slowly came down, until it had sunk a little below its original position.

At 80 millims. difference between gauge and barometer the effect was almost the same as at 85 millims. The preliminary rise was, if any thing, a little more marked. For fear of injuring the connexions of the apparatus, I did not like to apply the spirit-flame for a longer time than a minute, as the glass then commenced to soften; I therefore, at the higher exhaustions, paid most attention to the initial movement of the glass beam, merely keeping the lamp beneath long enough to see if the continued heating drew the beam down again.

At a difference of 45 millims. between the gauge and the barometer, the neutral point was about reached. The glass beam was not quite motionless on applying the spirit-

* It is perhaps scarcely necessary to say that the apparatus was allowed full time to acquire the ordinary temperature of the laboratory between each of these experiments.

lamp: it rose very slightly at first, and then descended to the same distance below the original level on continuing the heat.

At 35 millims. the initial rise was immediate and decided. Continued heat only lowered the end a trifle, but did not bring it down to any thing near its original position.

From this point the attraction towards the heat ceased to be perceptible. The upward movement increased in strength and amplitude until the Sprengel vacuum was reached *.

At 10 millims. below the barometer the glass beam was repelled by a glass rod or a lump of copper heated to 100° C.; at a difference of 5 millims. between the gauge and the barometer the finger applied below repelled the beam, and at 2 millims. difference the beam was repelled when the finger was applied above.

In the Sprengel vacuum the beam was very sensitive to the approach of a warm body, a touch of the finger sending it away to the fullest extent.

67. I consider that the difference observed between the behaviour of this glass beam, the straw beam with brass ends (37 to 40), and the straw beam with pith ends (30, 31, 32) is to be accounted for by the different materials forming the balances, the different way in which heat was applied in the three cases, and the extent of surface exposed in proportion to mass. There is, however, much to be found out in respect to the position of the critical point, and I am still at work on the subject.

Carbonic acid was then let into the apparatus, and it was re-exhausted. Not wishing to tax the potash too severely, I did not spend much time during this exhaustion; a few observations were, however, taken at different heights of the gauge, and the position of the neutral point was apparently a little raised. The general results were, however, the same as with air. In a carbonic-acid Sprengel vacuum the repulsion by heat was exactly the same as in an air Sprengel vacuum.

68. The remainder of the process for producing a chemical vacuum was then gone through; the tube was set aside till the potash had done its work, and experiments were then tried with it. To the heat of a spirit-lamp, a warm glass or metal rod, the fingers, or the image of a gas-flame concentrated by a lens, the balance was very sensitive, being instantly repelled to a distance varying with the intensity of the heat. Experiments were tried with different rays of the solar spectrum with a similar result to that described in par. 58. The vacuum was so nearly perfect that an induction-spark would not pass, but preferred to strike across its full distance in air.

69. If one of the most sensitive balances (straw and pith (24), or the glass one last described (66)) in a well-exhausted tube is carefully turned on its side, after a little

* In these and many of the other observations accurate measurements were taken of the extent of movement by means of a micrometer-eyepiece, and the time was also accurately noted by a seconds' watch. As, however, the equal and uniform delicacy of the instrument could not be depended upon, I think it best only to give the results in general terms rather than to mislead by an affectation of accuracy not justified by the instrumental means employed.

practice the beam can be balanced on one point of the suspending-needle, which will be nearly vertical. In this position the beam has a horizontal movement; and by carefully adjusting the level of the tube the delicacy of the beam can be made very much superior to what it would be suspended in the ordinary way. By bringing a warm body near one end of the balance, it is now driven away to the utmost extent, and a piece of ice attracts it with equally marked energy.

70. On trying this experiment in air of ordinary density, the approach of a hot body causes unmistakable attraction, and a cold body repulsion. In a vacuum this mode of arranging the apparatus did not at first appear to offer advantages over the plan already adopted; but as from the direction of movement it was not likely that air-currents could interfere, or at all events not to any great extent, I have arranged apparatus for obtaining the movements of repulsion and attraction in a horizontal instead of a vertical plane, so as to examine the action in air.

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. I propose shortly to submit an account of this second series of researches to the Society.

71. I have more recently instituted experiments to ascertain how far the action of gravitation in CAVENDISH's celebrated experiment is likely to be modified under the influence of heat. For many months I have been experimenting with apparatus devised for this purpose. The investigation is not sufficiently advanced to justify further details, but I may perhaps be permitted to give here an outline of one of the results.

72. I find that a heavy metallic mass, when brought near a delicately suspended light ball, attracts or repels it under the following circumstances:—

I. *When the ball is in air of ordinary density.*

- *a.* If the mass is *colder* than the ball, it *repels* the ball.
- *b.* If the mass is *hotter* than the ball, it *attracts* the ball.

II. *When the ball is in a vacuum.*

- *a.* If the mass is *colder* than the ball, it *attracts* the ball.
- *b.* If the mass is *hotter* than the ball, it *repels* the ball.

73. The density of the medium surrounding the ball, the material of which the ball is made, and a very slight difference between the temperatures of the mass and the ball exert so strong an influence over the attractive and repulsive force, and it has been so difficult for me to eliminate all interfering actions of temperature, electricity, &c., that I have not yet been able to get distinct evidence of an independent force (not being of the nature of heat or light) urging the ball and the mass together.

Experiment has, however, shown me that, whilst the action is in one direction in dense air, and in the opposite direction in a vacuum, there is (as I have already pointed out with the balances) an intermediate pressure at which differences of temperature appear to exert little or no interfering action. By experimenting at this critical pressure, and at the same time taking all the precautions which experience shows are necessary, it would seem that such an action as was obtained by CAVENDISH, REICH, and BAILY should be rendered evident.

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74. Throughout the course of these investigations I have endeavoured to keep in my mind the possible explanations which may be given of the actions observed, and I have tried, by selecting some circumstances and excluding others, to put each hypothesis to the test of experiment. The most obvious explanation is that the movements of the beam, or of the horizontal index (69, 70), 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.

In favour of this explanation it may be urged that a highly rarefied gas may be much more mobile than when it is denser, and therefore the more rapid impingement of its particles, when set in ascension by warmth, would increase their mechanical action. Increased momentum may counterbalance diminished number.

That the residual gas in an air-pump vacuum is capable of exerting considerable mechanical action, may be assumed by the phenomena attending the passage of meteorites through the upper regions of the atmosphere, their friction against the air at an average height of 65 miles above the earth's surface raising them to incandescence, although at that height the attenuation of the air probably surpasses that of most artificial vacua.

On the other hand, it is most difficult to believe that the residual air in a Sprengel vacuum, where the gauge and barometer are appreciably level, can exert, when gently warmed by the finger, an upward force capable of instantly overcoming the inertia of a mass of matter weighing several grains, and setting it in motion. It must be remembered that the upward current supposed to do this is simply due to the diminished weight of a portion of the gas, caused by its increase in volume by the heat applied.

75. Another argument in favour of the air-current explanation can be drawn from the fact that when a light beam, having equal weights of pith and platinum suspended at the ends, is sealed up in a Sprengel vacuum, the application of warmth below causes the pith to rise more readily than the platinum (62); the pith obviously offers a much more extended surface than the platinum does to the impact of air-particles.

This, moreover, is not an isolated instance. Throughout the whole of these experiments the law appears to be that the force exerted is in proportion to the extent of surface exposed (48, 62, 63, 64, 65, 67) rather than in proportion to the mass. Much surface and extreme lightness are the requisites in selecting materials for the beam, index, or gravitating mass; and when the masses have the same specific gravity and extent of surface, their position in respect to the source of heat determines the extent of movement. Thus a cylinder of pith is more sensitive when arranged for the heat to act on its side than on its end; and the film of mica in experiment 65 was more affected when the heat struck its flat surface than its edge, although the difference was not so great as might have been expected had air-currents been the cause of motion.

76. But these facts can equally well be used on the opposite side; for assuming that the movement is due to a repulsive action of radiation, it is reasonable to suppose that extended surface, weights being equal, would have an advantage. The repulsion by radiation only acts on the surface of bodies, and does not seem to act on the molecules which constitute thickness. When radiant heat gets below the surface of a body, it spends itself in doing mechanical work of another kind, viz. dilatation or expansion.

77. However strong may be the reasons in favour of the air-current explanation, they are, I think, answered irrefragably by the phenomena themselves. An air-current produced by heat can cause the beam of a balance to rise, can drive a suspended index sideways, and, by a liberal assumption of eddies and reflections, can perhaps be imagined to cause these movements to take place sometimes in the opposite directions; but as rarefaction proceeds these actions will certainly get less, and they will cease to be appreciable some time before a vacuum is attained: a point of no action or neutrality will be reached. But this neutral point should certainly be nearer a vacuum when a light body exposing much surface, such as pith, is under experiment than when the mass acted on is heavy like brass; whereas in practice the contrary obtains. Pith and thin glass balances, which should be sensitive to highly attenuated air-currents, cease to respond to heat at a rarefaction of 7 millims. (30) and 45 millims. (66), whilst brass only ceases to be affected when the gauge and the barometer are appreciably level (43).

But even could the phenomena up to neutrality be explained by air-currents, these are manifestly powerless to act after this critical point is passed. If a current of air within 7 millims. of a vacuum cannot move a piece of pith, certainly the residual air in a Sprengel vacuum should not do so; and, *à fortiori*, the residual air in a chemical vacuum could not move a piece of platinum (55).

It is, however, abundantly demonstrated that in all cases, after this critical point is reached, the repulsion by radiation is most apparent, and it increases in energy as the vacuum approaches perfection.

78. Again, the movements not only reappear on passing a particular point of atmospheric density, but they take place in the opposite direction (31, 32, 33, 44, 46, 66). Thus in all cases, when the atmospheric density is between the neutral point and a vacuum, the action of a body hotter than the moving beam or index is to repel it, whilst the action of a colder body is to attract. Now it is very probable that were it not for the interference of air-currents the action in air of greater density would always be for the hot body to attract. This is actually the case in many experiments (27, 37,

38, 40, 41, 66, 70, 72); and observations more recently made, but only alluded to in par. 72, have proved that in air of ordinary density a cold body repels.

On the supposition that air-currents are the motive power, the effects noticed when the source of heat is internal to the tube, and applied above the moving beam (37, 39, 40, 41, 45), are inexplicable, whilst they are easily comprehended on the repulsion-by-radiation hypothesis.

If an additional argument is necessary to show that air-currents are not the cause of the repellent action of a hot body, I bring forward the fact that the movement attains its maximum when there is no air at all present (54, 55, 68).

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79. Effects probably due to this repulsive action of radiation are constantly met with. I will instance the following:—

Cohesion and adhesion are diminished by heat. This naturally follows if increased temperature augments the force of repulsion between the molecules.

The phenomenon of the spheroidal state is probably due in some measure to a repulsive force exerted between closely approximated bodies, one of which is at a very high temperature. This action is generally supposed to take place only when one of the bodies is volatile, and the rapidly formed skin of vapour is held to be a sufficient cause of non-contact. I venture to anticipate that a condition similar to the spheroidal state will be found to obtain between non-volatile bodies.

Many finely divided chemical precipitates, when incandescent in a platinum crucible, assume a remarkable mobility and flow about like water. Precipitated silica is an instance which will occur to chemists. A space can readily be distinguished between the powder and a hot capsule containing it. Electricity may, however, play some part in this action; for precipitates, when heated, sometimes become sufficiently electrical by stirring with a glass rod to fly out of the basin containing them; oxalate of lime possesses this property in a remarkable degree*.

80. It must, however, be remembered that my experiments show the action of hot bodies in air to be that of attraction, and that the repulsion by heat only becomes evident near upon a vacuum. It is seen, therefore, that radiant light or heat has an attractive or repulsive action, according to the medium in which it acts, corresponding results being furnished by cold. There appears to be an interfering action of air other than that of the currents caused in it by heat, which masks or overcomes the true action of heat; but in a vacuum this interfering cause is absent, and radiant heat is free to exert its full repellent action, whilst cold or negative heat acts in the opposite direction. Heat and cold, heat present and heat absent—molecular activity and molecular rest—are therefore antagonistic in their action on a body free to move in empty space. The molecules of matter whose mode of motion constitutes heat are drawn together and

* FARADAY's 'Experimental Researches in Electricity,' vol. ii. p. 163.

condensed as these vibrations diminish in amplitude, whilst heat drives them apart, expanding a solid, changing a solid into a liquid and a liquid into a gas.

The masses used in my experiments are likewise repelled by heat and drawn together by cold. And it is with no weak force or feeble action that I have been dealing. It is so decided that in some of my balances the approach of a finger will completely overturn them, whilst the radiant warmth of the body affects them 6 feet off; and at higher temperatures and with larger masses the action must be still more energetic.

81. It is not unlikely that in the experiments here recorded may be found the key of some as yet unsolved problems in celestial mechanics. In the sun's radiation passing through the quasi vacuum of space we have the radial repulsive force, possessing successive propagation, required to account for the changes of form in the lighter matter of comets and nebulæ; and we may learn by that action, which is rapid and apparently fitful, to find the cause in those rapid bursts which take place in the central body of our system; but until we measure the force more exactly we shall be unable to say how much influence it may have in keeping the heavenly bodies at their respective distances.

So far as repulsion is concerned, we may argue from small things to great, from pieces of pith up to heavenly bodies; and we find that repulsion shown between a cold and warm body will equally prevail, when for melting ice is substituted the cold surface of our atmospheric sea in space, for a lump of pith a celestial sphere, and for an artificial vacuum a stellar void.

Attraction being developed by radiant heat under influences connected with air, it is not easy to conceive how it will be produced for cosmical purposes by heat; the upper surface of our atmosphere must present a very cold front, and from this we might argue repulsion by the sun, unless we fill space with a body acting like air, when we should have attraction. We might readily find conditions for both, but how to harmonize them is a difficulty.

Although the force of which I have spoken is clearly not gravity solely as we know it, it is attraction developed from chemical activity, and connecting that greatest and most mysterious of all natural forces, action at a distance, with the more intelligible acts of matter. In the radiant molecular energy of solar masses may at last be found that "agent acting constantly according to certain laws" which NEWTON held to be the cause of gravity.