XXVIII. *On the Forces caused by the communication of Heat between a Surface and a Gas; and on a new Photometer.* *By* Prof. OSBORNE REYNOLDS. *Communicated by* B. STEWART, *F.R.S., Professor of Natural Philosophy in Owens College, Manchester.*

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IN a paper read before the Royal Society*, April 1874, I pointed out that the communication of heat from a solid surface to a gas, whether accompanied by evaporation or not, must, according to the kinetic theory, be attended by a reactionary force equivalent to an increase in the pressure of the gas on the surface, and, conversely, when heat is communicated from the gas to the surface the pressure against the surface is diminished; and I also suggested that these forces are the probable cause of the motion, resulting in some way from radiation, which Mr. CROOKES had brought into such prominent notice.

Since the publication of this paper neither my conclusions as to the existence of these "heat reactions," nor the reasoning by which I supported them, have been controverted or even questioned; but, on the other hand, they have received important confirmation. The results at which Professors TAIT and DEWAR arrived after a careful investigation fully bear out my conclusions, not only as to the existence of the forces, but also as to the way in which they explain Mr. CROOKES's experiments.

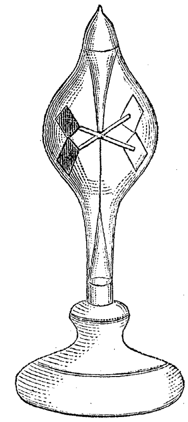
Still it seemed desirable, if possible, to settle the question by obtaining such quantitive measurements of the effects produced as would show whether or not they agreed with what might be expected from theoretical considerations. I have accordingly been on the look-out for some means of making these experimental verifications. Such a means I at length found in one of the beautiful little instruments constructed by Dr. GEISSLER, of Bonn, after the manner of Mr. CROOKES, and called by him "Light-Mills." As this instrument has taken an important part in the experiments I have to describe, I shall commence by giving a detailed description of it.

The Light-Mill.

This consists of a glass envelope in the shape of a pear, about $2\frac{1}{2}$ inches through its thickest part; standing up from its lower end is a steel needle, coincident with its axis. On the top of this needle is balanced (after the manner of a compass-card) the mill or wheel; this consists of a small central glass cup which rests on the point of the needle, and to which are fused four very thin platinum arms, which have their outer ends attached to four square plates (which appear to be talc or mica-schist) $\frac{1}{2}$ inch

* Roy. Proc. 1874, vol. xxii. p. 401.

square, fastened so as to stand vertically with a corner at the top. The distance of the centres of these plates from the axis is about $\frac{3}{4}$ of an inch. The plates are very thin, and are covered on one of their sides (which sides are all turned the same way) with lampblack.



Descending from the top of the vessel is a small tube, the function of which is to keep the wheel from falling from its pivot when the instrument is turned over. The air within the mill has been greatly rarefied; electricity will not pass; but more than this I cannot say.

The Action of the Light-Mill.

When placed in the light the mill quickly arrives at its maximum speed, and rotates continuously with a velocity depending on the intensity of the light. It will rotate steadily at speeds varying from 1 revolution in 6 minutes (in the light of the full moon) to 240 revolutions in a minute (in the strongest light I have been able to obtain).

When the mill is revolving, and the light is suddenly extinguished, it rapidly comes to rest.

These two facts, namely (1) that the mill rapidly arrives at its maximum velocity when the light is turned on, and (2) that it as rapidly comes to rest when the light is turned off, are those to which I wish first to direct attention, for they appear to me to prove conclusively that *the air within the envelope does exercise influence on the mill.*

(1) If it were true, as has been supposed, that the best results are obtained in a vacuum so perfect that there is not sufficient air to exercise any influence on the vanes of the mill, then it follows that the mill would move without experiencing any resistance from the air, and the only known resistance would be the friction of the pivot. Now whether or not this is the case is easily ascertained. The resistance of the pivot, whatever may be its magnitude, does not increase with the speed of the mill, and hence does not oppose a greater resistance to its motion when it is turning fast than when it is turning slowly. The friction of the air, on the other hand, increases rapidly with the velocity. There is therefore a difference in the manner in which these two resistances will affect the motion of the mill. If the mill were only subject to the resistance of the pivot, any force which would start it would continue to turn it with increasing velocity as long as it acted; whereas, when subject to the resistance of the air, the resistance increasing with the speed, the mill would soon arrive at such a speed that the resistance balanced the turning force; after which the motion would be steady. This difference in the action of the friction of a pivot and that of the air is well known in mechanics and utilized, as, for instance, in the striking part of a clock. If prevented by nothing but the friction of the spindles when the clock is striking 12 say, each stroke would follow after a less interval than the previous one. Now the invariable means by which this is prevented is by a fan like the wheel in the light-mill, which, by the resistance it experiences in moving through the air, prevents the clock striking at more than a certain rate.

Now, from the description of Mr. CROOKES's instruments which he has published, it appears that they, like the one which I possess, arrive at a constant velocity depending on the intensity of the light. Hence it may be fairly inferred that in them the motion of the wheel is restrained by the same resistance as in mine; and that this resistance, as I have just shown, is not the resistance of the pivot.

(2) The limited velocity of these mills is therefore exactly what would be caused by the friction of the air, just as in the clock: but there is another conceivable cause of the limit; and this is, that the force which causes the motion diminishes with the velocity. Fortunately, however, there is another test by which the resistance may be examined, a test altogether independent of the action of light or heat. This is the rate at which the mill comes to rest when the light is turned off. If the pivot were the only source of resistance the time required for the mill to come to rest would be as the speed; that is to say, if it required 15 seconds for the mill to come to rest when making 10 revolutions per minute, it would require 150 seconds to come to rest from 100 turns per minute. In fact, however, my mill, which requires 15 seconds to come to rest from 10 revolutions, does not take 30 to come to rest from 100 revolutions. In these experiments the wheel was set in motion by turning the envelope, and not by the aid of light or heat. We have, therefore, conclusive evidence that the resistance is not merely that of the pivot (which, in fact, is so small as to be inappreciable); and the only other resistance of which we know* is that of the air. But this is not all.

The behaviour of the mill furnishes us with the exact law of the resistance; and this is identical with the law of the resistance of air in a highly rarefied condition, a law distinctly special in its character.

The resistance which bodies experience in moving through the atmosphere at considerable velocities is proportional to the square of the velocity; but if the velocity is very small, less than one tenth of a foot per second, then, as Prof. STOKES has shown, the resistance is nearly proportional to the velocity. Now, so far as this latter resistance goes, Prof. MAXWELL has shown the singular fact that, although it depends on the nature, it is independent of the density of the air or gas. A body moving at a very small velocity would therefore experience the same resistance whether moving outside or within the receiver of an air-pump in which the air was highly rarefied, the only difference being that the speed for which the resistance continues proportional to the velocity is higher in proportion as the tension of the air is reduced.

If, therefore, the vanes of the light-mill were moving in air as dense as the atmosphere they would experience a resistance increasing with this speed according to a law varying from the velocity at low speed to the square of the velocity at high speeds; but since they move in an exceedingly rare medium, the resistance which it offers is more nearly proportional to the velocity throughout, and only at the highest speeds can there be any appreciable deviation from this law.

* Ethereal friction, if it exists at all, must be too small to produce any appreciable effect, and it is not probable that it would follow the same law as air.

The limit which this resistance would impose on the speed would, at low speeds, be very simple; the velocity would be proportional to the force causing it.

If the light from each of two candles would cause the mill to turn with a certain velocity, then the two candles acting together should cause the mill to turn with double velocity; and this is exactly what happens, as the following Table shows:—

Distance from the candles in feet.	Number of revolutions per minute.	
	1 candle.	2 candles.
2	1·2	3
1	$5\frac{1}{2}$	11
$\frac{1}{2}$	23	36
$\frac{1}{4}$	65	120

It will be seen that at very small velocities the effect of 2 candles is rather more than double that of 1; this is owing to the friction of the pivot, which is constant.

Also at the higher velocities there is a falling off in the speed, exactly as might be expected from the air. Hence we see that the force which limits the speed of the mill, follows the same complicated law as that of the resistance which would result from the friction of the air; and hence there cannot be a doubt but that they are the same.

The Force which turns the Mill is not directly referable to Radiation.

With reference to the assumption that the force is radiant or in any way *directly* referable to radiation, I pointed out at Bristol, before Section A (Brit. Assoc.), that in any such supposition the results of the experiments are directly opposed to one of the fundamental laws of motion, viz. that action and reaction are equal. In these experiments a hot body causes a cold body to recede, while a cold body causes the hot body to approach; so that if both the bodies were free to move, we should have the cold body running away and the hot body running after it. This fact is, I take it, a conclusive proof that the force does not act from body to body, but between each body and the medium in which it is placed; that each body, as it were, propels itself through the surrounding medium in a direction opposite to its hottest side.

The truth of this reasoning has been set beyond all doubt by a very beautiful experiment made by Dr. SCHUSTER. The results of this he is about to communicate to the Royal Society; and as his paper will contain a full account of the experiment, it is only necessary here for me to refer to the results and the way in which they bear on the subject in hand. Dr. SCHUSTER suspended my light-mill by a double fibre, so that if undisturbed by any torsional force it would hang with the vessel always turned in one direction, but in such delicate equilibrium that the smallest torsional force would cause it to take a fresh position. In this way he was enabled to ascertain whether the action of light on the vanes of the mill was attended with any effect to turn the envelope.

Some such effect must have been caused whatever had been the nature of the force, either in the commencement or in the maintenance of the motion.

For, in the first place, if the force acting on the vanes arose from an external source, then the vanes in turning, owing to the friction of the pivot and the friction of the air, must tend to drag the envelope round with the mill; consequently, on the light being turned on, the envelope would have turned in the same direction as the vanes, and continued to do so until the torsion of its suspension had restrained its further motion: it would then have remained steady until the light was turned off, when it would have come back to its former position.

Whereas, on the other hand, if the force on the vanes arises entirely within the vessel, if the air is, as it were, the fulcrum against which the force acts, then, in order to overcome the inertia of the vanes and set them in motion, the air must itself move in the opposite direction, just as when a steamboat starts it sends a stream of water backwards. This motion of the air will be communicated by friction to the vessel, and the effect will be that on the light being turned on the envelope must turn in the opposite direction to the vanes; that when the mill has acquired its full speed then, as in the case of a steamboat, the backward motion given to the fluid by the propellers will just balance the forward motion imparted by the resistance of the ship, and the resultant force will be nothing. When, therefore, the mill has acquired its full speed the envelope will come back to its normal position, where it will remain until the light is turned off, when the friction acting alone will tend to drag the internal fluid and hence the envelope forward.

This was the view of the case which I took when Dr. SCHUSTER first suggested his experiment to me; and when it came to be performed the results, as may be seen, were in strict accordance with the second supposition, namely, that the force acts entirely between the vanes and the air within the mill.

This experiment of Dr. SCHUSTER'S also afforded a means of arriving approximately at

The Magnitude of the Force.

The weight of the mill and the envelope, considered in conjunction with its manner of suspension, gave the moment of the torsional force necessary to turn it through an angle of ·06 as ·0000000264 lb., or one forty-millionth part of a pound acting on a lever a foot long. To cause this deviation the light had to be such as would cause the vanes to make 240 revolutions per minute. Hence, when making 240 revolutions per minute, we have a measure of the force which causes the motion and the resistance which opposes it. Now considering that the centres of the vanes are $\frac{3}{4}$ inch from the axis, the whole force acting on the vanes will be $16 \times$·0000000264 of a pound,—that is ·00000042, or one two-million-five-hundred-thousandth part of a pound; this distributed over the vanes (whose joint area is 1 sq. inch) is ·00000042 lb., or one two-million-five-hundred-thousandth part of a pound on the square inch. And assuming that the tension of gas within the mill is ·0005 lb., or one two-thousandth part of a pound on the square inch (the tension of a toricellian vacuum at 60° F.), *then we see that the difference of pressure on the two sides of the vanes is* ·0008 *of the pressure within the mill, or less than one thousandth part.*

These results, although they do not pretend to be more than approximate, show how exceedingly small is the real effect, and they place these phenomena of motion caused by heat in a light from which the exceeding delicacy and sensitiveness of the instruments have altogether withdrawn them.

The difference of Temperature.

Having obtained these measurements of the force, it remained to see what difference of temperature would be necessary, according to the kinetic theory, that the reaction from the communication of heat might equal these forces, and then to ascertain how far such a difference of temperature actually existed. To do this I have had to enter upon new and somewhat doubtful ground: however, I venture to submit the following, which, although it contains assumptions, contains none but what are legitimate and strictly in accordance with the kinetic theory.

Theoretical Difference of Temperature.

Whatever may be the nature of the action by which heat is communicated from a surface to a gas, the result, according to the kinetic theory, is to increase the mean square of the velocity with which the molecules move in the ratio of the temperature: thus, if v be the initial velocity, and τ the initial absolute temperature, and if

$$v^2 = A\tau,$$

where A is a constant depending on the nature of the gas, then

$$(v+dv)^2 = A(\tau+d\tau),$$

or, neglecting dv^2 as a small quantity,

$$A d\tau = 2v dv,$$

$$d\tau = 2dv \frac{\tau}{v}.$$

Now, if we assume that each molecule comes up to the surface with a velocity v, and leaves with a velocity $v+dv$, we shall have the greatest reactionary force which it is possible that the heat could produce. That the force produced is as large as this is not probable. We know that at ordinary densities the molecules communicate the heat to each other, so that they do not come up to the surface with so small a velocity as v. The smaller the tension of the air, however, the less will be the difference; so that the force which we have assumed is the limit towards which its force tends as the vacuum improves, so long as the conditions of a perfect gas are fulfilled*. The increase dv in the velocity with which the molecules leave the surface would increase the pressure in the ratio

$$\frac{2v+dv}{2v},$$

or

$$\frac{p+dp}{p} = 1 + \frac{dv}{2v},$$

* Proc. Roy. Soc. 1874, vol. xxii. p. 407.

$$\therefore \frac{dp}{p}=\tfrac{1}{2}\frac{dv}{v};$$

and by the foregoing

$$\frac{dv}{v}=\tfrac{1}{2}\frac{d\tau}{\tau},$$

$$\therefore \frac{dp}{p}=\tfrac{1}{4}\frac{d\tau}{\tau}.$$

Therefore if, as we have calculated,

$$\frac{dp}{p}=\cdot 0008,$$

$$\frac{d\tau}{\tau}=\cdot 0032,$$

and taking $\tau=520°$ F.,

$$\therefore d\tau=1\cdot 6640.$$

If, therefore, the difference of temperature caused by the light were not greater than 1°·7 F., it would appear from these measurements that the forces arising from the communication of heat would not be adequate to cause the effect produced. That is to say, 1°·7 is the lowest limit that the theory admits for the heat reaction to have caused the effects in this particular case. The theory points to the probability, however, that the difference was considerably greater than 1°·7.

To put this to the test it was necessary to obtain some measure of the

Actual Difference of Temperature on the Black and Bright sides of the Plates.

So far as I am aware there is no recognized means of measuring this difference; and although it is admitted that a black surface exposed to light will attain a higher degree of temperature than a white or bright surface, no comparative experiments have been made.

While taking part with Dr. SCHUSTER in his experiments, I held an ordinary thermometer containing some dark red fluid in the place which the mill had occupied exposed to the light. This came from a lime-light, and was condensed by an ordinary lantern.

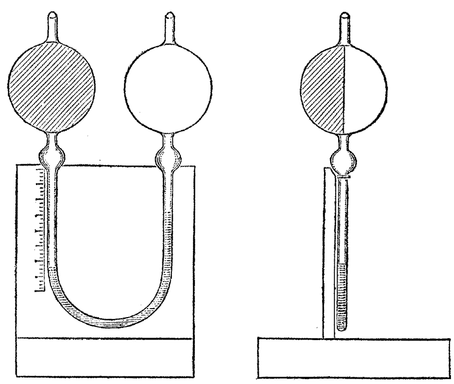
The thermometer rose to 130° F., and was still rising when the experiment had to be discontinued.

This measure, great as it was, was not satisfactory, for it was not comparative, and a white-bulbed thermometer would obviously have risen to some extent. I therefore took two similar mercurial thermometers, blackened the bulb of one and whitened that of the other, and exposed them to similar intensities of light. Under all circumstances the black bulb was the most affected, for however long a time the exposure was continued; the light of a candle which caused the light-mill to make 30 turns per minute made a difference of $2\frac{1}{2}°$ in the thermometers, whereas a feeble sun, which gave the mill about 60 turns, caused a difference of 5°. These results showed a close agreement with the action of the light-mill; but whereas the light acted instantaneously on the mill, the thermometers did not show signs of moving for some time. It also seemed probable that the immediate surface which was exposed to the light, besides coming to its temperature almost instantaneously, would probably assume a higher temperature than that which would be communicated through the material. In order to show this it occurred to me to construct

A New Photometer.

This instrument consists of two very thin hollow glass globes, $2\frac{1}{2}$ inches in diameter, connected by a siphon-tube $\frac{1}{8}$ inch internal diameter.

One of the globes was blackened *on the inside* with lampblack over one hemisphere, and the other was whitened with chalk in a similar manner; the siphon-tube was filled with oil, the air within the globes was carefully dried, and they were sealed. The two clean sides of the globes are turned in the same direction, so that any light entering through these clean sides falls equally on the blackened and whitened surfaces within. The air within instantly commences to receive heat in proportion to the temperature of these surfaces, and, expanding, moves the liquid in the tube.



By comparing the volume of a certain length of the tube with the volume of the globes, the distance which the liquid moves for 1 degree difference of temperature has been found: 1 inch means 2·2 degrees. A scale having been fixed to the tube, the effect of light to cause a difference of temperature in the air can be read off.

There is, however, still one difficulty: the air within the globes does not arrive at the temperature of the surfaces, as these do not entirely enclose it. All that can be said is that it is proportional, probably about $\frac{1}{4}$ or rather more.

This difference may, however, be set off against the difference which must exist in the mean temperature of the vanes of the mill, and what it would be if they remained steadily perpendicular to the light. As it is, each part of the surface of the vane is only exposed to the light for half its time, and then at varying angles; so that the light that it receives bears to the light which would fall on it, if fixed and perpendicular, the ratio of the diameter to the circumference of a circle, *i. e.* the ratio $\frac{1}{\pi}$. In the case of the photometer the ratio of the section of the intercepted beam to the whole surface of the sphere is that of the area of a great circle to that of the sphere, or $\frac{1}{4}$; so that it is probable the photometer only registers $\frac{3}{4}$ the difference of temperature which similar surfaces would acquire on the mill.

The white surfaces on the mill, however, are not similar to those of the photometer, and they probably absorb considerably more light, and consequently diminish the difference of temperature; so that, on the whole, it is probable that the differences recorded by the photometer are quite as great, if not greater than those which exist in the mill.

The instrument is very sensitive, and begins to move as soon as the light falls on it. Its indications agree surprisingly with those of the light-mill: 1° on the photometer corresponds with 11 revolutions per minute of the mill. When the mill made 200 revolutions per minute, the reading on the photometer was 21°, which is the highest it will record. Differences to $\frac{1}{10}$ of a degree can be read on the photometer, or the effect of light which will turn the mill at 1 revolution per minute. It can be used, therefore, for all purposes of photometry for which the mill may be useful. It is much more convenient, as it requires no counting, and it can be made with much less trouble.

Measured by this photometer, the difference of temperature in Dr. SCHUSTER'S experiment would have been 24°. This, which must be looked on as an outside measure, leaves ample room for allowance for the inaccuracy of the calculation. We have, on the one hand, the least estimated heat 1°·7, and the greatest limit of the measured heat 24°, and the probability that both these quantities tend towards each other.

Conclusion.

The investigation of which this paper gives an account was undertaken with a view to settle the only point respecting my previous explanation of the motion caused by heat which appeared to me to remain doubtful, after I had discovered that, according to the kinetic theory, the communication of heat to a gas was attended by a reaction on the surface, viz. whether this reaction was adequate in amount to produce the motion. This point has now been cleared up. We have:—

1. The remarkable agreement between the law of the resistance experienced by the mill and the peculiar law of the resistance which air offers at small tension.

2. Dr. SCHUSTER'S positive proof that the force which acts on the vanes arises within the mill itself.

3. The exceedingly small magnitude of the actual force, as shown by quantitive measurements.

4. The fact that the estimated difference of temperature necessary to produce heat-reactions of equal magnitude with the forces which act is well within the difference of temperature actually found to exist.

Taking all these facts into consideration, it seems to me that the evidence is conclusive as regards the nature of the forces which cause the motion in light-mills, and that we may now look upon the motion caused by light and heat as a direct proof of the kinetic or molecular theory of gas.

A new Light-Mill.

Although the proofs against the forces in the light-mills being directly referable to radiation are already more than sufficient, I will venture to suggest one more test, which the difficulty of obtaining the instrument has as yet prevented me applying. If a "light-mill" were made unlike those which have hitherto been constructed, inasmuch that, instead of its vanes being perpendicular to the direction of motion, and having one side black and the other white, it has vanes arranged like the sails of a wind-mill or the screw of a ship, all inclined to the direction of motion, and of the same colour on both sides; then if this mill turned, it would show that the force is not influenced by the direction from which the light and heat come, but that, like the wind on a wind-mill, it acts perpendicularly to the surface of the vanes*.

It seems to me that, inasmuch as the vanes of such a mill would be continuously acted upon, and would experience the full and not merely the differentiated effect of light, it would be much more sensitive than those at present constructed.

APPENDIX (March 7, 1877).

Vanes fixed in the Envelope.

In the discussion which followed the reading of this paper, it was stated by Mr. CROOKES that he had suspended his instruments upside down by a single fibre, and floated them upside down in water, and had then found, when the vanes could not turn in the envelope, that the whole envelope rotated very slowly under the action of light, steadily and continuously in the same direction as that in which the vanes would have turned had they been free. And at the Meeting on March 30th, subsequent to the reading of this paper, Mr. CROOKES described how the case of one of his instruments floating in water revolved at a rate of about 1 revolution an hour when the vanes were free to turn. Comparing this effect with that which was caused when the vanes were fixed by the magnet one revolution in 2 minutes, it appears that the force turning the envelope with the vanes free was $\frac{1}{30}$th that turning the vanes; for the resistance of the water at such small velocities would be proportional to the velocity.

As no such effect to turn the envelope had been observed during Dr. SCHUSTER'S experiment, in which I took part, and as it was difficult to conceive any method of

* In the discussion which followed the reading of the paper, Mr. CROOKES mentioned that he had already constructed mills with inclined vanes, and found them answer; and I am informed that he exhibited one at the next meeting of the Society. I may mention here that I have received a mill from Dr. GEISSLER, which I had previously ordered. This instrument, although damaged in transit, is sufficiently sensitive to prove that the action of heat is altogether independent of the direction from which the heat comes.—July 31, 1876.

suspension more delicate than that then adopted, I was forced to believe that the effect found by Mr. CROOKES was due to some accidental cause, such as air-currents, about the outside of the case of his mill. I therefore repeated Mr. CROOKES's experiment; first, by floating the mill, as he describes, in a beaker of water, and simply covering the whole with a glass shade. I then found that it was impossible to bring sufficient light to bear on the mill to cause the vanes to revolve without causing the case to turn; although this turning was irregular, and such as might be caused by air-currents. Dr. SCHUSTER and myself then suspended the same light-mill we had previously used in a manner in all respects similar to that of his former experiments, except that the mill was upside down, so that the vanes could not turn in the envelope. On the light being turned on a certain amount of disturbance was always consequent so long as the receiver was not exhausted; but when the receiver was exhausted to about $\frac{1}{2}$ inch of mercury, no motion at all could be observed. At the soirée given by the Royal Society on the 14th of June, 1876, I had two mills suspended, the one upright and the other reversed. The envelope of the upright mill moved when the light was turned on through a distance represented by several hundred divisions of the scale; but the reversed mill showed no motion at all, although a motion of two divisions must have been perceived. The mills were suspended in vessels from which the air had been pumped until the pressure was about half an inch of mercury. In these experiments, therefore, there was no residual force tending to turn the envelope with the mill so great as $\frac{1}{100}$ the force on the mill.