XV. *On the Supposed "New Force" of* M. J. Thore.*

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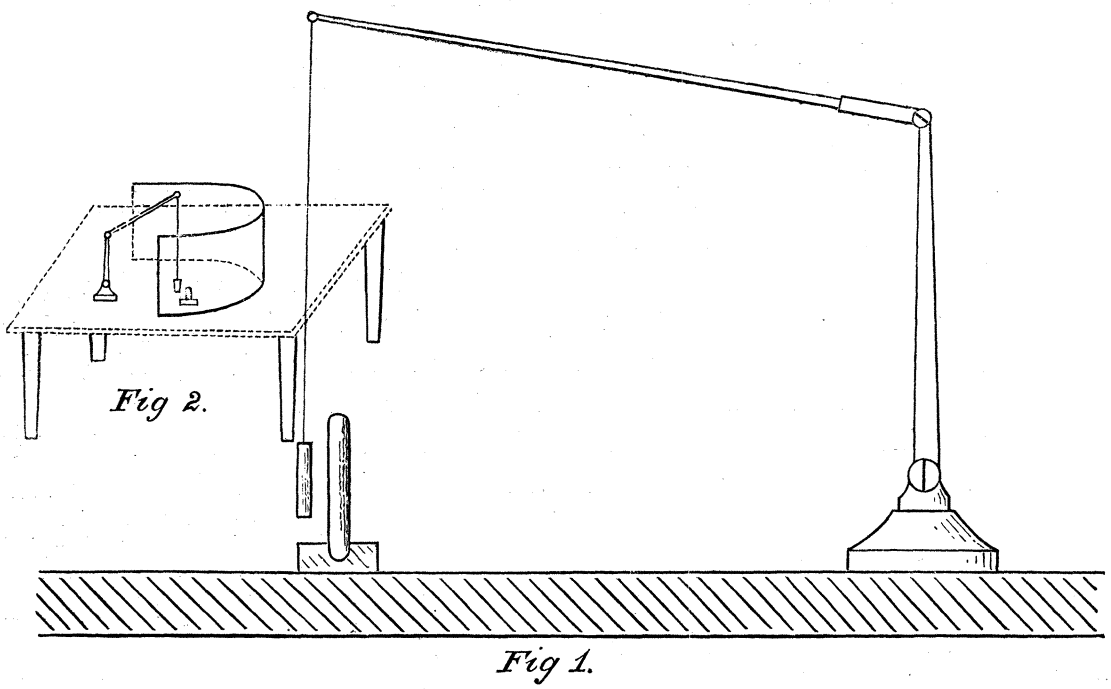
On February 15 last M. J. Thore communicated to a scientific society at Dax a short paper describing some results he had obtained on the rotation of a delicately suspended cylinder of ivory. So remarkable were these results that in a private letter to myself, accompanying a printed copy of his paper, M. Thore said "they seem to demonstrate the existence of a new force inherent in the human organism."

Figs. 1 and 2.



Figs. 1 and 2 are accurately copied from M. Thore's diagram. The following description of the apparatus and the experiments tried with it are translated from the printed paper.

"It consists simply of a cylinder of ivory, 24 mm. long and 5 mm. in diameter, suspended by a single fibre of cocoon silk, so that its axis is accurately in line with the suspending fibre. The fibre is fixed to a movable support allowing the cylinder to be

* 'Une Nouvelle Force?' Par J. Thore. Dax, 1887.

raised or lowered without sudden jerks which might rupture the fibre. The apparatus, in a word, is a small pendulum which hangs freely over the centre of a level table in the middle of a room having all the windows closed to avoid draughts.

"When thus arranged, if the cylinder is left to itself, after oscillating and rotating for some time, it becomes almost motionless. If desired, it can be steadied more quickly by lightly touching it with something. When steady, if a second ivory cylinder is gently brought about a millimetre from the first cylinder, vertical and parallel to it, as shown in the accompanying figure, the hanging cylinder is observed to acquire a movement of rotation, accelerating, and apparently only limited by the torsion of the fibre.

"This rotation always takes place in the direction of the hands of a watch when the second cylinder is on the left of the first in relation to the observer, who is supposed to face the apparatus; and in the contrary direction when the second cylinder is on the right. This double movement always takes place, whatever be the position of the observer round the table, when he approaches the second cylinder.

"This law is of remarkable constancy, for during the course of my numerous experiments I have never met with a single failure when I carefully took all precautions to avoid interfering influences.

"The nature of the substance of the two cylinders has no influence on the production of movement, and the same may be remarked as to their mass. With liquids or solids, full or empty, the rotation is always the same. The second cylinder may even be replaced by a single stretched hair, or by a single fibre of silk, which is still more fine, without there being any sensible modification.

"The speed of rotation is a function (1) of the length of the two cylinders; (2) of their nearness; (3) of the diameter of the first cylinder. It is in direct proportion to this length; it appears to vary inversely as the diameter of the first cylinder, and to diminish much faster than would be required by the law of inverse squares.

"Flat screens interposed between the experimenter and the apparatus, or placed the other side (at least when 20 centims. from the cylinders), interfere with the movement. When, on the contrary, they are arranged laterally on the right or left, or placed above or below, they are without action. A hemicylindrical screen placed behind the cylinders, as shown in fig. 2, so that the observer is opposite the opening, has the singular action of reversing the direction of rotation.

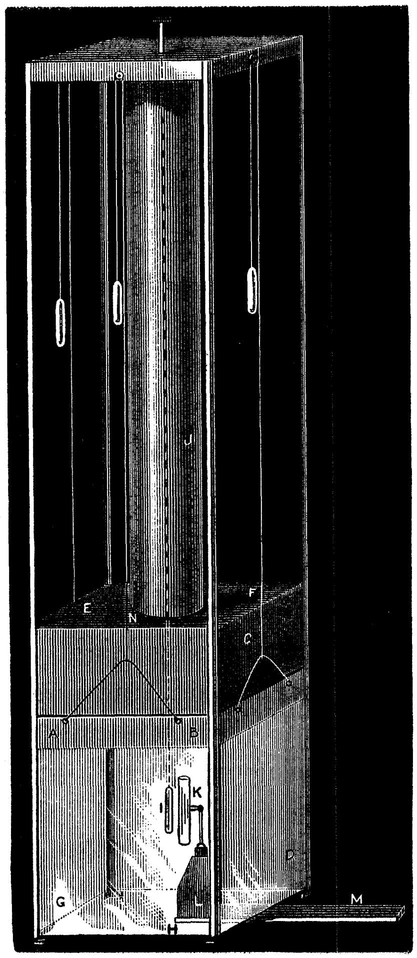
"I have ascertained that light is without action, whatever its nature, its intensity, or its direction. It is the same with heat. Neither can the action be attributed to electricity, for the cylinders, after immersion in water, are as active as before. The poles of a magnet are inert. The action of gravity is balanced by the tension of the suspending fibre. Lastly, air-currents cannot explain the remarkable uniformity of direction in which the rotation always takes place, nor its inversion in the case of the hemicylindrical screen.

"It is seen that the direction of rotation is closely connected with the position of

the observer ; this seems to indicate that the origin of this force is in the observer himself. If so, what is its nature ?"

Rough preliminary experiments having enabled me to verify the broad facts of rotation as described by M. THORE, I fitted up a more accurate apparatus (fig. 3), consisting of a glass case, ABCDEF, six-and-a-half inches square and seven inches high, with a rising glass window, ABGH, in front, and similar windows at the sides. The top is of card, in the centre of which is a small hole. The cylinder, I, is suspended

Fig. 3.



in the middle of the case by a very fine cocoon silk fibre, 5 feet long, surrounded by a card tube, J, attached to the top of the glass box. K is a second cylinder attached to a support, LM, by a ball-and-socket joint for convenience of adjustment. The support, M, projects outside the case to admit of the second cylinder (which I shall call the *pillar*) being brought close to the suspended cylinder (which, for distinction, I shall call simply the *cylinder*) and transposed from one side to the other, &c., without opening the windows of the box. N is a cord attached to the front glass window, weighted

at the end, and passing over a pulley for convenience of raising and lowering the glass. A similar arrangement is attached to the other glass windows. Cotton-wool is put at the bottom of each glass window and round the support M, to keep out air draughts.

The cylinder is attached to the end of the fibre with a loop and hook, so that it can be rapidly changed for other cylinders. The fixed pillar screws to the arm, so that it also is easily changed. Ivory, ebonite, glass, and metal have been used for the cylinders, and ivory, ebonite, brass, and wood for the pillars. The pillars have also been made square, round, and wedge-shaped in section, and the surfaces have been bright and lampblacked. The apparatus was fitted up in a room free from draughts and quick changes of temperature, and during the course of the experiments no one but myself entered the room.

The cylinder mostly used was of ivory, 5 mm. in diameter and 25 mm. long. The first pillar experimented with was also ivory, 7 mm. diameter and 30 mm. long. The mode of experimentation was the following:—The cylinder being at rest, I sat down in front of the apparatus with my face 8 inches from the cylinder and pillar, taking precaution to keep the breath as much as possible away from cylinder and pillar. The pillar was always placed on the right of the cylinder. On raising the front glass the cylinder commenced to rotate in the opposite direction to the hands of a clock, the side nearest me moving to the right.* It made 4·5 complete revolutions, the maximum speed being one revolution in 12 seconds.

In several succeeding experiments a four-ounce flask of boiling water was used as the source of heat. It was coated externally with lampblack, and was placed exactly one inch from the pillar and cylinder.

In other experiments the ivory pillar was replaced by one of hollow brass, 9 mm. in diameter and 38 mm. long, the surfaces being brightly polished; by a wedge-shaped pillar of boxwood, 38 mm. high, 22 mm. broad, 9 mm. at the thickest end, and tapering off to a blunt edge at the thinnest part; by an ebonite cylinder 9 mm. in diameter and 38 mm. long; and by a brass parallelogram 38 mm. long by 7 mm. square.

The results are given most conveniently in a tabular form. The first column of the following Table gives the numerical order of the experiment, the second column shows the material of which the pillar consists. In the third column is given the maximum speed in seconds of one revolution; the fourth column gives the number of revolutions performed by the cylinder before being stopped by torsion of the fibre; and the exciting agent, the face or hot-water flask, is shown in the last column. In all cases the rotation was *negative*, namely, in the opposite direction to the hands of a clock.

* I will call this the *negative* direction, and when the rotation is clockwise I will call it *positive*.

No. of experiment.	Material of pillar.	Maximum speed of one revolution.	No. of revolutions.	Exciting agent.
		seconds.		
1	Polished ivory	12	4·5	Face
2	Polished ebonite	16	3·50	,,
3	Boxwood wedge, edge to cylinder . .	41	3·25	,,
4	,, ,, flat side to cylinder .	32	3·00	,,
5	Polished brass tube	39	3·0	,,
6	Polished square brass rod, flat side to cylinder	24	3·5	,,
7	Polished square brass rod, edge to cylinder	21	4·0	,,

Instead of using the radiation from the face as the active agent, I now employed the four-ounce flask, lampblacked outside, full of boiling water. This was put 2 inches from the cylinder and pillar, and the following experiments were tried with it:—

No. of experiment.	Material of pillar.	Maximum speed of one revolution.	No. of revolutions.	Exciting agent.
		seconds.		
8	Polished ivory	18	4·25	Hot water
9	Polished brass tube	. . .	1·0	,,

In these experiments a point which struck me as being remarkable was the greater action which took place when I held my face 8 inches off the cylinder than when the exciting agent was a lampblacked flask full of boiling water. M. THORE says that heat is without action, and that the origin of the force appears to lie in the observer himself. At first sight these results appear to favour this view. It must, however, be remembered that the circumstances are not such as would bring out in a marked manner any action due wholly, or in great part, to heat. White polished ivory, such as M. THORE used for the rotating body, is a very bad absorber of heat rays; and it is quite possible that the aggregate of heat rays absorbable by polished ivory, emitted by a few square inches of lampblacked glass at 100°, 2 inches off, might not be inferior in amount to those emitted by the much larger surface of moist skin 8 inches off. It seemed possible to put this action to a test by blackening the ivory cylinder. If the action was, as M. THORE seemed to think, one inherent in the human organism, and not an effect of heat, the effect of blackening the cylinder should not materially alter the relative effects of the face and the hot-water flask; the action of the boiling water should still be less than that of the face. If, on the other hand, the action was one in which radiant heat played the principal part, the effect of blackening the ivory cylinder would be to upset this ratio, and to give a decided preponderance in favour of the hot-water flask. The ivory cylinder was accordingly blackened by holding it over the

smoke of burning camphor. On re-suspension, the following series of experiments was tried:—

No. of experiment.	Material of pillar.	Maximum speed of one revolution.	No. of revolutions.	Exciting agent.
		seconds.		
10	Polished ivory	21	4·25	Face
11	" "	18	4·25	Hot water
12	Boxwood wedge, edge to cylinder . .	11	5·75	Face
13	" " " . .	15	5·5	Hot water
14	Polished brass tube	20	4·25	Face
15	Polished square brass rod, flat side to cylinder	37	2·75	"
16	Polished square brass rod, edge to cylinder	16	6·5	"
17	Polished brass wedge, edge to cylinder	10	7·5	"
18	" " "	7	17·5	Hot water
19	Glass rod, 65 mm. long, 1·5 mm. thick	11	5	Face
20	" " "	21	4·25	Hot water
21	Brass wire, 60 mm. long and 0·5 mm. diameter	20	2·25	Face
22	Brass wire, 60 mm. long and 0·5 mm. diameter	24	2·5	Hot water
23	Platinum wire, 65 mm. long and 0·4 mm. diameter	61	2·25	"
24	Platinum wire, 65 mm. long and 0·4 mm. diameter	34	4·5	Face
25	Fine glass fibre, 65 mm. long . . .	. .	0·5	"
26	" " " . . .	46	1·5	Hot water
27	Single fibre of cocoon silk, 50 mm. long	. .	0·75	Face
28	" " "	29	2·0	Hot water

This series of experiments having shown a decided increase of motion due to blackening the cylinder, another series was tried with the pillars as well as the cylinder coated with the soot from burning camphor.

No. of experiment.	Material of pillar.	Maximum speed of one revolution.	No. of revolutions.	Exciting agent.
		seconds.		
29	Ivory, lampblacked	18	4·0	Face
30	" "	7	10·25	Hot water
31	Ebonite, lampblacked	14	4·75	Face
32	" "	7	11·75	Hot water
33	Boxwood wedge, lampblacked, edge to cylinder	19	3·5	Face
34	Boxwood wedge, lampblacked, flat side to cylinder	28	2·75	"
35	Boxwood wedge, lampblacked, flat side to cylinder	4	16·75	Hot water
36	Brass tube, lampblacked	10	7·25	Face
37	" "	10	10·5	Hot water
38	" "	28	2·5	Candle 8 inches from cylinder
39	Square brass bar, lampblacked, edge to cylinder	16	6·5	Face
40	Square brass bar, lampblacked, edge to cylinder	8	8·0	Hot water
41	Square brass bar, lampblacked, flat side to cylinder	18	3·75	Face
42	Square brass bar, lampblacked, flat side to cylinder	8	8·25	Hot water
43	Brass wedge, edge to cylinder, lampblacked	8	11·5	"
44	Glass rod, 65 mm. long, 1·5 mm. thick, lampblacked	17	3	Face
45	Glass rod, 65 mm. long, 1·5 mm. thick, lampblacked	16	4·5	Hot water

These results leave little doubt that the action exerted by the face was due to the radiant heat emitted by it. M. THORE, however, considers that heat is without action. This being a most important point, and one which must be settled beyond all doubt, I devised the following experiments:—A large sheet of thick cardboard had an oval aperture 5 inches by 3·5 inches cut in the centre. This was fixed in front of the suspended cylinder, about 8 inches off. A Winchester quart-bottle covered with slightly damped brown paper was arranged on a stand close behind the aperture, and easily removable. By this arrangement I could compare the effect of a given surface of the face with that of the same surface of radiation from moist brown paper heated to the temperature of the face, which for this purpose was taken as 33° C. The cylinder in each case was of blackened ivory. The following Table gives the mean of a number of experiments:—

No. of experiment.	Material of pillar.	Maximum speed of one revolution.	No. of revolutions.	Exciting agent.
		seconds.		
46	Blackened brass tube	17	4·0	Face
47	" "	15	3·5	Bottle

Considering that it would be almost impossible to get absolute equality between the radiating power of the face and any other substance which could be used in the comparison, I think these results are quite near enough, especially when taken with those already tried, to prove that there is nothing special in the human organism, beyond the heat it radiates, to produce rotation of the cylinder.

I attempted to verify the experiment of M. THORE's in which he got reversed rotation by putting a hemicylinder behind the rotating cylinder. A half-cylinder of glass, $4\frac{1}{4}$ inches across and $4\frac{3}{4}$ inches high, was put behind so that the suspended ivory cylinder was in the centre of the curve. The following experiments were then tried, the cylinder, as in the other cases, being of lampblacked ivory :—

No. of experiment.	Material of pillar.	Maximum speed of one revolution.	No. of revolutions.	Exciting agent.
		seconds.		
48	Polished ivory	18	3·75	Face. With glass cylinder
49	"	12	4·5	Face. Without glass cylinder
50	"	. .	1·25	Hot water. With glass cylinder
51	"	18	4·25	Hot water. Without glass cylinder
52	Polished brass tube	. .	0·5	Face. With glass cylinder
53	" "	19	4·5	Face. Without glass cylinder

These results show that the effect of putting a half-cylinder as a screen behind the suspended cylinder does not produce exactly the effect described by M. THORE. It does, however, cause a marked diminution of action, and, had the cylinder and pillar been freely exposed to the air as in M. THORE's experiment, reversal of movement might have taken place. I found it difficult to try accurate experiments in the free air of a room, owing to the interference of air-currents. The deadening of motion in my experiments, and its reversal in the experiments of M. THORE, I attribute to the reflection of heat rays from the concave surface of the cylinder, and their concentration to a focus on the further side of the suspended cylinder.

The following experiments were made with the object of ascertaining what would really be the effect of an upward current of air on the suspended cylinder. A glass jet about 1 mm. in diameter was fixed vertically in front of the cylinder. To the jet was attached a long india-rubber tube, connected at the other end with a system of water-bottles in such a way that, by raising one of them, a gentle stream of air rose from the jet in front of the cylinder. The amount of air ascending could be varied at will from a scarcely perceptible current to a strong blast. A telescope was fixed near the bottles, some distance away from the cylinder, so that observations could be taken uninfluenced by the heat of the body. The blackened brass wedge, with its edge towards the cylinder, as in Expt. 43, was used as the pillar, and the lampblacked

ivory was used as the cylinder. The jet was half an inch from the cylinder, and a moderate stream of air issued from it. The cylinder revolved three times to the right and then two and a-half to the left.

Similar experiments were tried, placing the jet at different distances from the cylinder, altering the velocity of the air, and increasing the size of the orifice, but the results in all cases were of the same kind; the cylinder first rotating once or twice to the right, and then about the same amount to the left. No permanent movement of eight or ten revolutions could be got, neither by any modification of the draught could I see my way to produce any of the strong rotations easily obtained with hot bodies.

There is a general accord between these experiments, but the agreement between repetitions of the same individual experiment is not so close as I should like. All were performed in duplicate or triplicate, and the mean taken. Much of the discrepancy may be accounted for by great variations of zero owing to the silk fibre becoming warmed or absorbing moisture, and part may be due to the impossibility of bringing the pillar and cylinder exactly 1 mm. apart in every case.

Another noteworthy point is the non-accord between the maximum speed of one revolution and the total number of revolutions performed by the cylinder.

All the experiments tried so far show that the rotations are produced by radiant energy falling on the cylinder and pillar. Radiant heat (and in less degree light) falling on the lampblacked surfaces is absorbed, and increases the surface-temperature. There are two ways in which this increase of temperature may act :—

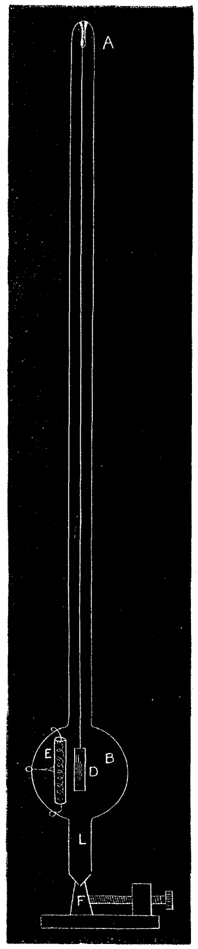
1. It may produce a current of warm air, rising in front of the surfaces of the moving body; to replace this, cold air will come in from all sides, and, striking against the delicately suspended cylinder, caused it to rotate. If, however, the source of heat is of considerable surface, such as the face, or a Winchester quart-bottle full of warm water, it is difficult to imagine that there would be much tendency to rotate in one direction rather than in the other.

2. An increased surface-temperature of the cylinder and pillar may produce an increase of molecular pressure between the two bodies, and thus give rise to motion, after the manner of the radiometer. In this, as in the former case, the movement should be in the opposite direction to what it is in reality, as it would be produced by mutual repulsion acting between the sides nearest the source of heat.

It seemed likely that information decisive as regards one or other of these two theories might be gained by suspending the cylinder in a glass tube attached to a SPRENGEL pump, and taking observations at different degrees of exhaustion. An apparatus was accordingly fitted up as shown in the accompanying figure (fig. 4). ABC is a glass tube, 39 inches long, expanded into a bulb, BC, at the lower end, and connected with a SPRENGEL pump at the upper part. An ivory cylinder, D, is suspended from A by a single fibre of cocoon silk, and at E is attached to the glass bulb a hollow brass tube, inside which, but not touching it, is a platinum spiral with the two ends sealed through the glass. By making battery contact between the

extremities of this spiral, the brass tube E can be heated. The upper end of the tube is clamped at A, and the lower end rests in a socket, F, capable of a little lateral adjustment by a screw. By this means the brass tube (the pillar) can be adjusted in respect to its distance from the cylinder D. Both E and D are lampblacked.

Fig. 4.



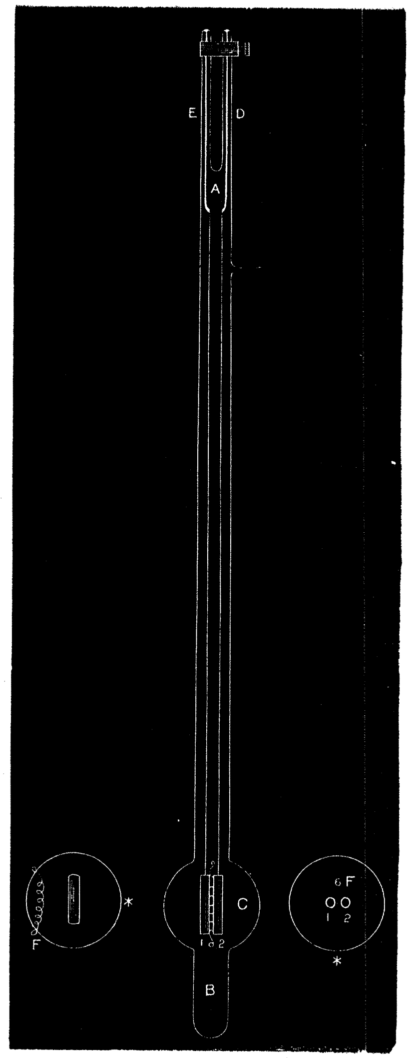
This apparatus is almost identical with one described and figured in my paper "On Repulsion resulting from Radiation" * (Part II., pars. 99, 100), only there the cylinder was of magnesium, and the platinum spiral was bare. With my old apparatus the noteworthy fact was ascertained that the incandescent spiral attracted the suspended cylinder to a moderate extent at normal atmospheric pressure; the attraction diminished to a minimum between a tension of 50 mm. and 150 mm., then rose as the pressure diminished, until, at a tension of 1·15 mm., the attraction was nearly four times what it was in dense air. Above this exhaustion the attraction suddenly dropped and changed to repulsion, and at the best vacuum I could get the repulsion was nearly thirteen times stronger than the attraction in air.

With the present apparatus I was able to verify the broad phenomena formerly

* 'Phil. Trans.,' 1875, pp. 528–532.

obtained of attraction in air and stronger repulsion in high vacua, although, owing probably to the blackening of the cylinder and pillar, or to the diminished sensitiveness of the apparatus, I could detect no repulsion at intermediate pressures. Igniting the spiral, so as to make the brass tube E hot, produced attraction or repulsion according to the degree of exhaustion, but it produced no rotation. Rotation, however, could easily be obtained by placing a flask of boiling water in front of the bulb, close

Fig. 5.



to a point equidistant from E and D ; and applying a gas-flame to this part of the bulb produced a still stronger effect. Experiments were carried out with this apparatus, gradually raising the exhaustion to a very high point, and noting the repulsion by the spiral, and the rotation by externally applied heat. The two phenomena ran absolutely in parallel lines ; when there was attraction of D to E, I could also produce negative rotation of D ; when the exhaustion was such that the attraction was *nil*, the rotation was *nil* also ; when the attraction changed to repulsion,

the rotation changed from negative to positive; and when the vacuum was so good that the repulsion between the two heated bodies was at its maximum, then also the positive rotation was the strongest. It was impossible to resist the conclusion that the two sets of phenomena were due to the same cause, and that, as air-currents did not produce the old attractions of the magnesium pendulum, so likewise were they equally inoperative in giving rise to the present rotations of the suspended cylinder. I will not give in a tabular form the observations taken with this apparatus, as more decided results were obtained with a modified form of apparatus which I will now describe, and it is not worth while to record observations beyond what are needed to prove the case under discussion.

If the rotation is produced by a reaction between the suspended and fixed body, it follows that, were both free to move, each would rotate, but in opposite directions. A modification of the form of apparatus last used was therefore devised; it is shown in fig. 5. It consists of a long glass tube, AB, having a bulb, C, blown near the lower part. At the top two narrower glass tubes, D, E, are blown on; these contain glass rods sealed to the tubes at the upper ends, but, in other respects, loose in the tubes. To the ends of these rods are attached two fine silk fibres, each having a cylinder of blackened ivory, 1, 2, suspended to it. F is a platinum spiral, equidistant from the two cylinders. The small tubes, D and E, are clamped at their upper ends by a brass band having a screw at one side. By tightening or liberating this screw the tubes are more or less inclined to one another, and the cylinders, 1, 2, can thus be adjusted to any desired distance apart. Exhaustion was effected through a lateral tube. Observations were taken at intervals during exhaustion, the source of heat being either a flask of hot water or a non-luminous gas-flame applied to the glass bulb at the place marked by an asterisk. In all cases when rotation was obtained the two cylinders moved in opposite directions. Thus, in air of ordinary density, cylinder No. 1 rotated counter-clockwise, while No. 2 rotated clockwise; I shall designate this movement as *negative*, and the opposite rotations, where No. 1 rotated clockwise and No. 2 counter-clockwise, I shall call *positive*.

The following Table exhibits, in a convenient form, the results obtained with this apparatus :—

Barometer = 767 mm.

No. of experiment.	Pressure.	Direction.	Maximum speed of one revolution.	No. of revolutions.	Exciting agent.
	mm.				
54	400	Negative	47 seconds	2·25	Hot water
55	338	,,	50 ,,	2·15	,,
56	220	,,	52 ,,	1·75	,,
57	178	,,	61 ,,	1·50	,,
58	129	,,	130 ,,	1·00	,,
59	100	,,	160 ,,	0·75	,,
60	80	,,	Slow	0·75	,,
61	50	,,	,,	0·25	,,
62	30	,,	Very slow	0·20	,,
63	20	,,	Still slower	0·10	,,
64	14	,,	,,	..	Gas flame
65	14	,,	Just visible	..	Hot spiral
66	14	0	No movement	..	Hot water
67	8	0	0	0	{ Gas flame, Hot spiral, Hot water }
68	4	0	0	0	,,
69	3	0	0	0	,,
70	1·5	Positive	50 seconds	2·0	Gas flame
71	0·75	,,	20 ,,	3·00	,,
72	0·50	,,	..	7·00	,,
73	0·30	,,	..	10·00	Hot spiral
74	0·129	,,	..	13·00	,,
75	0·0495	,,	..	13·00	,,

It will be observed, on comparing these results with those obtained in 1875,* that the neutral point here is between 8 mm. and 3 mm., whereas in the former case it was between 0·8 mm. and 0·3 mm. I, however, attach little importance to this, as the older apparatus was much more sensitive than the one now used.† The important fact is that in each case the direction changes at a high exhaustion, and then the movement becomes five times as strong as it was originally.

The motive force producing these rotations is, at high exhaustions, the molecular impacts between adjacent surfaces of the suspended cylinders excited by the radiation falling on them from the hot water, hot spiral, or a candle (which is equally effective).

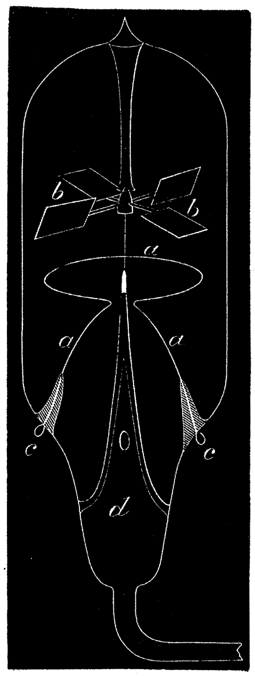
* *Loc. cit.*

† "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. 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 have not attained the neutral point until the exhaustion was within a very small fraction of a millimetre; whilst, if the metal is in the form of thin foil, the neutral point may easily be got lower than with pith." "On Repulsion resulting from Radiation. Part II." (March, 1875.) 'Phil. Trans.,' vol. 165, p. 540.

But what produces the negative rotation at ordinary atmospheric pressure? *Air-currents* are the obvious explanation, but there are grave reasons for believing this explanation to be inadequate. In the first place, actual air-currents, when tried, did not produce the desired result. Secondly, it is most logical to assume that, as the present set of experiments are strictly paralleled with those tried in 1875, and as the results at high exhaustions are in each case due to molecular bombardment, so also must the similar results at low exhaustions be due to the same cause.

My series of papers on "Repulsion resulting from Radiation"* contain numerous observations of attraction in air of ordinary density or at low exhaustions; and in the

Fig. 6.



Bakerian Lecture for 1878 I described an apparatus devised with the object of distinguishing between the action of air-currents, and of attraction in air of low exhaustion, and the repulsion in air at high exhaustions. In the following description I have condensed the experiments tried in 1878, and have added other results obtained subsequently with a similar piece of apparatus.

The apparatus is shown in fig. 6†; it consists of a cylindrical glass vessel sealed at the top, drawn off narrow at the other end, and having a stem, *d*, sealed in to hold a needle-point. The vessel is connected with the Sprengel pump by the narrow tube

* 'Phil. Trans.,' 1873, pp. 501–527; 1875, pp. 519–547; 1876, pp. 325–376; 1878, pp. 243–318; 1879, pp. 87–134.

† To avoid unnecessary complication, I have omitted from the drawing parts not used in the present series of experiments.

at the lower end. Round the needle is a ring of platinum wire, *a, a, a,* sealed into the glass and connected with outside terminals, *c, c.* A current of electricity from two Grove cells, turned on and off by a contact key, gives the power of making the wire red-hot when required. The fly consists of four thin vanes of clear mica, *b, b,* supported on light aluminium arms, and has in the centre a small glass cap, which rests on the needle-point. The vanes are inclined at an angle of 45° to the horizontal plane. They are in such a position that, when rotating, the centres of the vanes pass along the platinum ring and keep about 5 mm. distant from it.

In describing the direction of rotation of the fly, I shall consider the observer's eye to be on a level with the plane in which the fly rotates, and the direction recorded will be that taken by each vane as it passes in front. Assuming that the fly is rotating in the direction of the hands of a watch held face upwards on the top of the apparatus, each vane will be foreshortened, and, passing the observer, will have the appearance of /. The direction of rotation in this case will be considered as *positive, i.e.,* as the direction followed by the fly, were molecular pressure or a molar wind to proceed from the platinum wire.

In air of ordinary pressure (Bar. = 760 mm.), on igniting the platinum ring to redness by a current from two Grove cells, the vanes rotate in the positive direction, such as would be produced either by air-currents or by molecular pressure from the platinum ring.

The following experiments were tried :—

No. of experiment.	Pressure.	Direction of rotation of vanes.	Number of revolutions per minute.
	millims.		
76	760	Positive	13
77	200	Positive	7
78	100	Positive	2
79	30	Positive	Very slight when tapped
80	20	0	No movement
81	10	0	No movement
82	1	0	No movement
83	700 M*	Negative	40
84	450	Negative	30
85	300	Negative	20
86	200	0	No movement
87	138	0	No movement
88	110	Positive	18
89	98	Positive	33
90	54	Positive	60
91	17	Positive	150
92	10	Positive	600
93	5	Positive	1000
94	2·5	Positive	Increasing, but too quick to count.
95	0·5	Positive	,, ,, ,,
96	0·1	Positive	,, ,, ,,

Some points in this series of experiments are noteworthy. The vanes were arranged at such a slope in relation to the heated ring that the effect of the rising current of hot air should be a maximum. Owing to this, the first action of heat is to drive the vanes round in the positive direction, in opposition to the tendency to negative rotation which is almost always observed with air of moderate density. This antagonism lasts until a pressure of about 25 mm. is reached, when the two opposing forces balance and no movement is observed. After this point is reached the negative rotation continues till a pressure of about 250 M is reached, when it dies out, to be succeeded at an exhaustion of about 100 M by the positive rotation which all experiments show to be the natural direction for high vacua. The neutral point arrived at in Experiments 86 and 87 is, I believe, the analogue of the neutral

* M = one-millionth of an atmosphere, or 0·00015 mm. At low exhaustions I speak of millimetres of pressure, but at high exhaustions I prefer to count in millionths of an atmosphere. The inconvenience of using two units of measure is less than that of employing one system for both ends of the scale.

point seen at Experiments 66 to 69 when working with M. THORE's apparatus; in each case the negative rotation is slight before the neutral point is reached, while the positive rotation observed after neutrality increases rapidly in each case, until it eventually far exceeds the original movement.

In conclusion, I think I may consider as established by these experiments the following results :—

1. The broad facts of rotation as observed by M. THORE are abundantly confirmed.

2. The numerous experiments in which the face and hot water are tested under the same circumstances, and especially Experiments 46 and 47, prove that the action is due to radiation alone.

3. Blackening the cylinder increases the action. This is especially shown in Experiments 11, 12, 13, 14, and 15.

4. The action is slightly increased by blackening both cylinder and pillar.

5. The remarkable fact observed by M. THORE that a fine fibre of silk brought near the suspended cylinder produces rotation has been verified in Experiments 27 and 28, and with other fine fibres in Experiments 19 to 26.

6. That the rotation is produced by a reaction between the cylinder and pillar, and not between the cylinder and the source of radiation, is shown in Experiments 54 to 75.

7. The hypothesis that the rotations are produced by air-currents is disproved partially by the experiments in which the effect of an ascending current of air is shown to be almost without action, and it is entirely disproved by Experiments 66 to 75, and 86 to 96, in which the movements become more energetic in proportion as the space in which they occur is exhausted of air.

8. The rotation takes place negatively in dense air, and positively in high vacua. It is proved beyond a doubt by Experiments 66 to 75 and 86 to 96 that the positive rotations are due to the same cause which produces rotation of the radiometer; *i.e.*, to molecular pressure caused by radiation falling on the blackened surfaces.* In all cases there is noticed strong action in very high vacua, diminishing as the vacuum gets less perfect, until a point is reached where there is no action. Below this neutral point movement recommences, but in the opposite direction to that observed at high exhaustions. This negative movement is common to M. THORE's phenomena, and to the whole series of phenomena investigated in my researches on "Repulsion resulting from Radiation." The explanation of the negative motion is, however, not clearly made out. But, from the strict parallelism between the two sets of phenomena, I have no doubt that the explanation which will account for the one will be equally adequate to account for the other.

In my sixth paper on "Repulsion resulting from Radiation,"† pars. 415, 416, I described apparatus in which negative rotation was produced at an exhaustion of 117 M, and positive rotation at an exhaustion of 0·18 M. This phenomenon is,

* 'Phil. Trans.,' 1876, pp. 375–376; 'Roy. Soc. Proc.,' vol. 25, 1876, p. 308.

† 'Phil. Trans.,' 1879, pp. 101–103.

I consider, perfectly explained in the paper by the "molecular bombardment" theory, and I, therefore, am justified in assuming that the negative rotations in M. THORE's apparatus will equally well be explained by the same theory.

ADDENDUM.

(Added May 24, 1887.)

I sent M. THORE a detailed account of my experiments, asking him to favour me with any comments or remarks he might wish to make, and offering to communicate them, if desirable, to the Royal Society. I have just received a long communication, partly printed and part in MS., in which he describes many fresh experiments, and adduces arguments to show that my dynamical explanation is not sufficient to account for more than a few of the facts he describes, and saying that he "persists in still believing that this force emanates from the observer, or else that the observer is the indispensable intermediary for its manifestation."

The experiments are numerous, and are devised with great ingenuity. It is impossible in the space of a brief abstract to do more than refer to a few of the principal facts here brought forward. M. THORE commences by objecting to my having experimented in an enclosed space, saying that he always operates in free air. He thinks that enclosure may almost or quite suppress his force. To this I can reply that I have myself verified nearly all M. THORE's facts of rotation (including those just now communicated) when working in the free air of a large room, and it was only when I found the delicacy of the observations was impeded by draughts and currents that I put screens round the apparatus. I have not found glass screens interfere materially with any of the rotations. M. THORE now says that it is necessary to hold the pillar or the exciting body in contact with the hand during the whole duration of the experiment. I was not aware that importance was attached to this point, but I have since repeated many of my former observations, holding the pillar in the hand. The results are certainly stronger; but the extra heat imparted to the apparatus is, in my opinion, sufficient to account for this. M. THORE brings forward many new and ingeniously devised experiments to prove that heat cannot be considered the cause of the movement. He exposes the instrument to the full sun, and then brings it into a cool dark room; he suspends it over boiling water; he places a large block of ice between the cylinder and the observer; he similarly interposes metallic vessels full of boiling water between the cylinder and observer (the observer not moving from his place in front), and he tries the experiment in a hot chamber, alternately moist and dry, without finding the regularity of the movements interfered with. I have tried most of these, and obtained results corroborating M. THORE's, but I have also tried the experiment of quietly bringing near to the stationary cylinder a bottle of hot water, and observing the movement from a safe

distance through a telescope, and I find that the hot bottle is able to effect rotation as well as the observer.

Among the curious observations mentioned by M. THORE is this:—Placing the pillar in front of the cylinder (between it and the observer), if the pillar is held with the right hand the movement is clockwise, and if the left hand is used the rotation is counter-clockwise. The right hand is stronger in its effects than the left hand in the proportion of 2 to 1.

M. THORE has given, in addition, a large number of curious and interesting observations, using two, three, and more movable cylinders, and recording their movements under a great variety of circumstances. I admit I do not see at once how all these are to be explained on the molecular bombardment theory. But this theory has not yet explained all the anomalous results I have recorded in my papers on "Repulsion resulting from Radiation," although I believe it capable of doing so; and I therefore think that it is not necessary to call upon a new force to explain any of M. THORE's results which radiation does not yet seem able to account for.