PAPERS

ON

MECHANICAL AND PHYSICAL SUBJECTS.

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ON

MECHANICAL AND PHYSICAL SUBJECTS

BY

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PREFACE.

HAVING found the various reprints of papers by the same author, which have been published during the last thirty years, of the greatest assistance, and being assured that it would be a convenience to some of my scientific friends if the papers on mechanical and physical subjects, which I have communicated to various societies and scientific journals, were published in a collected form, also having secured the able assistance of Mr Charles B. Dewhurst, M.Sc., in collecting and arranging the papers and correcting the press, I gladly availed myself of the opportunity afforded me by the liberality of the Syndics of the University Press of having papers I have written between the years 1869 and 1900 reprinted in a collected form. These include all the papers which I have published in the transactions and journals, with the exception of certain abstracts of papers which were printed in full at somewhat later dates, six short papers of only temporary interest, and a Memoir of James Prescott Joule which is published separately, being Vol. VI. Fourth Series of the Memoirs and Proceedings of the Manchester Literary and Philosophical Society. The titles and text of the first forty of these from 1869 to 1882 are included in this volume.

In reprinting the papers errors resulting from inadvertence have been corrected, where discovered; but otherwise there have been no alterations nor have any notes been added.

The chronological order has been followed in arranging the papers notwithstanding that it entails a somewhat excessive amount of discontinuity in the sequence of papers on the same subjects. With a view to obviate the inconvenience of this discontinuity, in addition to the references back to the earlier paper, references forward to the papers in which the subjects are continued have been added.

As affording some explanation of the absence of any connection between many of the subjects in this collection of papers it may be pointed out that these subjects have not been determined by arbitrary selection, neither have they been the result of following up one line of research. They have, for the most part, been suggested by the discrepancies between the actual results obtained in definite mechanical arrangements, such as occur in some parts of the large field of practical mechanics, and the conclusions arrived at, as to what these results should be for the same circumstances, by means of geometrical and physical analysis as far as this analysis was developed at the time.

When such discrepancies occur, if the experimental results are consistent and approximately accurate, they afford evidence that some circumstance has not previously been taken into account in the general theoretical analysis, and thus indicate the necessity for its further extension. Such discrepancies may also afford a suggestion or clue, and when this occurs the extension of the theoretical analysis necessary to remove the discrepancy is in general not difficult to find, and requires only a short paper for its exposition. But when this has been accomplished, further consideration may show that these extensions of the analysis have a more general application than to the immediate circumstances which led to their recognition, the study of which demands further research and exposition, which require time, and before this is ready some other discrepancy in another part of the field of practical mechanics has appeared and secured precedence.

OSBORNE REYNOLDS.

MANCHESTER,

March, 1900.

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APPENDIX.

Page 58, paper 9. ......depth than a single screw.
(add) For continuation see paper 13, page 78.

Page 245, 6th line, in place of (see Report, 1875, I. p. 145)
insert (see paper 19, p. 134).

1.

ON THE SUSPENSION OF A BALL BY A JET OF WATER.

[*From the Fourth Volume of the Third Series of* "Memoirs of the Literary and Philosophical Society of Manchester." *Session* 1869–70.]

(*Read* March 8, 1870.)

WHEN a ball made of cork, or any very light material, is placed in a concave basin, from the middle of which a jet of water rises to the height of four or five feet, the jet maintains the ball in suspension; that is to say, it takes and keeps it out of the basin. The ball is not kept in one position, it oscillates up and down the jet; nor is its centre kept exactly in a line with the jet, it often remains for a long time on one side of it. In fact, the ball appears to be in equilibrium when it is struck by the jet in a point about 45° below the horizontal circle. In this way, for some seconds at a time, the ball appears as though it were hanging to the jet, and then oscillates in an irregular manner about this position. If its oscillations become so great that it leaves the jet, it instantly drops, but in descending it generally comes back into the jet before it reaches the basin. The friction of the water causes the ball to spin rapidly; and as it moves about the jet, it spins sometimes in one direction, sometimes in another, always about a horizontal axis. Of the water which strikes the ball, part is immediately splashed off in all directions, part is deflected off at the tangent, and part adheres to the ball, and is carried round with it, until it is thrown off by centrifugal force.

The only explanations of this that appear to have been offered are based on one or the other of the following assumptions, viz. that the centre of gravity of the ball remains directly over the jet, or that the jet is accompanied by a current of air which tends to carry the ball into it. With respect to these assumptions, the fact that the ball will come back again into the jet when driven entirely away from it must upset the truth of the first, and at the same time it appears to establish the truth of the second. However, some experiments, which will be subsequently described, made with a view to ascertain if this current exists, show that it does not. Besides which, Mr Routledge and Mr Wild have made some experiments. The former found that when the jet, directed horizontally to avoid the influence of the falling drops, was brought very near to a light ball suspended by a thread, the ball showed no tendency to move towards the jet; and Mr Wild settled the point by showing that the action of the ball is the same in a vacuum as it is in air. It appears, then, that neither of these assumptions is satisfactory.

Now, of the forces which act on the ball, its weight acts at its centre in a vertical line, and is the only force which is not due to the action of the water. When the jet strikes the ball directly underneath, it will produce a force acting upwards in a vertical line, the magnitude of which depends on the height, and may therefore balance the weight of the ball. In this position the ball is, by the action of these two forces, in equilibrium, in the same manner as if it were balanced on a point. The slightest deviation in the jet will upset it; and then the jet will strike it on one side of the vertical line through its centre: when so struck, the forces at the point of contact may be resolved into two, of which one acts along the normal to the surface, or through the centre of the ball, and is due to the impulsive action of the water (this is called P'), and another in the tangent plane at the point of contact (p) (this is due to the friction of the water, and is called R). If W be the weight of the ball, then P', R, and W are the only forces which at first sight appear to exist; and the question is, can the ball be in equilibrium under the action of P', R, and W? This question is easily answered; for these forces are necessarily in the same plane; but they do not all pass through the same point, and therefore they are not in equilibrium. To balance these forces, then, there must be some other force acting on the ball in the same plane with them, and which does not pass through p, or the centre of the ball. Now, besides the water which leaves the ball at p, there is the water which adheres to the ball until thrown off by centrifugal force; and to this we must look for the required force. The effect of a drop adhering to the ball will be very complex, being due to its weight, centrifugal force, and friction. However, if we neglect the weight as being very small, and therefore only able to increase slightly the weight of the ball, and to shift the point at which it acts a little way from the centre, the forces which the drop will produce may be stated accurately. For whenever a drop whose weight is w (lbs.) comes on to the ball with a velocity v (feet per second), and leaves with a velocity u, its whole effect, minus that of its weight while it is on the ball, is equivalent to a force wv/g (lbs.) acting for one second, in the direction in which the drop was moving, and at the point at which it comes on to the ball, and a force wu/g at the point at which it leaves, and in a direction opposite to, that in which it flies off. The first of these forces will form part of P' and R; and therefore, besides the forces at the point P, the effect of any adhering drop will be equivalent to a reaction, such as would be produced if the force necessary to throw the drop from the ball were concentrated at the point at which it leaves. If several drops be leaving the ball at the same time, the several reactions will have a single resultant, which will not pass through p, or the centre, unless they should be distributed equally all round the ball, in which case the reactions would simply produce a couple on the ball, and would not have a single resultant. If the drops are not leaving equally all round, the resultant will act in a direction opposite to that in which the greatest number fly off. If, then, more water is thrown off in one direction than in another, and this direction is the same as that of the resultant of the three forces P', R, and W, this water will produce a force such as it has been shown must exist. First, then, is there any reason why more water should be thrown off in one direction than in another? and, second, in what direction will that be? The water comes on to the ball at p, and that which adheres is at first spread out in the form of a thin film, on which centrifugal force immediately acts to collect it at the equator. As it collects at the equator, the adhesion becomes less, compared with the mass of water, and the drops separate themselves and fly off; in this way the water would begin to leave at p, and go on until it was all thrown off, so that much more water would leave above p than below. But, besides this, the weight of the water will tend to keep it on or to throw it off, and its action to keep it on will be greatest up to the top, after which the conditions for its leaving become more favourable; so that the water may begin to leave at p, or not till it has passed over the top of the ball; but in either case most of the water will be thrown off before it gets below the horizontal circle on the opposite side to p. On examining the ball, it appears that the water which adheres begins to leave at the top. And by far the greater part of the water flies away from the jet.

It is the discovery of this fact which has enabled me to explain the phenomenon; for this water causes a resultant reaction, which is the additional force necessary to maintain the equilibrium of the ball.

Let this resultant reaction be called Q: it will act towards the jet, and its effect will be, first, to force the ball into the jet, and so will help to counteract the obliquity of P; secondly, it will assist in supporting the ball; and, thirdly, since it opposes the rotation, it will balance the tangential force R, caused by the friction at p; and, provided it have the proper magnitude, together with the forces P', R, and W, it is all that is requisite to explain the equilibrium.

It remains to explain the fact, that the ball will fall back again into the jet after it has been driven out of it. This may be done; for the force P which forces the ball out, ceases as soon as contact ceases; but not so with Q, which drives the ball back again towards the jet; for there will still be some water to be thrown off, so that perhaps for half a revolution Q will continue undiminished, and so bring the ball back again into the jet.

Position of Equilibrium.

With respect to the position of the ball when in equilibrium, nothing very definite can be established, as there are no known laws of adhesion; but it may be shown by general reasoning, that there are limits between which the point p must be, so that there may be equilibrium.

Let the point p be at a fixed height, P equal the full force of the jet at this height when acting on the bottom of the ball or on a perpendicular plane, and let P' be the force on the ball. Then, if a be the angle which the normal at p makes with the vertical,

$$P' = P \cos a,$$

and the horizontal component

$$P' \sin a = \frac{P}{2} 2 \sin a \cos a = \frac{P}{2} \sin 2a;$$

therefore

$$P' \sin a = \frac{P}{2}, \text{ and is a maximum when } a = 45°,$$

and

$$P' \sin a = 0 \text{ when } a = 0, \text{ or } a = 90°;$$

so that the tendency of the jet to force the ball to one side increases from nothing to $P/2$ as p moves from the bottom to a point at which the normal makes an angle of 45° with the vertical, and then decreases to nothing as p moves to the middle of the ball.

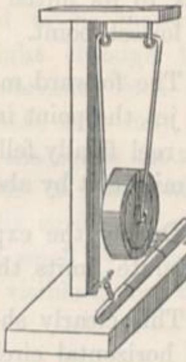
The force Q may be fairly assumed to increase as the speed of rotation increases; and this will be as the point of contact moves from the bottom to the middle of the ball. In the same way the force R, which will necessarily increase as Q increases, will increase as p moves from the bottom to the middle of the ball; and its horizontal component will follow nearly the same law as that of P.

Considering, then, the horizontal forces only, there must be some position for p in which the horizontal component of Q and R will be equal to that of P; and if a horizontal circle be drawn through this point, it will limit the part of the ball in which p must be for equilibrium to be possible.

For any deviation without this circle the equilibrium will be stable; *i.e.* if the centre of the ball gets so far from the jet that the ball is struck in some point without this circle, it will come back again. As to the nature of the equilibrium for any deviation within this circle, I cannot speak positively; but it is probably nearly neutral all over the enclosed area. This seems to agree very well with the fact that the ball is in equilibrium when struck 45° below its horizontal circle, and oscillates about this position.

The following is a description of some experiments. The object was to ascertain:—first, whether or not air is the medium by which the water acts on the ball; secondly, how far the horizontal equilibrium of the ball depends on its rotation; and, thirdly, what is the exact position of the point in which the ball must be struck so as to be in equilibrium, and, moreover, what is the nature of the equilibrium:—

The apparatus employed in these experiments consisted of a wheel, three inches in diameter and half an inch broad at the rim, made of painted wood, capable of turning very freely about its axis, and suspended by two wires, with its axis horizontal, so that it could swing like a pendulum. A vertical jet of water was so arranged that it could be made to strike the reel at any point from below, or to miss it altogether. This was done by bringing the jet out of a horizontal pipe which would slide backwards and forwards in the same direction as the wheel could swing. This pipe was furnished with a cock, so that the force of the jet might be altered.



In experiment No. 1, the pipe from which the jet issued was pushed forward so that the jet missed the reel by about an inch, and the jet was turned on to rise about six feet above the reel; the pipe was then brought back until the water passed as near as possible to the reel without touching it—but there was no apparent effect produced on the reel. The tap was turned so as to increase and then diminish the height to which the jet rose—still, without any effect.

Experiment No. 2 was made with the same apparatus as No. 1. The reel was then changed for one six inches in diameter, and the same experiment repeated.

The jet was placed so that it missed the reel (when hanging freely) by about two inches, and the water was turned on to rise about six feet. The reel was then pushed forward until it touched the jet, and then let go; it immediately began to turn about its axis, but left the jet, swinging backwards and forwards, touching the jet each time, and each time gaining in speed of rotation. This went on for several oscillations; but as it got to turn faster, it appeared to stick to the jet for an instant before letting go; and having done this once or twice, it stuck to the jet altogether, and remained in contact with it, spinning rapidly. The experiment was then repeated with the jet at different distances, and with the larger wheel; the result was the same in all cases. I found it possible, however, either to increase or to diminish the force of the jet enough to prevent the reel from remaining in contact with it. The limits were about 2 and 8 feet.

In experiment No. 3, the position of the reel when free was carefully marked, so that the least alteration could be noticed, and the jet was placed directly under its centre. In this position the jet did not cause the reel to move to either side in particular, but to oscillate backwards and forwards. The jet was then pushed slowly forwards, and the motion of the ball watched. At first it moved away from the jet slightly, and remained away until it was struck about 60° from its lowest point, after which it gradually came back to its initial position, which it reached when struck about 65° from its lowest point.

The forward motion of the jet being continued, the ball began to follow the jet, the point in which it was struck moving upwards very slowly. When the reel finally fell from the jet and came back into its initial position, the jet missed it by about $2\frac{1}{2}$ inches.

During the experiment the force of the jet was altered; but within moderate limits this did not affect the position of equilibrium.

This clearly shows that the position of equilibrium is about 25° from the horizontal circle, and for any deviation below this the equilibrium is much more nearly neutral than for any deviation above it.

2.

THE TAILS OF COMETS, THE SOLAR CORONA, AND THE AURORA, CONSIDERED AS ELECTRICAL PHENOMENA.

[*From the Fifth Volume of the Third Series of* "Memoirs of the Literary and Philosophical Society of Manchester." *Session* 1870–71.]

(*Read* November 29, 1870.)

PART I.

ALTHOUGH the tails of comets are usually assumed to be material appendages, which accompany these bodies in their flight through the heavens (and the appearance they present certainly warrants such an assumption), yet this is not the only way in which these tails may be accounted for. They may be simply an effect produced by the comet on the material through which it is passing, an effect analogous to that which we sometimes see produced by a very small insect on the surface of still water. We see a dark spot, and on looking closer we find a small fly or moth flapping its wings and creating a disturbance which was visible before the insect which produced it.

There is nothing else that we can conceive their tails to be ; so that they must be one or the other of these two things,—either

(1) Material appendages of the nucleus, whether the material be limited to the illuminated tail or surround the comet on all sides—or

(2) Matter which exists independently of the comet, and on which the comet exerts such a physical influence as to render it visible.

Respecting the composition of these bodies Sir John Herschel says:—"There is beyond question some profound secret and mystery of nature concerned in the phenomenon of their tails. Perhaps it is not too much to hope that future observation, borrowing every aid from rational speculation, grounded on the progress of physical science generally (especially those branches of it which relate to the ætherial or imponderable elements), may ere long enable us to penetrate this mystery, and to declare whether it is matter in the ordinary acceptation of the term, that is projected from their heads with such extravagant velocities, and if not impelled, at least directed in its course by reference to the sun as a point of avoidance. In no respect is the question as to the materiality of the tail more forcibly pressed on us for consideration, than in that of the enormous sweep which it makes round the sun in perihelio, in the manner of a straight and rigid rod, in defiance of the law of gravitation, nay, even of the received laws of motion, extending (as we have seen in the comets of 1680 and 1843) from near the sun's surface to the earth's orbit, yet whirled round unbroken: in the latter case through an angle of 180° in little more than two hours. It seems utterly incredible that in such a case it is one and the same material object which is thus brandished. If there could be conceived such a thing as a *negative shadow*, a momentary impression made upon the luminiferous æther behind the comet, this would represent in some degree the conception such a phenomenon irresistibly calls up. But this is not all. Even such an extraordinary excitement of the æther, conceive it as we will, will afford no account of the projection of lateral streamers, of the effusion of light from the nucleus of the comet towards the sun and its subsequent rejection, of the irregular and capricious mode in which that effusion has been seen to take place, none of the clear indications of alternate evaporation and condensation going on in the immense regions of space occupied by the tail and coma—none, in short, of innumerable other facts which link themselves with almost equally irresistible cogency to our ordinary notions of matter and force."

There can be no doubt that, if these tails are matter moving with the comet, this matter must be endowed with properties such as we not only have no experience of, but of which we can form no conception. This would almost seem a sufficient reason for rejecting the first hypothesis. Moreover, on the second hypothesis there is no difficulty in the immense velocity with which these tails are projected from the head, or whirled round, when the comet is in perihelio; for, to take the "negative shadow" as an illustration, here we should have a velocity of projection equal to that of light, and the only effect of the whirling would be a slight lagging in the extremity of the tail, causing curvature similar to that which actually exists; and whatever the action may be, if its velocity of emission or transmission be sufficiently great, this effect will be the same. But whether this hypothesis is to be rejected because it involves assumptions beyond conception, or contrary to experience, must depend on the answers to the following question:—Do we know, or can we conceive, any physical state, into which any substance which can be conceived to occupy the space traversed by comets could possibly be brought, so as to make it present the appearance exhibited by comets?

I think the answer must be in the affirmative, and that we may leave out the terms conceive and conceivable. For electricity is a well-known state, and gases are well-known substances; and when electricity under certain conditions, as in Dr Geissler's tubes, is made to traverse exceedingly rare gas, the appearance produced is similar to that of the comets' tails; the rarer this gas is, the more susceptible is it of such a state; and, so far as we know, there is no limit to the extent of gas that may be so illuminated. Hence we may suppose the exciting cause to be electricity, and the material on which it acts, and which fills space, to have the same properties as those possessed by gas. What is more, we can conceive the sun to be in such a condition, as to produce that influence on this electricity which should cause the tail to occupy the direction it does; for such an electrical discharge will be powerfully repelled by any body charged with similar electricity in its neighbourhood.

The electricity would be discharged by the comets on account of some influence which the sun may have on them, such an influence being well within the limits of our conception.

The appearances of the comet in detail, such as the emission of jets of light towards the sun, and the form of the illuminated envelope, are all such as would necessarily accompany such an electrical discharge.

In fact, if the possibility of such a discharge is admitted, I believe it will explain all the phenomena of comets. As to the possibility, or even the probability, of such a discharge, I think it may be established on very good grounds.

The tails of comets may or may not be one with their heads; but whichever is the case, it is certain that the difference in the appearance of comets and of planets indicates some essential difference, either in the materials of which these bodies are respectively composed, or else in the conditions under which their materials exist. Now, from the motion of comets, we know that their heads follow the same laws of motion and gravitation as all other matter; and therefore we have good evidence, so far as it goes, that comets and planets are similarly constituted as regards materials. And since the appearance of a comet changes very much as it passes round the sun, any assumptions with regard to the material of comets, in order to account for their difference from planets, would not account for the variety of appearance the same comet presents at different times. On the other hand, the conditions of comets and planets must necessarily be very different, from the extreme difference in the shapes of the orbits they describe. Each planet remains nearly at a constant distance from the sun (whatever that distance may be), so that the heat, or any physical effect the sun may have upon it, will also be constant; on the comets its action must change rapidly from time to time, particularly when the comet is in certain parts of its orbit. Hence we may say that the temperature and general physical condition of planets is nearly constant, and that of comets, for the most part, continually varying.

There is, too, a very remarkable connexion between the appearance of the comet and the rate at which the sun's action on it changes. Herschel says :—"Sometimes they first make their appearance as faint and slow-moving objects, with little or no tail, but by degrees accelerate, enlarge, and throw out from them this appendage, which increases in length and brightness till (as always happens in such cases) they approach the sun and are lost in his beams. After a time they again emerge on the other side, receding from the sun with a velocity at first rapid, but gradually decreasing. It is, for the most part, after thus passing the sun that they shine forth in all their splendour, and their tails acquire their greatest length and development, thus indicating plainly the sun's rays as the exciting cause of that extraordinary emanation. As they continue to recede from the sun, their motion diminishes, and their tail dies away, or is absorbed into the head, which itself grows continually feebler, and is at length altogether lost sight of."

Here, although unconsciously, Herschel has connected the increase of brightness with the increase of speed with which comets approach the sun, and the diminution in brightness with the diminution of the velocity with which they leave the sun. And although from Herschel's remark just quoted, it might be inferred that proximity to the sun is the cause of the increase of brightness, this is proved not to be the case ; for (as in the case of Halley's comet) when near its perihelion the tail sometimes dies away, and the comet shrinks. In such cases, when the comet is nearest to the sun there is no development of tail, which shows clearly that it is not the intensity of the sun's rays, but the change in their intensity, that is the exciting cause of these extraordinary appearances; so that there is no reason to suppose that a planet composed of the same material as a comet, no matter how close to the sun, would show a vestige of tail or other cometic appearance.

It is, then, to this change in position that we must attribute those peculiar appearances which belong to comets.

Now is not electricity the very effect which would naturally result from such a state of change and variation in condition ?

A. de la Rive remarks, "Electricity is one of the most frequent forms which the forces of nature assume in their transformations." It certainly often accompanies a change in temperature. There is every indication that it is so in our atmosphere; for the times when its intensity is a maximum, are just after sunrise, and just after sunset, both winter and summer.

For these reasons it seems to me not only possible, but probable, that these strange visitors to our system are clothed in electrical garments, with which the regular inhabitants are unacquainted.

The electricity must, after all, depend on the composition of the comet; for known substances do not all show the same electrical properties. Hence, by assuming comets to be composed of various materials, we have a source to which we can attribute the different appearances presented by the different individuals. To the same source we may attribute the irregularity in the direction of their tails, and the lateral streamers they occasionally send out.

Secondly, I think this electrical hypothesis is supported by the, to me, seeming analogy between comets, the corona, and the aurora—an analogy which suggests that they must all be due to the same cause. They may be all described as streams of light or streamers, having their starting point more or less undefined, and traversing spaces of such extent, and with such velocities, as entirely to preclude the possibility of their being material in any sense of that word with which we are acquainted.

The aurora has long been considered an electrical phenomenon; and recently the same effect has been produced by the discharge of electricity of very great intensity through a very rare gas, there being no limit to the space which it will thus traverse. This being so, why should not the tails of comets, and the corona also, be electrical phenomena? Their appearance and behaviour correspond exactly with those of the aurora; and there is surely nothing very difficult in imagining the sun, which is the source of so much heat, being also the source of some electricity. Neither will there appear any thing wonderful in the electricity of comets, when we consider that of the earth. We must not look on our inability to explain the cause of such an electrical discharge as fatal to its existence; for we cannot any more explain the existence of the electricity which causes the aurora. If we cannot explain whence these electricities come, we can at least show that the conditions, which are most favourable to the development of the aurora, exist in much greater force on the comets than they do on the earth. The greatest development of the aurora borealis takes place at the equinoxes. There is a cessation in summer, and another in winter. Now the equinoxes are the times when the action of the sun on our northern hemisphere is changing most rapidly. Hence the condition favourable for the aurora is change in the action of the sun. The same thing is pointed out by the diurnal variation in the electricity of the atmosphere; for, as has been already shown, the change in temperature on the comets is incomparably greater than it is on the earth, and its variation corresponds with the variation in the atmosphere of the comet.

Ångström has also shown that the light from the aurora, the corona, and the zodiacal light are all of the same character, or all give the same bright lines when viewed through the spectroscope, and that these lines correspond to the light from no known substance. This indicates that, whatever this light may be, the incandescent material is the same in all cases; or may we not assume that it is the medium which fills space, that is illuminated by the electrical discharges? This would be supported by the fact that the light from the heads of two small comets indicated carbon, whereas that from the tails only gave a faint continuous spectrum. For an electrical discharge would first illuminate the atmosphere of the comet, or even carry some of the solid material off in a state of vapour, and then pass off to the surrounding medium; thus, while the spectrum from the head would be that of cometary matter, the tail would be due to the incandescent ether.

I would here suggest that gas, when rendered incandescent by electricity, may reflect light; (it will certainly cast a shadow from the electric light;) and if this be the case, part of the light from comets' tails may after all be reflected sunlight.

At any rate, it is certain that the appearance of streamers, the rapidity of change and emission, the perfect transparency, and the wave-like fluctuations, which belong to these phenomena, are all exhibited by the electric brush; in fact the electric brush will explain all these appearances, which have defied all attempts at explanation on a material hypothesis.

I have only to add that the main assumption involved in the electrical theory is, that space is occupied by matter having similar electrical properties to those of gas; and I would ask, is it not more rational to make such an assumption, than it is to attribute unknown and inconceivable properties to cometary matter?

Theories, even, if founded only on rational speculation, often, I believe, prove very useful, inasmuch as they afford observers a definite purpose in their speculations—something to look for, something to establish or to refute; and I publish these speculations of mine at this particular moment in the hope that they may perchance serve such a purpose.

PART II.

(*Read* February 7, 1871.)

In the paper which I read before this Society on the 29th of November last, I endeavoured to show that it is probable that these phenomena are a species of that action, known as the electric brush, taking place in the medium which fills space, be it ether, or simply gas, or both. The reasoning I made use of was essentially *a fortiori*. I pointed to the fact that the electric brush as seen in the Geissler tubes exhibits similar appearances, and that at the times of greatest display on the part of comets and the aurora similar conditions are present, such as a change in the action of the sun, conditions which, to say nothing more, are favourable to electric disturbance. I purposely avoided all attempts to explain how the brush may be produced, feeling that it was sufficient to point to the aurora, which is universally admitted to be electrical, as a proof that such phenomena do exist, even if we cannot explain how. This proof, however, is perhaps not quite satisfactory. In order that it might be complete, the other phenomena would have to be produced in the same way as the aurora; and this, although possible, is not necessary. An assumption, which is commonly made respecting the phenomena of the aurora, cannot be made with respect to the others. This assumption assigns the two magnetic poles of the earth as the two electrodes, between which the electrical discharge takes place, which forms the aurora borealis and the australis. If this assumption be maintained, some other explanation must be found for the manner in which electricity may form the tails of comets and the corona. It is quite clear that the tail of a comet cannot be due to a discharge between two electrodes situated on the comet itself. In the same way, from the position occupied by the corona, it can hardly be due to electricity passing between two electrodes on the sun. In fact, if a comet's tail is electrical, it is due to a discharge of electricity, of one kind or another, from the comet, which for the time answers to one of the electrodes only. The same may be said of the corona and the sun. If we could observe the aurora from a point distant from the earth, it is very probable that we should find the same to be the case; but whether this would be so or not, an assumption has been made as to the cause and nature of the aurora, which will answer just as well for the corona and comet's tails: it is, that the sun, acting by evaporation or otherwise, causes continual electrical disturbance between the earth and its atmosphere, the solid earth being negatively, and the atmosphere positively charged, and that the aurora is the reunion of these electricities taking place in the atmosphere.

Now, as has been already said, this assumption will serve for the comets and the sun, as well as for the aurora. If there is a continual electrical disturbance between the sun and the medium in which it is placed, so that the sun becomes negatively, and the medium positively charged, the reunion of these electricities would form the corona. It must not be supposed that I assume the sun to be a reservoir of electricity, which it is continually pouring into space. I consider that the supply of electricity in the sun is kept up by some physical action going on between the sun and the medium of space, whereby the sun becomes negatively, and the medium positively charged.

This may be well illustrated by reference to the common electrical machine: here the motion of the glass against the rubber causes the glass to become positively, and the rubber negatively charged; and these electricities do not unite instantly there and then, but remain and accumulate in the respective bodies, until collected and brought together again by the conductor.

Assume, then, that the sun is in the position of the rubber, while the ether is in that of the glass; then the corona corresponds to the spark or brush which leaves the conductor. On the same assumption, the negative electricity of the comet would be more and more set free by the inductive action of the sun, as the comet approached it, and would also be driven off by induction in a direction opposite to that of the sun—and, combining with the positive electricity in the ether, would form the tail of the comet, in a manner analogous to that in which a negative spark is given off by the lid of the electrophorus.

I think that a rational account may in this way be given of the manner of the electrical action to which I have attributed these phenomena; but I do not consider that the probability of the truth of this electrical hypothesis depends on the value of such an explanation. It is an assumption, based on the manner in which it fits into its place, and explains the appearances presented by these beautiful phenomena.

Since this paper was written, my attention has been called to the fact that Mr Richard Proctor has published views of these phenomena which somewhat resemble mine. He attributes them in part to electricity, and in part to meteors. There is, however, this fundamental difference between our views—that he regards the tails of comets as consisting of cometary matter, the difficulty of conceiving which was the origin of these speculations. Moreover I can conceive no electrical discharge between two meteors without a medium between them; and if there is a medium, why is there any necessity for meteors? If, as I see good reason to suppose, gas, when glowing with electricity, reflects or scatters rather than absorbs light of the wave-length which it radiates, that portion of the coronal light which is polarized, and assumed to be reflected, will be accounted for. I think that recent observations have confirmed the probability of these speculations, inasmuch as they have confirmed the facts on which these speculations were based. There is one point which has not been already noticed, but which seems to me to be of some importance.

If the corona be an electrical discharge, the electricity will be continually carrying off some of the elements of the sun into space, where they will be deposited and condensed. May not this stream of matter be the cause of the existence of small meteors, and supply the place of those which continually fall into the larger bodies?

3 A.

ON COMETARY PHENOMENA.

[*From the Fifth Volume of the Third Series of* "Memoirs of the Literary and Philosophical Society of Manchester." *Session* 1871–72.]

(*Read* November 28, 1871.)

THE observation of comets by powerful telescopes has shown them to be in a state of violent internal agitation—a feature which is as much their characteristic as tails or vaporous appearance; for it is not possessed by any of the planets or fixed stars. Robert Hook seems to have been the first to notice this: when he observed the great comet of 1680 (Newton's comet) through his 14-feet telescope, he saw bright streams issuing from some point near the centre of the comet's head, and at first taking a direction opposite to the tail and towards the sun, then gradually diverging, and finally falling back into the tail. These streams were continually changing in magnitude and direction, some of them disappearing, and fresh ones appearing in their places. Their behaviour was such as to lead the philosopher to the conclusion that they were flame and smoke, or vapour excited by the action of the sun on the constituents of the body of the comet. Robert Hook again noticed this phenomenon in the comet of 1682. In 1836 Bessel observed similar appearances in Halley's comet; and, although he appears to have been in ignorance of the fact that they had been noticed before, he was led to the same conclusion as Hook as to their origin, viz. that these streams were jets of vapour caused by the action of the sun's heat on the more solid part of the comet. This hypothesis, started by Hook and afterwards by Bessel, seems to have been very generally confirmed by subsequent observation, almost all comets, large and small, showing signs of the same action. Now although at first sight there seems nothing improbable in the supposition that the sun causes a great amount of evaporation on a comet, yet, before we can admit it as altogether satisfactory, it is necessary to show why the same action should not go on to the same extent on the earth and on the planets; for neither do the planets show this same appearance, nor are we aware of any action on the earth which could give rise to these appearances. I do not know that any attempts have as yet been made to explain this; but I think an explanation may be found in the difference between planets and comets, in their size and the shape of their orbits, in the fact that the planets are, so to speak, large bodies moving in approximately circular paths, and so remaining at about the same distance from the sun, while comets are small and move in eccentric paths, continually altering their distance from the sun. I have (in the subsequent part of this paper) endeavoured to state these reasons, and, further, to show that the difference of evaporation on a comet and on a planet, is a sufficient cause for electrical phenomena on the former, which do not take place on the latter.

I think that the reason why the materials of comets should at times (*i.e.* when the comets are in certain positions) evaporate under the sun's heat in a greater degree than those of planets, will be rendered clear by considering the reasons why the heat of the sun does not continually evaporate the materials of the earth—always remembering:—

1st, That comets move in eccentric, and planets in nearly circular orbits.

2nd, That comets are very much smaller in mass or weight than planets.

Why, then, does not the heat of the sun evaporate the materials of the earth? The heat which the sun is continually pouring into the earth or any of its surrounding bodies is expended in one of the three following ways:—

I. By external radiation from the body.

II. By the evaporation and liquefaction of the materials of the body.

III. By producing changes in the body such as the formation of coal and the growth of living things.

The amount of heat expended in the third way may be considered very small in any such body as the earth; for the amount of energy given out by the combustion of fuel and the work of animals must be nearly equal to that stored in the growing plants.

Therefore the heat which the earth receives from the sun during any period (a day, or a year) is nearly all spent in evaporation and liquefaction, or radiated away into space. Hence the quantity of heat spent in evaporation &c., is the difference between the heat received and that radiated away; and consequently it follows:—

1. If these are equal, there will be no evaporation.

2. If the heat received is greater than that radiated away, there will be evaporation &c.

3. If less, there will be condensation &c.

That is to say, if over any definite period of time, the heat which the earth receives from the sun is equal to that which it radiates into space, then the amount of ice and vapour will be unchanged (unless there be some interchange between these).

If, on the other hand, the heat received is in excess of that radiated away, the vapour in the atmosphere will increase and the ice diminish, and *vice versâ*. Now the relation which the heat radiated away bears to that received will depend on two things, viz. the temperature of the earth's surface, and its distance from the sun. For both the heat received, and that radiated, depend in the same way on (and, in fact, are both proportional to) the extent and nature of the earth's surface; and the quantity of heat received depends on (in addition to this) the distance of the earth from the sun (it varies inversely as the square of this distance); whereas the quantity of heat radiated away depends on the temperature of the earth's surface, as well as on its extent and nature. Hence the *ratio which the heat received bears to that radiated away will decrease as the distance of the earth from the sun increases, and also as the temperature of the earth's surface increases.*

If there were nothing to melt or evaporate on the earth (or any other body whose distance from the sun is nearly constant), then the heat radiated away would eventually equal the heat received; for the temperature at the surface would continually rise until the quantity radiated away equalled that received, and there was equilibrium. This temperature I have in the remainder of this paper called the *temperature of equilibrium.* It will depend simply on the distance of a body from the sun, increasing inversely as the square of the distance. Hence in the case of planets this will be constant, whereas in the case of comets it will vary. If there is any material on the body, which evaporates at a lower temperature than that of equilibrium, then there will be evaporation until the material is all gone, or its conditions of boiling are altered. The temperature at which the softest material will evaporate, will depend on the nature of that material, and on the pressure of the atmosphere surrounding the body. Any increase in the pressure of the atmosphere, will increase the temperature required to evaporate the material. If initially a body has no atmosphere, then we may assume that its materials will evaporate, until the vapour forms one sufficient (if possible) to increase by its pressure the temperature of evaporation to that of equilibrium. But the possibility of this will depend on the size of the body, and the consequent attraction it has for its atmosphere; for it is clear that there must be a limit to the pressure which the atmosphere can exert on the surface of the body; and this limit must depend on the size of the body. Up to a certain point, the thicker the atmosphere the greater would be the pressure on the surface; yet there must be a limit beyond which the extension of the atmosphere would produce no effect, a limit beyond which the external air would be so distant, that the central body would not exert sufficient coercive force to retain it, so that all excess would go off, expanding into space. *Hence the temperature at which evaporation will continue on any body, must depend on the size of the body. The smaller the body, the lower will be this temperature.*

If, then, the body is so small that the atmosphere, when at its greatest, cannot restrain evaporation until the temperature is equal to the temperature of equilibrium, then the body will go on evaporating, and the vapour go on expanding into space until it is all evaporated, or, at any rate, until all the softer materials, those which evaporate at a low temperature, are gone. Thus we see that *no body whose temperature of equilibrium remains fixed—that is to say, no body which moves round the sun in a circle—no planet, in fact, can remain for ever in a condition of permanent evaporation;* for in time, no matter how long, it would lose all those materials on its surface which would evaporate at a temperature below the temperature of equilibrium. This, then, is the reason why there is not permanent evaporation going on on the earth. The temperature of equilibrium may be taken roughly at something like 50°, whereas the temperature at which the most volatile material (water) will boil is 212°. If, however, the earth were to approach the sun until its temperature of equilibrium rose to 300°, then the water would commence evaporating, until either the pressure of the atmosphere of vapour was sufficient to stop further boiling, or else until it was all gone*. Or if, on the other hand, the size of the earth were reduced so that it was no longer able to retain an atmosphere whose pressure on its surface was sufficient to prevent water boiling at 60° F., then the water would go on boiling until it was all consumed. We see, then, that the earth owes its stable condition to being so far away from the sun, and to being of such size that it can retain an atmosphere, sufficient to prevent its softest material from evaporating at a temperature below that of equilibrium, and that in all bodies where this is not the case evaporation will be going on. We may now see why comets should generally be in a state of evaporation, even though they may not contain softer materials than water, and may not approach nearer the sun than the distance of the earth. The fact of their being so much smaller, prevents their retaining the same pressure of atmosphere; and so their materials evaporate at a lower temperature. The eccentricity of their orbits is also essential to the explanation; for if they remained at a constant distance, they must eventually lose all the material which would evaporate at that distance, and so become like the other planets. This will be the case with periodic comets, those which, in spite of the eccentricity of their orbits, return again and again; for each time they

* It is true that water evaporates from its surface at a temperature much below 212°; but an atmosphere of steam would soon prevent this.

come near enough to the sun for the temperature of equilibrium to rise above that of evaporation, they will lose some of their softer materials, until these are all done; and then, so far as evaporation is concerned, the comets will behave as planets.

This may appear as though it were incompatible with the existence of periodic comets. It is not so, however; it is only incompatible with the permanence of periodic comets; and it is an explanation of the facts:—(1) that whilst there are apparently a countless number of comets which do not return, and, according to the laws of gravity, a countless number of these must have been converted by the disturbances of the planets into periodic comets, there are only a very few which are known to be periodic; (2) that the size of the periodic comets has been observed in many instances, if not in all, to decrease; (3) that there are many meteoric stones whose orbits are similar to those of many of the periodic comets, and which do not show cometic appearances,—the assumption being that numberless comets have been disturbed in their paths through space, and, instead of having been sent back in a parabolic orbit, have, owing to a second disturbance by one of our planets (generally Jupiter or Uranus) been attached to our system, and, for a time, have appeared as periodic comets such as those of Halley, but that they gradually lost their softer materials, becoming less and less, until they finally ceased to be comets, and became meteoric stones.

The rate of evaporation on such a body as a comet, would obviously increase as the comet approached the sun, and diminish as it receded; but it would not depend solely on the distance of the bodies from each other; for the materials of the comet would take time to heat, and consequently, as it was approaching, part of the heat would go to warming the body of the comet, and for any position the evaporation would be less than the sun would cause if the comet were stationary. As it left the sun it would be the other way; that is, the evaporation would be more than the position warranted. Thus the greatest rate of evaporation would not be exactly at the time when the comet was nearest the sun, but some time after it had passed its perihelion. Now this lagging (as it may be called) of the sun's action on the comet, is similar to, and consequently offers an explanation of, the lagging which is observed in the display of comets. This will be seen from the following quotation from Herschel:—

"Their variations in apparent size, during the time they continue visible, are no less remarkable than those of their velocity. Sometimes they make their first appearance as faint and slow-moving objects with little or no tail; but by degrees accelerate, enlarge, and throw out from them this appendage, which increases in length and brightness till (as always happens in such cases) they approach the sun and are lost in his beams. After a time they again emerge on the other side, receding from the sun with a velocity at first rapid, but gradually decaying. It is for the most part after thus passing the sun that they shine forth in all their splendour, and that their tails acquire their greatest length and development, thus indicating plainly the action of the sun's rays as the exciting cause of that extraordinary emanation."

The direct heat of the sun would only cause evaporation on that side of the comet to which it was opposite; and consequently the stream of vapour would be emitted towards the sun, just as appears to be the case from observation; the streams of vapour would first form an atmosphere round the comet, which would increase until the extent was such that its attractive force could no longer prevent the outside being driven away by any force there might be, and so forming a tail or train—such force, for instance, as would be exerted if the sun and vapour were both charged with electricity, and acted on each other by induction.

Again, as the vapour proceeded outwards from the comet, it would rapidly expand; and this expansion would cause clouds to form by condensation, which would travel outwards till they were again dispelled by the sun's rays; so that, on the side towards the sun, it is probable there might be one or several shells of cloud at certain distances from the central mass. These, under the action of the sun's rays, would be illuminated, and afford that striking appearance of several bands which is so often seen.

The effect of any repulsive or attractive force in the sun, acting on the vapour of the comet, but not on the central mass, would cause a train or tail; but the direction of this tail would depend on the motion of the comet, as well as on the intensity of the force.

It does not necessarily follow because the tail points away from the sun, that the force which repels it must entirely overcome the effect of the sun's attraction; but it can be shown that, even for those comets whose tails show the most curvature, the force must be comparable with the sun's gravitation; and to produce the straight tails which comets sometimes possess, it would require a force of almost infinite intensity, if it acted as gravity does on ordinary matter. It has been found by Professor Norton that if the bent tail of Donati's comet were composed of ordinary matter, leaving the comet under the action of a repulsive force in the sun, this force must be between 1·5 and ·39 of the sun's attraction, the matter in the leading edge being repelled by the former, and that in the following by the latter.

If the force is electrical, its intensity will depend on the charge of electricity in the vapour; and if there are several streams of vapour variously charged, these would each form a tail of distinct curvature; so that the comet might have several bent tails, or even a fan—which are features not uncommonly observed. This, then, affords an explanation of the great variety of bent tails often seen with comets, and even of those pointing towards the sun; for some of the vapour might have a charge of the opposite electricity to that in the sun, under which circumstance it would be attracted. But, so far as I can see, the straight tails which are so often seen, and sometimes in conjunction with the curved ones, can only be explained in the manner described in my previous paper before this Society (see Paper No. 2, p. 7) as an electric brush or discharge through space.

As a probable cause of the electricity in the vapour of the comet, I would suggest that the evaporation taking place from the surface of the comet, might cause a crust to form there, through which the subsequent vapour would have to force its way in the form of jets, which, like the jets from Armstrong's hydro-electrical machine, might issue charged with electricity, the solid being charged with electricity of the opposite kind. If this were the case, the vapour, as it formed an atmosphere round the nucleus, would discharge some of its electricity back, and so cause those portions of the atmosphere near the solid to be self-luminous; and, besides this, there might be other discharges, between the clouds, or outwards into the medium of space, so as to illuminate a part of or the whole atmosphere.

On this hypothesis, if the vapour of the tail is charged with electricity of one kind, say negative, the solid head must leave the neighbourhood of the sun charged with positive electricity, which, as it gets further from the sun, and evaporation becomes feeble, will at length overpower the negative, and charge the atmosphere of the comet, which would then be attracted by the sun instead of repelled; so that the tail, if it had one, would be towards the sun; or else, if by this time the comet had no atmosphere, it would carry off its charge, and (unless it lost it *en voyage*) on its next appearance it would be charged with positive electricity, and the first vapour would probably be attracted instead of repelled, and the first tail point towards the sun.

Thus a comet, as it left the sun, would arrive at a point where it had no tail, and afterwards would commence a tail towards the sun. This tail would not make much show on the comet's departure; for the evaporation continues to a much greater distance from the sun on the departure, than that at which it commenced; but if the comet returned it would make its first appearance with a tail towards the sun, which, as the evaporation increased, and the comet enlarged, would soon take the opposite direction.

There seem to have been some instances where comets have been first seen with a tail towards the sun, which they have afterwards changed; indeed this is said to be the case with Encke's comet, now visible; so that this is another instance of the remarkable way in which the actual phenomena agree with those which would result from the assumptions in the electrical hypothesis.

3 B.

ON AN ELECTRICAL CORONA RESEMBLING THE SOLAR CORONA.

[*From the Fifth Volume of the Third Series of* "Memoirs of the Literary and Philosophical Society of Manchester." *Session* 1871–72.]

(*Read* February 20, 1872.)

THE object of this paper is to point out a very remarkable resemblance between a certain electrical phenomenon (which may have been produced before, although I am not aware that it has,) and the solar corona. This resemblance seems to me to be of great importance; for the striking features of these two coronæ are not possessed by any other halos, coronæ, or glories, with which bright objects are seen to be surrounded.

Until the eclipse of 1871 there was considerable doubt how far the accounts given by observers of the corona could be relied upon; but Mr Brothers's photograph has left no doubt on the subject. In this photograph we have a lasting picture of what hitherto has only been seen by a few favoured philosophers, and by them only during a few moments of excitement.

This picture shows the beautiful radial structure of the corona, and the dark rifts which intersect it; and it also shows the disk of the moon, clear and free from light. I have not yet seen any of the photographs of the last eclipse; but I hear there are several, and that they show the radial structure and rifts even more distinctly than this one does; but whether they do or not, one photograph is positive evidence; the absence of more simply means nothing.

The features to which I refer as those which distinguish the solar corona are:—

1. Its rifts and general radiating appearance.

2. The crossing and bending of rays.

3. Its self-luminosity, shown by the spectroscopic observations of Professor Young.

4. The way in which its appearance changes and flickers.

When taken in connection with the blackness of the moon's disk (which shows that the corona did not exist in, or owe its existence to, matter between the moon and the plate on which the photograph was taken) these features show that we see on the card the picture of something which actually existed in the neighbourhood of the sun—that the bright rays, which we see photographed, were actually bright rays of light-giving matter, standing out from the sun to an enormous distance. The rifts, and general irregularity of the picture, show that these rays do not come out uniformly all over the sun's surface, but that they are partial and local, in some places thinly distributed, and in others absent altogether. The rays are not all of them straight or perpendicular to the sun's surface.

Such bright rays as these cannot be the result of the sun's light or heat, acting on an atmosphere, or on matter circulating in the form of meteorites. If they are due to the action of the sun's light or heat at all, the matter on which these act must be distributed in the rays we see; for the sun's light and heat coming out uniformly all round, would illuminate any surrounding matter, if at all, so as to show its figure.

The picture irresistibly calls up the idea of a radial emission. If it is the picture of distributed matter, that matter must exist in the form of streams leaving the sun; if it is the picture of some light-producing action, this also must exist in the form of an emission from the sun.

Such, then, are the extraordinary features of the solar corona; and, as I stated, they resemble those of an electrical corona. Anyone who is familiar with the various forms of electrical disruptive discharge, will recognize the general resemblance these coronal features bear to an electric brush. But to the electrical phenomenon I am about to describe, it is no mere general resemblance; it is an actual likeness, with every feature identical.

Before describing this phenomenon, I may be allowed to state how I came to notice it. It will be remembered that, in a former communication to this Society, I ascribed the phenomena of comets and the corona to a certain electrical condition of the sun. Well, the peculiar appearance of Mr Brothers's photograph induced me to try if a brass ball, brought into the condition I had ascribed to the sun, would give off a corona presenting this appearance.

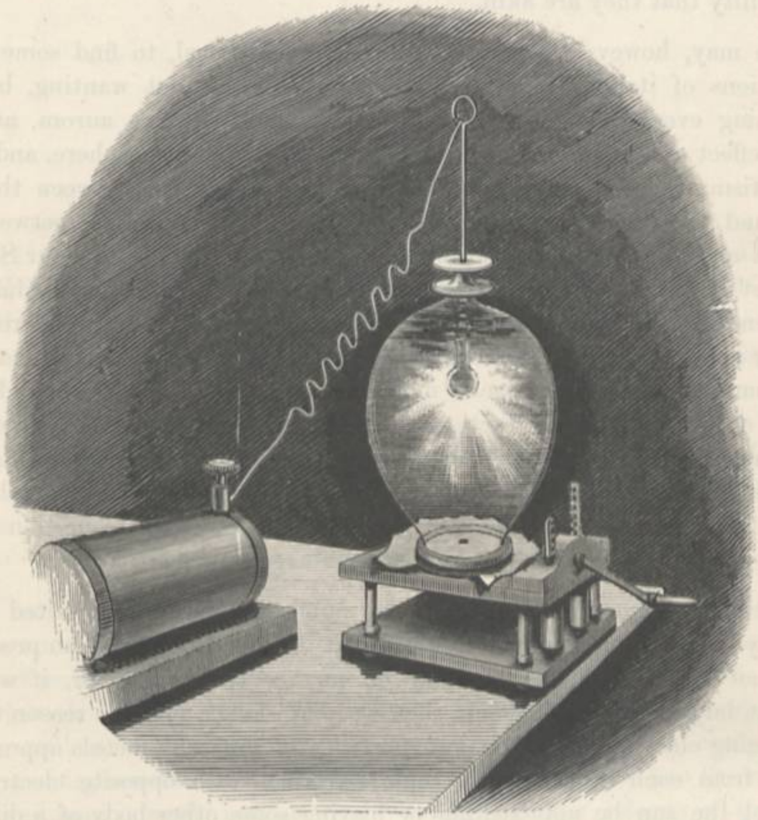
The phenomenon is produced by discharging electricity from a brass ball, in a partially exhausted receiver. To do this there is no second pole used, the objects which surround the outside of the glass probably answering for this purpose. In order to produce the appearance, a certain relation is necessary between the pressure of the air and the intensity of the discharge. It is produced best when the receiver is a glass globe, insulated on a glass stand, the ball being supported in its middle by a rod (coated with india-rubber, to prevent any discharge except from the ball). It is only negative electricity that is discharged into the globe*.

There is great difficulty, even when the apparatus is right, in producing the corona. Using a large coil, I just exhausted the receiver till the pressure was equal to half an inch of mercury; then there was no appearance of a corona, but one more resembling what is seen in a Geissler tube. I then let the air in gradually; and, as the pressure rose, the appearance changed at first to that of a most extraordinary mass of bright serpents, twining and untwining in a knot round the ball, then to that of the naked branches of an oak tree; and as the pressure kept increasing, I gradually observed amongst the branches a faint corona, which I saw at once was what I was looking for. It consisted of pencils of light, forming a faint radiating envelope round the ball, and diminishing in brightness as it receded from it. The tree gradually died out, until there was nothing left but the bright radiating envelope, out of which a brighter ray would occasionally flash. The diameter of this envelope was about three or four times that of the ball. It was not steady, but flickered, so that at times it appeared to rotate on the ball. It consisted of pencils, or, as they are termed, bundles of rays, between which there were dark gaps. These gaps moved round about the ball; subsequently, however, by sticking sealing-wax on the ball, I rendered them definite and permanent. As the pressure of air increased, the brush became fitful, and finally ceased altogether. It was best when there was about four inches of mercury in the gauge. The phenomenon could be produced with different pressures of air, by making a corresponding variation in the action of the coil; and hence I assume that there is a definite relation between the intensity of the charge in the ball, and the pressure of the air surrounding it, under which the phenomena can occur.

The appearance is very faint—so faint that it is difficult to see it even when close to the ball; and I find that it takes some time before the eye can fully appreciate its beauty. It was unfortunately so faint that Mr Brothers was unable to photograph it. The plate was exposed ten minutes; but there was not the slightest trace of anything on it.

* What becomes of this electricity is not clear; when a machine is used it probably distributes itself on the inside of the glass, and induces a corresponding charge on the outside. When the coil is used it must escape back; for I have had it going for hours without any variation.

The annexed woodcut represents the apparatus employed, except that the receiver was replaced by the globe described above. The light round the ball gives a fair idea of the momentary appearance; and it is impossible to represent more, as this flickers and changes so rapidly.



This corona has the same special features as the solar corona:—

1. The rifts and general radial appearance.
2. The bending and crossing of rays.
3. The self-luminosity.
4. The changefulness and flickering.

There is one point in which it differs from the solar corona; but this is no more than must be expected. The shading off of the light in the solar corona is much more rapid than that in its electrical analogue. If, however, the pressure of the air could be made to vary, so that it was denser close to the ball, even this difference could be done away with.

In this experiment, then, we have actually produced the very features which are so extraordinary in the larger phenomenon; and were there no other evidence that the solar corona is electrical, it seems to me that this resemblance constitutes a very strong proof. When two things existing at different times, or in different places, resemble each other perfectly, and resemble nothing else in the range of our knowledge, surely that is high probability that they are akin.

We may, however, expect, if the sun is electrical, to find some direct indications of its electricity. Such indications are not wanting, but are increasing every day. There are the phenomena of the aurora, and the direct effect of the sun on the electricity of the earth's atmosphere, and on its magnetism; there is, moreover, the observed connection between the sun-spots and terrestrial meteorology, as well as the connection between the planets and the sun-spots, shown by Mr De la Rue and Dr Balfour Stewart. It must be admitted that these are evident signs of some mutual influence between the sun and the planetary bodies, which is neither the result of gravity nor of heat. Almost all these signs are of an electrical character; and some are electricity itself; moreover, electricity, or electric induction, is the only other known "action at a distance" besides gravity and heat. Is it not, then, probable that this influence is electrical? Are we to reject an hypothesis which explains some of these phenomena, and may explain all, simply because we do not see any cause for the electrical condition of the sun*—why the sun should be charged with negative electricity?

Should we have discovered that the sun is hot if we had waited to find out why it was so? Surely it is sufficient to say that there is no proof that it is not electrical. But we may go further than this; for, if we may compare large bodies with small, then we may show a possible reason for the sun's being electrified. When two particles of different metals approach or recede from each other, they become electrified with opposite electricities. May not the sun be approaching or leaving some other body of a different material? I do not suggest this as a probable explanation, but simply in answer to those who say that it is absurd to suppose the sun can be electrified.

* This paper was read before Section A of the British Association in 1871, when these objections were raised.

4.

ON THE ELECTRO-DYNAMIC EFFECT WHICH THE INDUCTION OF STATICAL ELECTRICITY CAUSES IN A MOVING BODY.—THIS INDUCTION ON THE PART OF THE SUN A PROBABLE CAUSE OF TERRESTRIAL MAGNETISM.

[*From the Fifth Volume of the Third Series of* "Memoirs of the Literary and Philosophical Society of Manchester." *Session* 1871–72.]

(*Read* February 20, 1872.)

If an electrified body were placed near a moving conductor, so as to induce an opposite charge in the moving body, this charge would move on the surface of the conductor so as to remain opposite the electrified body, whatever the motion might be. If we suppose the moving conductor to be an endless metal band running past a body negatively charged, the positive charge would be on the surface of the band opposite to the negative body, and here it would remain whatever might be the velocity of the band. Now the effect of the motion of this positive electricity on the conductor, would be the same as that of an electric current in the opposite direction to the motion of the band.

If, instead of a band, the moving body consisted of a steel or iron top spinning near the charged body, the effect of the electricity on the top would be the same as that of a current round it in the opposite direction to that in which it was spinning.

It might be that the electricity in the inducing body would produce an opposite magnetic effect on the top; but even if this were so (and I do not think it has been experimentally shown that it would be so), its effect, owing to its distance, would be much less than that of the electricity on the very surface of the top. If we take no account of the effect of the inducing body, the current round the top would be of such strength, that it would carry all the induced electricity in the top once round every revolution. And if the top were spinning from west to east by south, it would be rendered magnetic, with the positive pole uppermost—that is, the pole corresponding to the north pole in the earth, or the south pole of the needle.

In order to show that such a current might be produced, a glass cylinder, twelve inches long and four across, was covered with strips of tinfoil, parallel to the axis, separated by very small intervals. These strips were about six inches long and half an inch wide, the intervals between them being the two-hundredth of an inch. In one place there was a wider interval; and from the strips adjacent to this, wires were connected by means of a commutator with the wires of a very delicate galvanometer. This cylinder was mounted so that it could be turned at the rate of twelve hundred revolutions in a minute, and brought near the conductor of an electrical machine. This apparatus, after it had been thoroughly tested, was found to give very decided results. As much as 20° deflection was obtained in the needle; and the direction of this deflection depended on the direction in which the cylinder was turned, and on the nature of the charge in the conductor. When this charge was negative, the current was in the opposite direction to that of the rotation. It may be objected that the measurement was not actually made on the cylinder. It must, however, be remembered that it was made in the circuit of metal round the cylinder, and that my object was to find the relative motion of the cylinder and the electricity. Altogether I think it may be taken as experimental proof of the fact previously stated, that, if a steel top were spinning under the inductive influence of a body charged with negative electricity, the effect would be that of a current round the top such as would render it magnetic.

The origin of terrestrial magnetism has not been the subject of so much speculation as we might have supposed from its importance. This magnetism seems to have been regarded as part of the original nature of things, just like gravity, or the heat of the sun, as a cause from which other phenomena might result, but not as itself the result of other causes.

Yet, when we come to think of it, it has none of the characteristics of a fundamental principle. It appears intimately connected with other things; and when two sets of phenomena have a relation to each other, there is good reason for believing them to be connected, either as parent and child, or else as brother and sister—the one to be derived from the other, or else both of them to spring from the same cause.

Now the direction of the earth's magnetism bears a marked relation to the earth's figure; and yet it can have had no hand in giving the earth its shape, which is fully explained as the result of other causes; therefore we must assume that the figure of the earth has something to do with its magnetism, or, what is more likely, that the rotation which causes the earth to keep its shape, also causes it to be magnetic.

If this is the case, then there must be some influence at work, with which we are as yet unacquainted—some cause which, coupled with the rotation of the earth, results in magnetism. From the influence which the sun exerts on this magnetism, we are at once led to associate it with this cause. Yet the cause itself cannot be the result of either the sun's heat, light, or attraction. What other influence, then, can the sun exert on the earth?

The analogy between the magnetism produced in a spinning top, by the inductive action of a distant body charged with electricity, and the magnetism in the rotating earth, probably caused by the influence of the sun, which influence is not its mass or heat, seems to me to suggest what the sun's influence is. If the sun were charged with negative electricity, it seems to me to follow, from what the experiments I have described establish, that its inductive effect on the earth would be to render it magnetic, with the poles as they actually are.

The only other way in which the sun can act to produce, or influence terrestrial magnetism, appears to be by its own magnetism. If the sun were a magnet, it would magnetize the earth. If this is the case, the sun's poles must be opposite to those of the earth. Now it follows that such a condition of magnetism would, or at least might, if its materials are magnetic, be caused by the rotation of the sun under the inductive action of electricity in the earth and planets, in exactly the same way as that caused in the earth by the inductive action of the sun. As the direction of rotation is the same in both bodies, and the electricities of the opposite kind, the magnetism would be of the opposite kind also. So that on this hypothesis it is probable that the sun would act by both causes.

When I first worked out this idea, I was not aware that any thing like it had been suggested before; but Mr Baxendell, after having seen my experiments, noticed a review of a book *On Terrestrial Magnetism*, to which he kindly called my attention. The author, without making any assumption with regard to the electrical condition of the sun, assumes it to act on the earth's electricity, precisely as it would under the conditions I have described; and he then proceeds to consider, not only the general features of the earth's magnetism, but all its details (and this in a most elaborate manner)—and to show the explanation which this hypothesis offers for them, particularly for the secular variation of the direction of the needle. I am therefore able to speak of the hypothesis as affording an explanation of the numerous variations of the earth's magnetism, as well as of its general features.

5.

ON THE ELECTRICAL PROPERTIES OF CLOUDS AND THE PHENOMENA OF THUNDER STORMS.

[*From the Twelfth Volume of the* "Proceedings of the Literary and Philosophical Society of Manchester." *Session* 1872–73.]

(*Read* December 10, 1872.)

THE object of this paper is to point out the three following propositions respecting the behaviour of clouds under conditions of electrical induction, and to suggest an explanation of thunder storms based on these propositions and on the assumption that the *sun is in the condition of a body charged with negative electricity*: an assumption which I have already made in order to explain the Solar Corona, Comets' Tails, and Terrestrial Magnetism.

1. A cloud floating in *dry* air forms an insulated electrical conductor.

2. When such a cloud is *first* formed, it will not be charged with electricity, but will be ready to receive a charge from any excited body to which it is near enough.

3. When a cloud charged with electricity is *diminished* by evaporation, the tension of its charge will increase until it finds relief.

I do not imagine that the truth of these propositions will be questioned, but rather, that they will be treated as self-evident. However, as a matter of interest I have made some experiments to prove their truth, in which I have been more or less successful.

Experiment 1 was to show that a cloud in dry air acts the part of an insulated conductor. The steam from a vessel of hot water was allowed to rise past a conductor, the apparatus being in front of a large fire, so that the air was very dry. When the conductor was charged the column of vapour was deflected from the vertical to the conductor, both for a positive and negative charge.

Experiment 2 was made with the same object as Experiment 1. A gold-leaf electrometer was charged so that the leaves stood open, and then a cloud was made to pass by the insulated leaves. As the cloud passed they were both attracted. This experiment was attended with considerable difficulty, as the moisture from the steam seemed to get on to the glass shade over the gold leaves, and so form a charged conductor between the leaves and cloud. The cloud was first formed by a jet of steam from a pipe, then by the vapour from a vessel of boiling water, and lastly by a smoke ring or rather a steam ring. By this latter method an *insulated* cloud was formed, which as it passed was attracted by the charged leaf.

Of the two latter propositions I have not been able to obtain any experimental proof. I made an attempt, but failed, through the bursting of the vessel in which the cloud was to be formed. I hope, however, shortly to be able to renew the attempt, and in the meantime I will take it for granted that these propositions are true. Faraday maintained that evaporation was not attended by electrical separation unless the vapour was driven against some solid, when the friction of the particles of water gave rise to electricity. So that unless there were some free electricity in the steam or vapour before it was condensed, none could be produced by the condensation, and hence the cloud when formed would be uncharged.

In the same way with regard to evaporation, unless, as is very improbable, the steam, into which the water is turned, retains the electricity which was previously in the condensed vapour, the electricity from that part of the cloud which evaporates must be left to increase the tension of the remainder. So that, as a charged cloud is diminished by evaporation, the tension of the charge will increase, although the charge remains the same.

I will now point out what I think to be the bearing which these propositions have on the explanation of thunder storms. In doing this, I am met with a great difficulty, namely, ignorance of what actually goes on in a thunder storm. We seem to have no knowledge of any laws relating to these every-day phenomena; in fact we are where Franklin left us—we know that lightning is electricity, and that is all.

It is not, I think, decided whether the storm is incidental on the electrical disturbance or *vice versâ*, *i.e.*, whether the electricity causes the clouds and storm or is a mere attendant on them. Nor can I ascertain that there is any certain information as to whether, when the discharge is between the earth and the clouds, the clouds are positive and the earth negative, or *vice versâ*. Such information as I can get appears to point out the following law: that in the case of a fresh-formed storm, the cloud is negative and the earth positive; whereas, in other cases, the cloud is positive and the earth negative.

Again, thunder storms move without wind, or independently of wind; but I am not aware whether any law connecting this motion with the time of day, &c., has ever been observed, though it seems natural that, however complicated by wind and other circumstance, some such law must exist. In this state of ignorance of what the phenomena of thunder really are, it is no good attempting to explain them. What I shall do, therefore, is to show how the inductive action of the *Sun* would necessarily cause certain clouds to be thunder clouds in a manner closely resembling, and for all we know identical with, actual thunder storms.

In doing this I assume that the thunder is only an attendant on the storm, and not the cause of it; and that many of the phenomena, such as forked and sheet lightning, are the result of different states of dampness of the air, and different densities in the clouds, and really indicate nothing as to the cause of electricity. In the same way, the periodicity of the storms is referred to the periodical recurrence of certain states of dryness in the atmosphere. Thus the fact that there is no thunder in winter is assumed to be owing to the dampness of the air, which allows the electricity to pass from and to the clouds quietly. What I wish to do, is to explain the cause of a cloud being at certain times in a different state of electric excitation to the earth and other clouds, and of this difference being sometimes on the positive side and sometimes on the negative, that is to say, why a cloud should sometimes appear to us on the earth to be positively charged, sometimes negatively, and at others not to be charged at all.

The assumed condition of the sun and earth may be represented by two conductors S and E acting on one another by induction, the sun being negative and the earth positive. The distance between these bodies is so great that the inductive action would not be confined to those parts which are opposed, but would in a greater or less degree extend all over their surfaces, though it would still be greater on that side of E which is opposite to S than on the other side.

The conductor E must be surrounded by an imperfectly insulating medium to represent damp air. The formation of a cloud may then be represented by the introduction of a conductor C near to the surface of E. Such a conductor, at first having no charge, would attract the positive electricity in E, and appear by reference to E to be negatively charged. If it was near enough to E, a spark would at once pass, which would represent a flash of forked lightning. If it were not near enough for this it would obtain a charge through the imperfect insulation of the medium. Such a charge might pass quietly or by the electric brush. When the cloud had obtained a charge it would not exert any influence on the earth, unless it altered its position. But if the heat of the sun caused part of the cloud to evaporate, the remainder would be surcharged and appear positive. Or if C approached E then C would be overcharged, and a part of its electricity would return, and on its return it might cause positive lightning. Thus, suppose that, after a cloud had obtained its charge, part of it came down suddenly in the form of rain. As the rain came lower, its electrical tension would increase, until it got near enough to the ground to relieve itself with a flash of lightning, almost immediately after which the first rain would reach the ground. It has often been noticed that something like this often takes place; it often begins to pour immediately after a flash of lightning, so much so that it seems that the electricity had been holding the rain up, and it was only after the discharge that it could fall. This, however, cannot be the case, for the rain often follows so quickly after the flash, that there would not have been time for it to fall from the cloud, unless it had started before the discharge took place. If on the other hand C receded from E, it would again be in a position to accept more electricity, or would again become negative. In this way, a cloud in forming, or when first formed, would appear negatively charged; soon after it would become neutral, and then if it moved to or from the earth it would appear positively or negatively charged.

If the air was very dry, as it is in the summer, any exchange of electricity between the earth and the cloud would cause forked lightning, in the winter it would take place quietly, by the conduction of the moist atmosphere.

In this way then there would sometimes be positive, sometimes negative lightning; sometimes the discharge would be a forked flash or spark, sometimes a brush or sheet lightning. And if clouds are formed in several layers, as would be represented by another conductor D outside C, then in addition to the phenomena already mentioned, similar phenomena would take place between C and D; and if in addition to this we were to assume that there are other clouds in the neighbourhood, the phenomena might be complicated to any extent.

And if, further, the motion of the sun is taken into account, as the conductor S moves round E the charges in D and E would vary, accordingly as they were more or less between S and E and directly under the induction of S; *i.e.*, the charge in a cloud would appear to change owing to the motion of the sun; thus a cloud that appeared neutral at midday would, if it did not receive or give off any electricity, become charged positively in the evening.

With regard to the independent motion of the clouds, there are several causes which would affect it. For instance, a cloud whether it appeared on the earth to be negatively or positively charged would always tend to follow the sun, though it is possible this tendency might be very slight. Again, one cloud would attract or repel another, according as they were charged with the opposite or the same electricities; and in the same way a cloud would be attracted or repelled by a hill, according to the nature of their respective charges.

Such, then, would be some of the more apparent phenomena under the assumed conditions. So far as I can see they agree well with the general appearance of what actually takes place, but, as I have previously said, the laws relating to thunder storms are not sufficiently known to warrant me in doing more than suggesting this as a probable explanation.

In these remarks I have said nothing whatever about what is called atmospheric electricity, or the apparent increase of positive tension as we proceed away from the surface of the earth. I do not think that this has much to do with thunder storms. If the law is established it seems to me that it will require some explanation, besides merely that of the solar induction acting through the earth's atmosphere on to the surface of the earth. It would rather imply that the sun acts on some electricity in the higher regions of the earth's atmosphere, and that electricity in these regions acts again on the surface of the earth; but, however this may be, the effect of the assumptions described in this paper would be much the same.

6.

ON THE RELATIVE WORK SPENT IN FRICTION IN GIVING ROTATION TO SHOT FROM GUNS RIFLED WITH AN INCREASING, AND A UNIFORM TWIST.

[*From the Thirteenth Volume of the* "Proceedings of the Literary and Philosophical Society of Manchester." *Session* 1873–4.]

(*Read* October 21, 1873.)

THE object of this paper is to show that the friction between the studs and the grooves, necessary to give rotation to the shot, *consumes more work with an increasing than with a uniform twist;* and that in the case of grooves which develope into parabolas, such as those used in the Woolwich guns, the waste from this cause is double what it would be if the twist was uniform. I am not aware that this fact has ever been noticed. It must not be confounded with the questions already at issue respecting the Woolwich or French system of rifling guns. The advocates of the gradually increasing twist, maintain that it relieves the pressure between the studs and the grooves at the breech of the gun, where it would otherwise be greatest, while the opponents argue that in order to obtain this otherwise advantageous result, the bearing surface of the studs has to be so much reduced, that they are not so well able to withstand the reduced pressure, as they are to withstand the full pressure with the plane grooves. Now I bring forward a collateral point, which has no bearing on the previous question, but which is, in itself, of sufficient importance to influence the decision in favour of one or other of these systems. I show that apart from any undue wedging or shearing of the studs, that with nothing but the legitimate friction, the amount of work wasted in imparting rotation to the shot is nearly twice as great with the parabolic as with the plane grooves. This is important, for, although the magnitude of this waste does not appear as yet to have been the subject of direct inquiry, it will be seen from what follows, that with the plane grooves it amounts to more than one per cent. of the whole energy of the shot, and, consequently, with the parabolic grooves it will amount to two per cent. of the energy of the shot; this is, to say the least, important as regards the effect of the discharge; and when we consider that all the work spent in friction is spent in destroying the gun and the shot, we see that it becomes a matter of the very greatest importance whether the gun spends one, or two per cent. of its power, on self-destruction. It was established as a fact in the trials of 1863–5, that the guns with an increasing twist gave a lower velocity than those with the uniform twist. In the trial with the two seven-inch guns made especially to test this point, the difference of velocity was such as to make three per cent. difference in the energy of discharge—a result somewhat greater than what would have been due to the legitimate friction, unless the coefficient of friction between the studs and the grooves was excessively high from some cause, such as the cutting of the studs into the grooves. However, it would seem that the conclusions at which I have arrived are in accordance with actual experience, and help to explain what was otherwise to a certain extent anomalous.

Although these conclusions cannot be definitely proved without the aid of mathematics, they may be shown to be true (or reasonable) under certain circumstances, as follows:

The work spent in friction will, both with the parabolic and plane grooves, be equal to the coefficient of friction multiplied by the mean pressure on the studs, and again by the length of the grooves (or by the length of the gun—nearly). Now, the coefficient of friction and the length of the gun are the same in both cases; hence this work will be proportional to the mean pressure on the grooves throughout the gun. Again, if the pressure on the parabolic grooves is constant (which it is the object of these grooves to make it), then the mean pressure in both cases will be inversely proportional to the angle which the shot turns through while in the gun. This follows directly from the fact that the speed, and consequently the energy of rotation with which the shot leaves the gun, is the same in both cases; for this energy is nearly equal to the mean pressure multiplied by the arc through which the studs turn*, and hence the mean pressure is equal to the energy divided by the arc.

We have then the work spent in friction proportional to the mean pressure; and the mean pressure inversely proportional to the angle turned through by the shot in the gun; therefore *the work spent in friction is inversely proportional to the angle turned through by the shot in the gun.*

Now, the angle turned through with parabolic grooves is half the angle

* This is always true for plane grooves, but it will only be true for parabolic grooves when the pressure on the studs is constant all along the grooves.

turned through with plane grooves (by a property of the parabola); hence the work spent in friction with the parabolic grooves, is double what it is with the plane grooves. This may be shown mathematically as follows:—

I. *To estimate the actual work spent in friction with plane grooves.*

Let μ = coefficient of friction.

i = the inclination of the grooves.

K = the work spent in friction.

E = the energy of discharge or the striking force with which the shot leaves the gun.

Then,
$$K = \frac{\mu i}{2} \times E.$$

For if R = the mean pressure on the grooves, l = the length of the gun, then

$$K = \mu R l \sqrt{i^2 + 1} \quad \text{..............................(1)}.$$

And the energy of rotation

$$= \frac{i^2}{2} E = \frac{Rli}{\sqrt{i^2+1}}.$$

$$\therefore \frac{i^2}{2} E = \frac{R}{\sqrt{i^2+1}} li \quad \text{..............................(2)},$$

$$\therefore K = \frac{\mu i}{2} E.$$

Hence (with a gun making one turn in 35 diameters) where $i = \frac{1}{11}$ and $\mu = \cdot 3$,

$$K = \frac{\cdot 3E}{22} = \cdot 013E.$$

The equation $K = \frac{\mu i}{2} E$ shows, what is otherwise quite obvious, that with the plane grooves, the work spent in friction is independent of the distribution of the pressure within the gun, and is proportional only to the energy of discharge; and hence will be the same, whether the powder is quick or slow, provided the shot leave with the same velocities.

This, however, is not the case with the parabolic grooves. It is obvious that the friction will involve the law of pressure in the gun. Consequently, we cannot calculate this work unless we make some assumption with regard to the law of pressure.

II. *To estimate the actual work spent in friction with parabolic grooves when the pressure on the studs is constant.*

Let $x = \frac{y^2}{b}$ be the equation to the developed grooves, and let s be the length of the grooves. Then, if we assume that $\frac{dy}{ds} = 1$, and that K_b (the work spent in friction with the parabolic grooves) $= \mu Rl$, we have the work of rotation

$$= \int R \frac{dx}{dy} dy$$

$$= \frac{l}{2b\mu} K_b.$$

And the work of rotation $= \frac{i^2 E}{2}$.

Since $i = \frac{l}{b}$,

$$\therefore K_b = \mu i E,$$

And for plane grooves $K = \frac{\mu i E}{2}$,

$$\therefore \frac{K_b}{K} = 2.$$

An expression for this work might have been obtained without assuming $\frac{dy}{ds} = 1$, but so long as i is less than $\frac{1}{10}$ the difference is very small.

Hence we see that on this assumption the work spent in friction with the parabolic grooves is twice as great as with the plane grooves. This assumption is not an unreasonable one, for the declared object of the increasing twist is that it may equalise the pressure of the studs on the grooves throughout the gun. However, it is not to be supposed that this object is always attained, for one kind of powder has a different law of force from another. It is necessary therefore to consider other laws of force. We cannot obtain a general expression which will include all, but we may examine several laws of force, which will enable us to see how far the law of force affects the results.

In all cases the force diminishes from the breech to the muzzle, and the law may be roughly expressed by $P = \frac{\lambda}{a + y}$ where y is the distance of the shot from the breech, and a and λ are constants for each class of powder.

Although with this value of P the equations of motion cannot be solved rigorously, an approximate solution may be found as follows:—

III. *To find the ratio of work spent in friction with parabolic and plane grooves when the law of force is*

$$P = \frac{\lambda}{a+y}.$$

The equations of motion are

$$\frac{1}{2}\frac{d}{ds}(v^2) = P\frac{dy}{ds} - \mu R \quad\text{.........................(1).}$$

$$R = P\frac{dx}{ds} + \frac{v^2}{\rho} \quad\text{.............................(2).}$$

Neglecting the μR as small in (1), and taking ρ (the radius of curvature) $= b$, we have if $\frac{dy}{ds} = 1$

$$\frac{v^2}{2} = \int P dy = \lambda \log \frac{a+y}{a}.$$

$$\int R dy = \int P \frac{y}{b} dy + \frac{2\lambda}{b} \log \frac{a+y}{a}$$

or $$K_b = \mu \int R dy = \frac{\mu\lambda}{b}\left\{(2l+a)\log\frac{a+l}{a} - l\right\}.$$

And for plane grooves ρ is infinite and $\frac{dx}{dy} = i$,

$$\therefore K = \mu \int R dy = \mu\lambda i \log \frac{l+a}{a},$$

$$\therefore \frac{K_b}{K} = 2 + \frac{a}{l} - \frac{1}{\log\left(1+\frac{l}{a}\right)}.$$

If $a = 0$ so that $\quad P = \frac{\lambda}{y}$,

$$\frac{K_b}{K} = 2.$$

If a is very great, so that $\quad P = \frac{\lambda}{a}$

$$\frac{K_b}{K} = \frac{3}{2}.$$

And the former law more nearly expresses the condition of most guns.

IV. *If we take a law of force,*

$$P = \frac{e^{-2\mu \tan^{-1}\frac{l}{b}}}{b^2 + y^2}$$

the equations of motion can be solved.

And if $$i = \frac{l}{b}, \ \theta = \tan^{-1} i.$$

We get $$K_b = \frac{e^{-2\mu\theta}}{2\mu b} \{e^{2\mu\theta} - 1 - 2\mu\theta + \mu^2 \log(1 + i^2)\}.$$

And for the plane curve

$$K = \frac{i}{2b} e^{-2\mu\theta} \{e^{2\mu\theta} - 1\}.$$

And therefore

$$\frac{K_b}{K} = \frac{e^{2\mu\theta} - 1 - 2\mu\theta + \mu^2 \log(1 + i^2)}{i(e^{2\mu\theta} - 1)}.$$

From which, for any given value of θ, we may obtain the actual value of $\frac{K_b}{K}$.

When θ is small, so that we may neglect high powers without error,

$$\frac{K_b}{K} = \frac{3}{2}.$$

Which result is in exact accordance with those previously obtained, for, it must be noticed, that with this law P is nearly constant. Hence we arrive at the following conclusions:—

(1) That when the pressure of the powder is constant,

$$\frac{\text{Work spent in friction with parabolic grooves}}{\text{Work spent in friction with plane grooves}} = \frac{3}{2}.$$

(2) That when the pressure diminishes rapidly the above ratio $= 2$.

(3) That this ratio may have any values between these two, but that it cannot go beyond these limits.

7.

ON THE BURSTING OF TREES AND OBJECTS STRUCK BY LIGHTNING.

[*From the Thirteenth Volume of the* "Proceedings of the Literary and Philosophical Society of Manchester." *Session* 1873–4.]

(*Read* November 4, 1873.)

THE results of the experiments referred to in this paper were exhibited to the meeting.

The suggestion thrown out by Mr J. Baxendell at our last meeting—that the explosive effect of lightning is due to the conversion of moisture into steam—seemed to me to be so very probable that I was induced to try if I could not produce a similar effect experimentally.

1. I first of all tried to burst a thin slip of wood by discharging a jar through it, taking care so to arrange the wood that the discharge should be of the nature of a spark, and not a continuous discharge; this was done by making the wood to form part of a discharging rod with balls on the ends.

This experiment was successful in the first attempt, although the results were on a small scale.

It should be mentioned that the wood had been damped with water.

This experiment was repeated with larger pieces of wood with various results.

2. It then occurred to me to try with a glass tube. This I did at first with a very small tube, passing wires from the ends of the tube until they were within ½ inch of each other.

The small tubes burst both with and without water.

3. I then used a larger tube (about $\frac{1}{10}$ inch bore) in a similar manner. The discharge without water produced no effect on this, even when repeated several times, but when the tube was full of water (with the ends open) the first discharge shattered that part of the tube opposite the gap in the wire. This tube was bent in the form of a syphon, and the water stood about 1 inch beyond the gap in the wire, on each side of it.

4. I then tried a stronger tube which I had been using for insulation. It had a bore of $\frac{1}{8}$ inch and was $\frac{3}{8}$ inch in external diameter. It was capable of sustaining a pressure of probably 10,000, and certainly 5,000 lbs. on the square inch, that is to say, a pressure from 2 to 5 tons per square inch. It was about 14 inches long and bent in the form of a square-ended siphon. The gap in the wire was about $\frac{1}{2}$ inch, and the water extended about $1\frac{1}{2}$ inches on each side of the gap. The ends of the pipe were open, and the jar charged in the same manner as before, with about 100 turns of a 12 inch plate machine. The surface of the jar is about $\frac{1}{2}$ a square foot, and the discharge, when effected with the common rod, took place through about 2 inches of air.

This tube was shivered at the first discharge. That part opposite the gap, and for some way beyond, is completely broken up into fragments, which present more the appearance of having been crushed by a hammer, than of being the fragments of a pipe burst under pressure. Some of the fragments show that the interior of the pipe has been reduced to powder.

These fragments were scattered to some feet on all sides, but there was nothing like an explosion. I held the pipe in my hand at the time of the discharges, and the sensation was that of a dead blow. There was no noise beyond the ordinary crack of the discharge.

The manner in which this pipe was destroyed clearly showed that a larger one might have been broken. But as it was two o'clock and my fire was out, I did not continue the experiments.

It is not easy to conceive the precise way in which a pressure of probably more than 1,000 atmospheres could be produced and transmitted in a pipe of water the ends of which were open. It might have been caused by the sudden formation of a very minute quantity of steam, or by the expansion of the water; but whichever way it was, its effect was due to its instantaneous character, otherwise there would have been an explosion.

When we consider the great strength of this pipe (which might have been used for a gun without bursting), and when we see that it was not only burst but that the interior of the glass was actually crushed by the pressure, and all this by the discharge of one small jar, we must cease to wonder at the bursting power of a discharge from the clouds.

7 A.

ON THE DESTRUCTION OF SOUND BY FOG AND THE INERTNESS OF A HETEROGENEOUS FLUID.

[*From the Thirteenth Volume of the* "Proceedings of the Literary and Philosophical Society of Manchester." *Session* 1873–4.]

(*Read* December 16, 1873.)

1. THAT sound does not readily penetrate a fog is a matter of common observation. The bells and horns of ships are not heard so far during a fog as when the air is clear. In a London fog the noise of the wheels is much diminished, so that they seem to be at a distance when they are really close by. On one occasion, during the launch of the *Great Eastern*, the fog was reported so dense that the workmen could neither see nor *hear*.

2. It has also been observed that mist in air or steam renders them very dull as regards motion. This is observed particularly in the pipes and passages in a steam-engine. Mr D. K. Clark found in his experiments that it required from 3 to 5 times as much back pressure to expel misty steam from a cylinder as when the steam was dry.

3. My object in this paper is to give, and to investigate, what appears to me to be an explanation of these phenomena; from which it appears that they are intimately connected, that, in fact, they are both due to the same cause. This explanation will be the clearer for a few preliminary remarks.

4. The nature of a fog, and the manner in which the small spherical drops are suspended against their weight, is well understood. So long as the fog is at rest or moving uniformly, the drops being heavier than the air tend to sink like a stone in water, and consequently they are not at rest in the air, but are moving through it with greater or less velocities, according as they are large like rain, or small like haze. This motion is caused entirely

by the difference in the specific gravity of the air and water; if the drops were merely little hard portions of air they would have no tendency to descend.

In some fogs the drops are so fine that they appear to be absolutely at rest, and will remain for a long time without any appreciable motion. The force which retards the downward motion of the drops is the friction of the air, and this is proportional to the surface of the drop, and the square of the velocity*. As the drops get smaller their weight diminishes faster than their surface, and consequently the friction will balance the weight with a less velocity. The exact law is that the velocity caused by the weight of a drop is proportional to the square root of its diameter.

This is the general explanation of what goes on under the action of gravity when the fog is at rest or moving uniformly, and we may make use of it to illustrate what goes on when the fog is subjected to accelerating or retarding forces.

5. If we imagine a vessel, full of such a compound as the fog is made of, to be set in motion or stopped, the accelerating or retarding force will have to be transmitted from the sides of the vessel to the fluid within it by means of pressure. These pressures will act equally throughout the fluid, and, if the fluid were homogeneous, they would produce the same effect throughout it, and it would all move together; but the pressures will obviously produce less effect on the drops of water, than they do on the corresponding volumes of air, and the result will be that the drops of water will move with a different velocity to the air—that the drops of water will in fact move through the air just as they do under the action of gravity. In fact, if the air is subject to an acceleration of 32 feet per second, the effect on the drops (their motion through the air) will be the same as that due to their weight. It is easy to conceive the action between the air and the drops of water. If a mass of air and water is retarded, it is obvious that the water, by virtue of its greater density, will move on through the air. This property has, in fact, been made use of to dry the steam used in steam-engines. The steam is made to take a sharp turn, when the water, moving straight on through it, is deposited on the side of the vessel.

6. Owing to this motion of the water through the air, it would clearly take longer, with the same force, to impress the same momentum on foggy air, than on the same when dry. This is obvious, for at the end of a certain time the particles of water would not be moving as fast as the air, and

* [This is not the case. The resistance is as the velocity, as was pointed out by Prof. G. G. Stokes.]

consequently the air and water would have less momentum than the same weight of dry air all moving together: that is to say, if we had two light vessels containing the same weight of fluid, the one full of dry air and the other full of fog, and both subjected to the same force for the same time, at the end of this time, although they would have exactly the same motion, their contents would not, for the drops of water in the fog would not be moving so fast as the vessel. Now the energy expended on each of these vessels would be the same, but, inasmuch as the effects are different, the energy acquired by the foggy air would be less than that acquired by the dry air, the difference having gone to move the water through the air: that is to say, it would require a greater pressure to impress in the same time the same velocity on foggy air, than on dry air of the same density.

7. This then fully explains the dulness with which foggy air acquires motion. In the passages of a steam-engine the steam is subjected to continual accelerations and retardations, each of which requires more force, in the manner described, with misty than with dry steam, and at each of which the particles of water moving through the steam destroy energy in creating eddies.

8. Although not so obvious, the same is true in the case of sound. The effect of waves of sound traversing a portion of air is, first to accelerate and then to retard it. And if there are any drops of water in the air, these will not take up the motion of the air so readily as the air itself. They will allow the air to move backwards and forwards past them, and so cause friction and diminish the effect of the wave as it proceeds, just as a loose cargo will diminish the rolling of a ship.

9. It is important to notice that this action of the particles of water is not analogous to their action in reflecting the waves of light.

It has been assumed, as an explanation of the action of fog on sound, that the particles of water break up the wave of sound by small reflections, in the same way as they scatter the waves of light. The analogy however is not admissible; for in the case of light the wave length is shorter than the thickness of the drops, and the surface of the drop acts in the same way as if the drop were of large extent; but in the case of sound the wave's length may be thousands of times the thickness of the drop, and instead of the whole wave being reflected, it will only be a very small portion of it. Even this portion can hardly be called a reflection; it is due to the motion of the air past the drops, like the waves of sound caused by a bullet, or the waves thrown off by the bow of a ship.

10. A certain portion of the resistance which the air offers to the motion of the water through it is this—what is called in naval science

wave resistance; but it can be shown that the proportion of this resistance, to the resistance in causing eddies, diminishes with the velocity, and consequently it can have very little to do with the effect of the drops of water on the waves of sound, in which the velocity of the water through the air must be very small*.

11. So far, then, I have shown the manner in which the fog diminishes the sound; it remains to consider the connection between the size of the drops and their effects. I am not aware that any observations have been made with respect to this. I do not know whether it has ever been noticed whether a fine or a coarse mist produces the most effect on sound. It does not appear, however, that rain produces the same effect as fog; and considering rain as a coarse fog, we must come to the conclusion that a certain degree of fineness is necessary.

If we examine theoretically into the relation between the size of the drops and the effect they produce, always assuming the same quantity of water in the air, we find in the first place that if the air is subjected to a uniform acceleration, which acts for a sufficient time for the drops to acquire their maximum velocity through the air, the effect of the drops in a given time—that is to say, the energy dissipated in a given time—is proportional to the square root of the diameters of the drops. This appears from the action of gravity. As previously stated, the maximum downward motion of the drops, and hence the distance they will have fallen in a given time, and the energy destroyed, is proportional to the square root of their diameters. Hence where the acceleration acts continuously for some time, as would be the case in a steam-pipe, the effect will increase with the size of the drops.

This effect may be represented by a parabolic curve, in which distances measured from the vertex along the axis represent the size of the drops, and the corresponding ordinates represent their effect in destroying energy.

If on the other hand the acceleration alternates very rapidly, then there will not be time for the drop to acquire its maximum velocity, and if the time be very short the drop will practically stand still, in which case the effect of the drops will be proportional to the aggregate surface which they expose. And this will increase as the diameter diminishes, always supposing the same quantity of water to be present.

This latter is somewhat the condition when a fog is traversed by waves of sound, so long as the drops are above a certain size; when, however, they are very small, compared with the length of the waves, there will be time for

* This reflection has nothing to do with the reverberation from clouds which occurs in a thunder-storm, which is probably due to the different density of the clouds, and takes place at their surfaces.

them to acquire their maximum velocity. So that starting from drops the size of rain, their effect will increase as their size diminishes, at first in the direct proportion, then more and more slowly, until a certain minuteness is reached, after which, as the drops become still smaller, their effect will begin to diminish, at first slowly, but in an increasing ratio, tending towards that of the square root of the diameter of the drops.

This effect may be represented by a curve which coincides with the previously described parabola at the vertex, but which turns off towards the axis, which it finally approaches as a straight line.

This completes the investigation, so far as I have been able to carry it. The complete mathematical solution of the equations of motion does not appear to be possible, as they are of a form that has not as yet been integrated. However, so far it appears to me to afford a complete explanation of the two phenomena, and further to show, a fact not hitherto noticed, that for any note of waves of sound there is a certain size of drop with which a fog will produce the greatest effect.

8.

ON THE EFFECT OF ACID ON THE INTERIOR OF IRON WIRE.

[*From the Thirteenth Volume of the* "Proceedings of the Literary and Philosophical Society of Manchester." *Session* 1873–4.]

(*Read* February 24, 1874.)

It will be remembered that at a previous meeting of this Society Mr W. H. Johnson exhibited some iron and steel wire, in which he had observed some very singular effects produced by the action of sulphuric acid. In the first place the nature of the wire was changed in a marked manner, for although it was soft charcoal wire, it had become short and brittle; the weight of the wire was increased; and what was the most remarkable effect of all, was that when the wire was broken, and the face of the fracture wetted with the mouth, it frothed up as if the water acted as a powerful acid. These effects, however, all passed off if the wire were allowed to remain exposed to the air for some days, and if it were warmed before the fire they passed off in a few hours.

By Mr Johnson's permission I took possession of one of these pieces of wire and subjected it to a farther examination, and from the result of that examination I was led to what appears to me to be a complete explanation of the phenomena.

I observed that when I broke a short piece from the end of the wire, the two faces of the fracture behaved very differently—that on the long piece frothed when wetted, and continued to do so for some seconds, while that on the short piece would hardly show any signs of froth at all. This seemed to imply that the gas which caused the froth came from a considerable depth below the surface of the wire, and was not generated on the freshly exposed face. This view was confirmed, when, on substituting oil for water, I found the froth just the same.

These observations led me to conclude that the effect was due to hydrogen, and not to acid, as Mr Johnson appeared to think, having entered into combination with the iron during its immersion in the acid, which hydrogen gradually passed off when the iron was exposed.

It was obvious however that this conclusion was capable of being further tested. It was clearly possible to ascertain whether or not the gas was hydrogen; and whether hydrogen penetrated iron when under the action of acid. With a view to do this I made the following experiments.

First, however, I would mention that after 24 hours I examined what remained of the wire, when I found that all appearance of frothing had vanished and the wire had recovered its ductility, so much so that it would now bend backwards and forwards two or three times without breaking, whereas on the previous evening a single bend had sufficed to break it.

I then obtained a piece of wrought iron gas-pipe 6 inches long and $\frac{5}{8}$ inch external diameter, and rather more than $\frac{1}{16}$ of an inch thick; I had this cleaned in a bath both inside and outside; over one end I soldered a piece of copper so as to stop it, and the other I connected with a piece of glass tube by means of indiarubber tube. I then filled both the glass and iron tubes with olive oil, and immersed the iron tube in diluted sulphuric acid which had been mixed for some time and was cold. Under this arrangement any hydrogen which came from the inside of the glass tube must have passed through the iron.

After the iron had been in the acid about 5 minutes small bubbles began to pass up the glass tube. These were caught at the top and were subsequently burnt and proved to be hydrogen. At first, however, they came off but very slowly, and it was several hours before I had collected enough to burn. With a view to increase the speed I changed the acid several times without much effect until I happened to use some acid which had only just been diluted and was warm; then the gas came off twenty or thirty times as fast as it previously had done. I then put a lamp under the bath and measured the rate at which the gas came off, and I found that when the acid was on the point of boiling, as much hydrogen was given off in 5 seconds as had previously come off in 10 minutes, and the rate was maintained in both cases for several hours.

After having been in acid for some time the tube was taken out, and well washed with cold water and soap so as to remove all trace of the acid; it was then plunged into a bath of hot water, upon which gas came off so rapidly from both the outside and inside of the tube as to give the appearance of the action of strong acid. This action lasted for some time, but gradually diminished. It could be stopped at any time by the substitution of cold water in place of the hot, and it was renewed again after several hours by again putting the tube in hot water. The volume of hydrogen which was thus given off by the tube, after it had been taken out of acid, was about equal to the volume of the iron.

At the time I made these experiments I was not aware that there had been any previous experiments on the subject; but I subsequently found, on referring to Watt's *Dictionary of Chemistry*, that Cailletet had in 1868 discovered that hydrogen would pass into an iron vessel immersed in sulphuric acid. See *Compt. rend.* lxvi, 847.

The facts thus established appear to afford a complete explanation of the effects observed by Mr Johnson.

In the first place, with regard to the temporary character of the effect, it appears that hydrogen leaves the iron slowly even at ordinary temperatures —so much so that after two or three days' exposure I found no hydrogen given off when the tube was immersed in hot water. With regard to the effect of warming the wire; at the temperature of boiling, the hydrogen passed off 120 times as fast as at the temperature of 60°. Also when the saturated iron was plunged into warm water the gas passed off as if the iron had been plunged into strong acid; so that we can easily understand how the hydrogen would pass off from the wire quickly when warm, although it would take long to do so at the ordinary temperatures. With regard to the frothing of the wire when broken and wetted; this was not due, as at first sight it appeared to be, simply to the exposure of the interior of the wire, but was due to warmth caused in the wire by the act of breaking. This was proved by the fact that the froth appeared on the sides of the wire in the immediate neighbourhood of the fracture, as well as the end, when these were wetted; and by simply bending the wire it could be made to froth at the point where it was bent.

As to the effect on the nature and strength of the iron I cannot add anything to what Mr Johnson has already observed. The question, however, appears to be one of very considerable importance, both philosophically and in connection with the use of iron in the construction of ships and boilers. If, as is probable, the saturation of iron with hydrogen takes place whenever oxidation goes on in water, then the iron of boilers and ships may at times be changed in character and rendered brittle in the same manner as Mr Johnson's wire, and this, whether it can be prevented or not, is at least an important point to know, and would repay a further investigation of the subject.

9.

THE CAUSES OF THE RACING OF THE ENGINES OF SCREW STEAMERS INVESTIGATED THEORETICALLY AND BY EXPERIMENT.

[*From the* "Transactions of the Institution of Naval Architects," 1873.]

(*Read* April 3rd, 1873.)

THE tendency which the screws of steam ships have, under certain circumstances, to lose their hold on the water, and consequently to let the engines start off at a great speed—a tendency which is one of the greatest sources of difficulty and danger with which steam navigation is attended—appears as yet to have its cause enveloped in mystery, and to require further explanation than has yet been given to it. For although the circumstances under which this racing occurs are such as appear *primâ facie* to afford an explanation of it—although we should naturally expect the pitching and tossing of the vessel, and the exposure of the screw to affect its speed—yet, when we come to look closer, it appears that something more than this is required. The partial exposure of the screw and the diminution of its resistance accounts for some increase of speed; and the backward and forward motion of the water on the tops and in the hollows of the waves also accounts for some, but neither of these is sufficient to account for the way in which the screw loses its hold on the water. And besides, it is not only in storms that racing occurs; whenever the vessel is moving slowly, and the screw working against great resistance—as, for instance, when it is starting the vessel or towing another—there is a liability for racing to occur. In fact, this liability prevents the screw from being used for tugs or boats which are required to stop and start frequently.

Whilst making some experiments on a screw model driven by a spring, I noticed a phenomenon in connection with racing which seems to me to throw new and important light on the subject. It probably would not have attracted my attention had it not been for some remarks of Mr H. Brunel (made last autumn) as to the insufficiency of the mere exposure of the screw to account for the phenomena of racing, which remarks rendered me alive to any other explanation that might present itself to my notice.

The phenomenon consisted in the connection between the racing of the screw and the breaking of the surface of the water, and the consequent admission of air to the blades of the screw. Whenever the screw raced, the water in its wake was broken up and mixed with air; and although at first sight this seemed to be a very natural result of the increased speed of the screw, yet, when I observed that these phenomena invariably happened in conjunction, and that not only did the screw never race without getting air, but that the admission of air was always followed by racing, then I came to think that there was more in it, and that the air must be the cause, and not merely the result of the racing.

With a view to ascertain if this was the case, I carried out the system of experiments I am about to describe; and these, again, led me to the theoretical explanation set forth in the last part of the Paper.

Some of the experiments were made with the model which had first called my attention to the subject. This model is 2 feet 6 inches in length, and has a screw 2 inches in diameter, driven by a spring, the spring being changeable, so that the boat will raise weights at a dead pull varying from half-an-ounce to 4 ounces.

The results of these experiments were as follows:—

1. When the boat was placed at one end of the trough, and allowed to start and run to the other end—the screw being so deeply immersed that it did not froth either in starting or running (this with the strongest spring required to be about half-an-inch under the surface)—then the screw would make just 180 revolutions, whatever might be the power of the spring.

2. If there were a few small waves on the trough, just sufficient to expose the top of the screw, then at such times the screw would apparently slip round without resistance, throwing up froth and water, and turning the boat out of its course. The number of revolutions required to take the boat the same distance as before was greatly increased, being between 250 and 300; and as the screw was not racing for the sixth of its time, during that time it must have turned at many times its ordinary speed. When the screw was not racing, the boat went very fairly straight forwards, but the moment racing commenced it turned to one side.

3. When the boat was so loaded that the screw was but slightly beneath the surface, then the screw raced until the boat got under way, causing froth, and turning the boat to one side; afterwards the racing ceased, or only occurred at intervals, but whenever it did so it was attended with

froth and turning to one side. The number of revolutions was about 250 under these circumstances.

4. When the load was so placed that the screw was just at the surface, then racing commenced, and continued until the spring was down, which happened before the boat got to the end of its course, after having made 350 turns.

5. When the boat was held by a suspended weight, and the screw was immersed deeply enough, it would raise a weight proportional to the strength of the spring without any undue racing, turning steadily all the time; but if, while suspending such a weight, the load on the boat was shifted forwards, the moment the surface was broken, away went the screw, the spring ran down almost with a rush, the water flew out in a jet behind the boat, and the boat was drawn backwards by the weight screwing over to one side as before.

These experiments, conducted with the small spring model, show conclusively that racing is a reality, and that it does not require the exposure of any part of the screw, but that it depends on the depth of the screw below the surface. The results were substantially the same whether the strongest or weakest spring was used, the only difference being, that the strong spring required that the screw should be rather more deeply immersed, in order to prevent racing at starting.

These experiments did not show whether the frothing was the result or the cause of the racing.

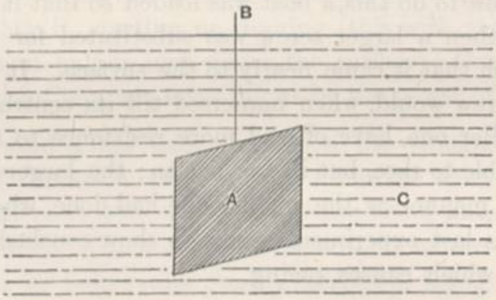
6. With a view to do this, a boat was loaded so that it would start without racing; and then a larger screw was substituted for the previous one, the size being such that it came nearly to the surface. It would be thought that the larger screw would, when immersed till its centre was at the same depth as the smaller one, have offered more resistance to the spring, and so have been less liable to race, but it was not so; the larger screw would race, just in the same manner as the smaller one had done, whenever the surface was broken. This last experiment seems to show conclusively that it is the admission of air which causes racing.

So far, then, the racing is shown experimentally to be the result of the admission of air to the screw, and not simply the diminished area of the part immersed; and it now remains to explain the precise way in which the air diminishes the resistance. This is done from theoretical considerations.

It will appear from the following reasoning that there are two different ways in which the admission of air diminishes the resistance of the screw:— In the first place it interferes with the power of the screw to obtain water; and in the second it reduces the resistance which would otherwise be offered by the water which the screw does get, and causes this water to turn round with the screw.

The driving power of any propeller will depend on the quantity of water on which it acts in a given time, and on the backward velocity which it imparts to this water. Now the quantity of water on which a propeller acts will depend on the power of the propeller to draw water, not so much when the boat is moving fast, for then it will necessarily have fresh water to act upon; but when from any cause the boat is going slowly, when it is starting, towing, or meeting a head wind, then the screw has to depend mainly on its power of drawing a supply of water. If the quantity of water is small, the speed imparted to it must be great; and anything which reduces the power of a propeller to draw water reduces its resistance, and consequently allows its speed to increase. Hence if the mere fact of the screw breaking the surface of the water so as to let air down behind its blades diminishes its power to draw water, it will diminish its resistance, and cause it to race. Now, although it seems to have been overlooked so far, there can be no doubt that the admission of air does act in this way. For the power of a propeller to supply itself with water manifestly depends on the rapidity with which fresh water will flow into the place of that which is driven astern.

Let us suppose A to be a vertical plate below the surface of the water, and capable of being driven forward with any velocity; then, if it were to start from rest, its velocity might be such that the water immediately behind it would or would not start off as fast; that is, its initial velocity



might be such that the water would not keep up, and a space would be left between the plate and the following water. So long as its velocity was not sufficient for this, the quicker the velocity of the plate, the greater would be the velocity of the following water; but after this limit had been once exceeded, the initial speed with which the water would follow would not depend on the speed of the plate, but would be the same for all speeds. That is to say, that urge the plate forwards as fast as we might, we could not get the water to follow at above a certain velocity. This velocity may be stated in terms of the pressure of the water against the plate before it begins to move, for it would obviously be that with which water would flow into the end of an empty pipe, or rather through an opening in the position of the plate; therefore the greatest quantity of water which a propeller could draw, would be equal to that which would flow into a vertical opening in the same position as the area through which the propeller acts. Now the velocity, with which water would flow through such an opening, would be proportional, and generally equal to the velocity which a body would acquire in falling freely through a vertical distance equal to the depth of water that would be necessary to produce such a pressure as that on the plate.

Hence, the limit of the power of a propeller to supply itself with water, will depend on the pressure of the water over the vertical area through which the propeller acts. The pressure of the atmosphere will or will not be included in this, according to circumstances. For if A is entirely below the surface, and moves too fast for the following water, it will leave a vacuum behind it, and the water will follow as fast as it could flow through an opening into a vacuum; in this case the pressure on A, must not only include the actual pressure of the water, but also the pressure of the atmosphere. If, however, A extends to the surface, or communicates with the surface in any way, then the space will be filled with air, and the water will only follow as fast as it would flow through an opening on which the pressure of air acts; in this case the effective pressure will only be the actual pressure of the water. If, therefore, air can get behind the plate, the greatest velocity at which the fluid will follow will be equal to that which would be acquired in falling through AB. But since the pressure of the atmosphere is equal to that of 30 feet of water, if the air cannot get in, then the greatest velocity will be due to $30 + AB$. Hence the power of a propeller to draw water will depend on the depth at which its plates act below water; and also on whether or not the air is let in, the exclusion of air being as good as 30 feet additional immersion.

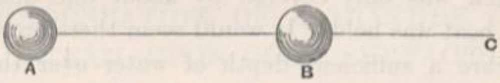
Suppose, then, that a stationary screw-propeller, totally immersed, were driven so fast that it was getting its maximum quantity of water, and that driving it faster would only cause a vacuum behind its floats. Then the quantity of water would be equal to that which would flow through an opening of the same size as the area through which the propeller acts, and 30 feet below the actual position of the screw. If, however, the air were let in, then the actual quantity would only be equal to what would flow through such an opening in the actual position of the screw. Thus, if its lowest point were 12 feet below the surface, the mean suction power over the whole area would be equivalent to a head of 36 feet of water, 30 for the atmosphere, and 6 for the mean pressure of the water. If then (by means of a wave), air were let in behind the floats, the suction power would be reduced until it was only equivalent to a head of 6 feet. And since the velocity of the water would be proportional to the square root of the head, the quantity of water which the screw would draw would be reduced from 6 to $2\frac{1}{2}$, or by more than half. And if the same driving force were maintained, the slip would have to be more than doubled to make up for the diminished quantity of water.

The diminution of power caused by admitting the air,—or more correctly, the increase gained by excluding it—will really be constant at all depths, but when compared with the power arising from the pressure of the water, it will be much greater for small depths; that is, the ratio of these quantities will diminish with the depth. This explains the fact that the screws of small boats are more liable to race than those of larger ones, and it was doubtless the exaggerated form of racing which existed in the small model that caught my attention, and led me to connect it with this cause. This also explains the fact that the racing causes the boat to turn out of its course. For as long as the air is excluded, the top of the screw will be as well able to obtain water as the bottom; but as soon as the pressure of the atmosphere is taken off, the power of getting water will increase with the depth, and consequently the bottom of the screw will get more than the top, and the resistance at the bottom will consequently be greater than it is at the top, and the boat will be pushed to one side.

The direct effect of the admission of air behind the propeller blades, to diminish the quantity of water, will be the same for all classes of propellers, and therefore the same for the paddle as the screw; but as, in the former, the air is always admitted, there will be no more racing from this cause in a storm than in a calm. If, however, this effect were the only one which the admission of air produced, a screw, even when breaking the surface, would be a better propeller than paddles for starting or towing; that is, because of its greater depth of immersion, it would have less tendency to race. But there is another way in which the existence of air in the water on which the propeller acts will diminish its power, both to drive the water away and get more; and although this effect will not be of much consequence to a direct acting propeller, like a paddle, yet it is aggravated to almost any extent in a revolving and oblique acting propeller like the screw.

Air increases the tendency of the water to whirl round with the screw, by diminishing the power of the screw to clear itself of the water it has set in motion. It has often been noticed, when a vessel is starting, that the screw seems rather to whirl the water round, than to drive it astern. It can be shown that the admission of air will be conducive to this end to a very great extent. And the mere fact that the whirling of the water has been observed, proves that at such times there was air in it. If the blade of a screw is driving not simply water but air and water, as it passes any particular portion of the mixture, the pressure will not simply drive it in front of the blade, as it would if it were a solid mass of water, but will compress the air bubbles, which as soon as the blade has passed will expand again, driving some of the water backwards and some of it forwards.

This action of the air in water is analogous to that of an elastic string, connecting two heavy balls A and B. It will only require about half the



force to impress a certain motion on B in the direction BC, that it would have required if the string had been inelastic, for then both balls would have had the same velocity; and as it is, when the force is removed, the elasticity will partially stop B and accelerate A. If when connected in this way, a force is impressed on B, equal to what would be necessary to give the two balls a certain velocity, then B will start with a greater velocity, which the force must follow up. So it is with the air and water. If the propeller gets as great a pressure, when acting on air and water, as when acting on unbroken water, it must move very much faster through it. This will be the result both of the compression of the air in front and the extension of that behind the blade. Thus the air, by virtue of its elasticity, will require an increased velocity in blades of an oblique propeller; and this increased velocity will, by friction, increase the tendency of the water to whirl with the blade. And each blade will leave a following mass of air and water behind for the next blade to act upon.

This effect of the air will not exist in the case of direct action, such as that of the paddles, for the water remains in front of the blade for a longer period, and the one blade does not come upon the leavings of the other. Thus we see the admission of air behind the blades of a propeller will reduce the power of the propeller to supply itself with water; and in the case of the screw will aggravate this evil by necessitating (on account of the elasticity it gives to the water) a higher velocity in the blade to impart the same velocity to the water; which is again aggravated by the tendency which the increased velocity has to whirl the water round. So that the tendency of the screw to race may be said to be due entirely to the admission of air below the surface.

It would seem that, if this is the explanation of racing, all that is necessary, in a calm sea, to render a screw much superior to paddles in stopping, starting, or towing, is to give it sufficient immersion.

This has been tried and proved to be the case so far as an experiment on a model is a proof. No matter what might be the power employed, so long as the surface was unbroken, there was a corresponding towing power; and so long as this condition was maintained, the screw started and stopped the boat quite as well as it was possible for paddles to do.

A larger steam model capable of towing with a force of 1 lb. was tried with two different screws—the one 3 inches in diameter and the other $4\frac{1}{2}$. The small one was covered by about an inch of water, and it was found that this screw would not race even when the boat was held still. The larger one, however, which was only covered by about one quarter of an inch, did race when the boat was held. It would seem therefore that it is of more importance to secure a sufficient depth of water over the screw than to increase the diameter, and it seems probable that some of the advantage of twin screws is due to the fact that they are generally covered to a greater depth than a single screw.

10.

ON THE CONDENSATION OF A MIXTURE OF AIR AND STEAM UPON COLD SURFACES.

[*From the* "Proceedings of the Royal Society," No. 144, 1873.]

(*Read* May 1st, 1873.)

1. THE object of this investigation is to ascertain how far the presence of a small quantity of air affects the power of a cold surface to condense steam. *A priori* it seemed probable that it might retard condensation very much; for when pure steam comes up to a cold surface and is condensed, it leaves an empty space which is immediately filled with fresh steam; so that the passage of the steam up to the cold surface is unobstructed, and if the surface could carry off the heat fast enough, then the rate of condensation would be unlimited. If, however, the steam is mixed with air, then, as the mixture comes into contact with the cold surface, the steam will be condensed and the air will be left between the fresh steam and the cold surface; so that, after condensation has commenced, that surface will be protected by a stratum of air, and fresh steam will have either to displace this, or pass through it, before it in turn can be condensed.

2. This question, besides its philosophical interest, has important practical bearings on the steam-engine.

First. If the quantity of air mixed with the steam affects the rate at which it condenses, then the ratio which the pressure of air bears to the pressure of steam in a condenser will materially affect its efficiency: this is particularly important with reference to the surface-condenser.

Second. If air prevents the condensation of steam, then by sending air into the boiler of a high-pressure engine, the condensation at the surface of the cylinder will be prevented, which, if allowed to occur, becomes a source of great waste; for when the steam comes into a cold cylinder it condenses, heating the cylinder and leaving water, which will again be evaporated as soon as the steam escapes; and this, in evaporating, will cool the cylinder. By preventing this, the mixing of air with the steam would effect the same object as the steam-jacket, only in a more efficient manner; for the heat communicated to the steam in the cylinder from the jacket is not nearly so effective as that which is communicated from the boiler, in consequence of the steam in the cylinder being at a lower temperature than that in the boiler.

3. The experiments for this investigation were, by the kind permission of Dr Roscoe, carried out by Mr Pasley, a student in the Chemical Laboratory of the Owens College; and I beg to tender him my best thanks.

4. In making these experiments two objects were particularly kept in view:—

First. To ascertain if there is a great difference in the rate of condensation of pure steam and a mixture of steam and air—to ascertain in fact, whether pure steam condenses at an unlimited speed.

Second. To ascertain if (and according to what law) the effect of air on the condensation increases as the proportion of air to steam increases.

5. Of these two undertakings the first is much the most difficult. The rate of condensation of pure steam is so great that it is practically impossible to measure it; and to institute a comparison between this and the condensation of a mixture of steam and air is like comparing the infinite with the finite. It is practically impossible to keep any surface cold when an unlimited supply of pure steam is condensed upon it, so that under such circumstances, the quantity of pure steam condensed is limited by the power of the surface to carry off the heat. The best method of obtaining a qualitative result seems to be by introducing sufficient cold water into a flask of steam to condense it all, and ascertain whether this condensation is effected suddenly or slowly.

6. The presence of hot water in the flask with the steam very much assists in ascertaining the rapidity of condensation. When there is no hot water in the flask, the condensation by the injected water is only a question of time; the gauge will come to the same point whether the condensation is quick or slow, the only difference being in the speed at which it will rise—a difference not easy to appreciate, especially when the motion is quick. But if hot water is present, then as the steam in the flask is condensed, it is replaced by fresh steam from the water, and the interval between the condensation and the consequent ebullition is the only time allowed for the creation of a vacuum; the vacuum which is attained in the interval will therefore depend on the rapidity of condensation. The interval will be very short; and the better the vacuum the shorter it will be; so that unless

the condensation is very sudden, there will be but a slight reduction of pressure.

If, however, the condensation is really instantaneous, a perfect vacuum may exist for an instant. Hence, when there is water in the flask, the rapidity of condensation is indicated by the height to which the gauge rises, instead of the speed with which it rises; and this is much easier to estimate.

7. The apparatus employed in making these experiments consisted of a glass flask, fitted with a mercurial vacuum-gauge, and pipes for admitting water and air, or allowing steam to escape.

The flask and all the pipes were freed from air by boiling; and when all the air had been driven out the pipes were closed, the lamp removed, and the flask allowed to cool until the gauge showed a slight vacuum; the water-pipe was then opened and a few drops of water allowed to enter and fall through the flask; as they did so the mercury rushed up the gauge, and, by its momentum, above the point for a perfect vacuum, showing that the condensation was instantaneous. Immediately afterwards the gauge fell nearly to its starting-point. Next, the flask was allowed to cool and a little air was let in (about equal to half an inch of mercury in the gauge, or about a sixtieth of the volume of the flask). The lamp was then replaced, and the operation was repeated as before: this time, however, as the cold water entered, the mercury did not rush up the gauge, but rose slowly a small distance and there remained.

8. This experiment shows, therefore, that there is a great difference in the rates at which pure steam, and steam with air, condense on a cold surface, so great in fact that the speed with pure steam must be regarded as nearly infinite.

9. To compare the various effects of different quantities of air, two methods have been used, which may be described as follows:—

I. A surface-condenser is formed within the boiler or flask, so that the steam may be condensed as fast as it is generated. Then, when a flame of a certain size acts on the boiler, the effect of the air is to cause the pressure of steam in the flask to increase. This method is founded on the assumption that the rate at which steam will condense at a cold surface is, *cæteris paribus*, proportional to its pressure—an assumption which is probably not far from the truth.

II. With the same apparatus as in method I. the rate of condensation is measured by the quantity of water condensed in a given time, obtained by counting the drops from the condenser, the pressure within the flask being kept constant. This method does not involve any assumption; but the conditions for its being accurate are such as cannot be obtained; for not only must the temperature of the condenser and the temperature of the steam remain constant, but the pressure of the *steam* must also remain constant, and if the two former conditions are fulfilled the latter cannot be; for the temperature of the steam will be the boiling-point of the water in the flask; and if this is to remain constant, the pressure of air and steam must be constant, and therefore, as the pressure of the air increases, the pressure of the steam must decrease. This variation of pressure is not very great; and its effect may be allowed for on the assumption that the condensation is proportional to the pressure of steam. This is accomplished by dividing the drops by the pressure of the steam.

These methods, neither of which, as it appears, is rigorous, seem nevertheless to be the best; and fortunately the law which the effect of the additions of air follows, is of such a decided character as to be easily distinguished; and the two methods give results which are sufficiently concordant for practical purposes.

10. The apparatus employed in these experiments consisted of a glass flask, in which a surface condenser was formed of a copper pipe passing in and out through the cork. This pipe was kept cool by a stream of water, and was so fixed that all the condensed water dropped from it, and the drops could be counted. The flask was freed from air by boiling; the volume of air passed into the flask could be accurately measured; and ample time was allowed for the air in the flask to produce its effect before more was admitted.

For the experiments according to method I., the flame under the flask, and the stream of water through the condenser, were kept constant from first to last. For those made according to method II., in one case the stream of water was kept constant, and in the other it was altered, so that the effluent water was kept at a constant temperature.

11. The results of these experiments are shown in Tables I., II., III.

The letters which head the columns have the following meanings:—

f stands for the volume of the flask in cubic centimetres.

a stands for the volume of the air at the pressure of the atmosphere.

h_0 stands for the height of the barometer in millimetres of mercury at the time of the experiment.

h_1 stands for the height of mercury in the gauge in millims.

t_0 stands for the temperature Centigrade of the effluent water.

t_1 stands for the temperature of the water in the flask, found from Regnault's tables of boiling-points.

$p_1 = h_0 - h_1$ stands for the pressure within the flask in millims. of mercury.

$p_2 = \dfrac{a}{f} + h_0 \dfrac{t_1 + 274}{t_0 + 274}$ stands for the pressure of the air within the flask corrected to the temperature T_1.

$p_3 = p_1 - p_2$ stands for the pressure of the steam.

$\dfrac{p_2}{p_3}$ stands for the ratio of the pressure of the air in the flask to that of the steam.

Table I.

$h_0 = 756, \quad t_0 = 9, \quad f = 500.$

a	t_1	h_1	Drops per minute	p_1	p_2	p_3	$\frac{1}{p_3}$	$\frac{p_2}{p_3}$	$\frac{2000}{p_3}$
0	9	754	...	2	0	2	......	·0	...
0	22	736	...	20	0	20	·0500	·0	100
1·5	36	712	...	44	2·4	41	·0240	·06	48
5·0	52	654	...	106	8·7	97	·0110	·09	22
10	66	557	56	199	18·4	183	·0055	·10	11
13	70	521	...	235	23·7	211	·0050	·11	10
21	77	433	...	323	40·0	283	·0035	·14	7
30	84	330	...	426	57	368	·0027	·15	5·4
40	88	264	...	492	77	414	·0024	·18	4·8
50	93	179	56	577	97	479	·0020	·20	4
60	96	115	...	641	117	523	·0019	·22	3·8
70	98	55	...	701	138	562	·0017	·24	3·4
80	100	0	...	756	159	596	·0016	·26	3·2

Table II.

$h_0 = 457, \quad f_0 = 500.$

a	t_0	t_1	h_1	Drops per minute	p_1	p_2	p_3	$\frac{p_2}{p_3}$	$\frac{\text{Drops}}{p_3}$	$\frac{\text{Drops}}{p_3}$
0	27	66	567	100	190	0	190	......	53	42·4
2·5	24	,,	572	84	185	4·5	180	·022	45	36
5	20	,,	582	59	175	9	166	·055	35	27
10	13	,,	582	21	175	19	156	·12	14	11·2
27	10	,,	582	10	175	48	127	·39	76	6·0
37	10	,,	579	10	177	66	111	·66	9	7·2
50	9	,,	572	8	184	90	94	·10	8	6·4

TABLE III.

$h_0 = 748, \quad t_0 = 11, \quad f = 500.$

a	t_1	h_1	Drops per minute	p_1	p_2	p_3	$\frac{p_2}{p_3}$	$\frac{\text{Drops}}{p_3}$
0	6	741	...	7	0	7	·0	0
...	47	663	106	85	0	85	·0	125
3·2	66	557	106	191	6	185	·032	60
5	”	”	56	”	9	182	·050	30
10	”	”	21	”	18	173	·104	11
15	”	”	17	”	27	164	·163	10
20	”	”	12	”	36	155	·23	8
30	”	552	10	196	54	142	·39	7
40	”	557	8	191	72	119	·60	$6\frac{1}{2}$
50	”	562	7	186	90	96	·93	7

12. Table I. shows the result of an experiment after the first method, during which the flame and condensation remained constant, whilst the pressure within the flask increased with the quantity of air.

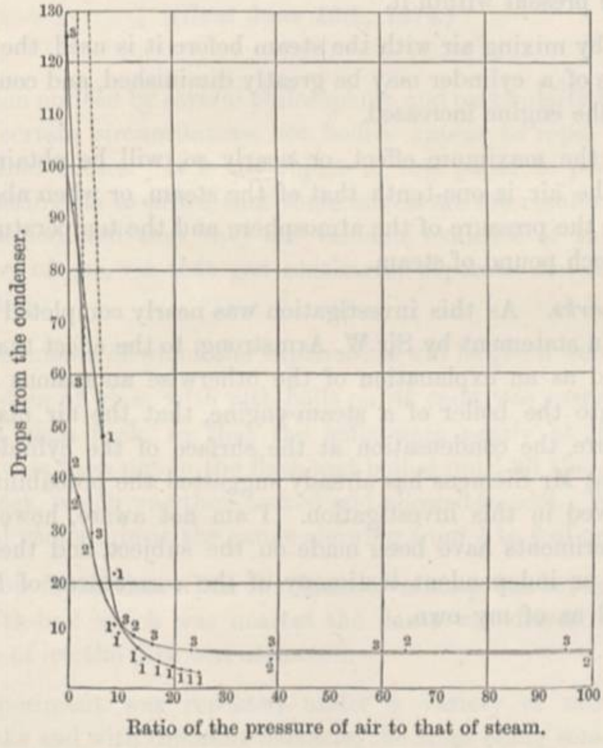
Table II. shows the result of an experiment after the second method, in which the pressure within the flask remained constant, whilst the flame and condensation were reduced as the air was admitted. In this experiment the rate at which the water passed through the condenser was constant from first to last, and consequently the temperature of the effluent water varied with the condensation.

Table III. shows the result of an experiment, also made according to the second method, but in which the quantity of water flowing through the condenser was so varied that the temperature of the effluent water remained constant.

13. Each of these Tables shows the effect of air on the condensation in a very definite manner; but the results as given in the column p_3 in Table I. cannot be compared with the $\frac{\text{Drops}}{p_3}$ in Tables II. and III. as they stand; for these show the effect of the air in a series of increasing figures. If, however, these figures show the power of the air to diminish condensation, then they will be inversely proportional to the quantity of water condensed, *i.e.* what would have been condensed if the pressure and other things had remained constant. Hence the numbers in the column $\frac{1}{p_3}$ should be proportional to the numbers in the column $\frac{\text{Drops}}{p_3}$ in Tables II. and III.

In order to compare the results of these experiments, the results in each Table have been multiplied by a common factor, so that they may be the same when the pressure of air is one-tenth that of the steam. Thus the numbers in the column $\frac{1}{p_3}$ in Table I. have been multiplied by 2000, and numbers under $\frac{\text{Drops}}{p_3}$ in Table II. by 7. The results of the experiments thus reduced are shown in the curves 1, 2, 3.

The point of no air might have been chosen as the point in which the curves should coincide; but, as has been previously explained, the results under such circumstances are to be taken as indicating the power of the condenser to carry off the heat. Had it been possible to keep the condenser cool, there is reason to believe that there would have been no limit to the condensation of pure steam, and that the true form of the curves is like that shown by the dots.



Ratio of the pressure of air to that of steam.

Although the curves do not coincide, yet they are all of the same form, and the difference between them is not greater than can be accounted for by the disturbing causes already mentioned. They all show that the effect of air begins to fall off rapidly when its pressure amounts to one-tenth that of the steam, and that when it amounts to about one-fourth that of the steam the admission of more air produces scarcely any effect.

14. *Conclusions.* The conclusions to be drawn from these experiments are as follows :—

1. That a small quantity of air in steam does very much retard its condensation upon a cold surface; that, in fact, there is no limit to the rate at which pure steam will condense but the power of the surface to carry off the heat.

2. That the rate of condensation diminishes rapidly and nearly uniformly as the pressure of air increases from two to ten per cent. that of the steam, and then less and less rapidly until thirty per cent. is reached, after which the rate of condensation remains nearly constant.

3. That in consequence of this effect of air the necessary size of a surface-condenser for a steam-engine increases very rapidly with the quantity of air allowed to be present within it.

4. That by mixing air with the steam before it is used, the condensation at the surface of a cylinder may be greatly diminished, and consequently the efficiency of the engine increased.

5. That the maximum effect, or nearly so, will be obtained when the pressure of the air is one-tenth that of the steam, or when about two cubic feet of air, at the pressure of the atmosphere and the temperature 60° F., are mixed with each pound of steam.

15. *Remarks.* As this investigation was nearly completed my attention was called to a statement by Sir W. Armstrong, to the effect that Mr Siemens had suggested as an explanation of the otherwise anomalous advantage of forcing air into the boiler of a steam-engine, that the air may prevent, in a great measure, the condensation at the surface of the cylinder. It would thus seem that Mr Siemens has already suggested the probability of the fact which is proved in this investigation. I am not aware, however, that any previous experiments have been made on the subject, and therefore I offer these results as independent testimony of the correctness of Mr Siemens's views as well as of my own.

11.

ON THE FORCES CAUSED BY EVAPORATION FROM, AND CONDENSATION AT, A SURFACE.

[*From the* "Proceedings of the Royal Society," No. 153, 1874.]

(*Read* June 18th, 1874.)

It has been noticed by several philosophers, and particularly by Mr Crookes, that, under certain circumstances, hot bodies appear to repel and cold ones to attract other bodies. It is my object in this paper to point out, and to describe experiments to prove, that these effects are the results of evaporation and condensation, and that they are valuable evidence of the truth of the kinetic theory of gas, viz. that gas consists of separate molecules moving at great velocities.

The experiments of which the explanation will be given were as follows:—

A light stem of glass, with pith-balls on its ends, was suspended by a silk thread in a glass flask, so that the balls were nearly at the same level. Some water was then put in the flask and boiled until all the air was driven out of the flask, which was then corked and allowed to cool. When cold there was a partial vacuum in it, the gauge showing from $\frac{1}{2}$ to $\frac{3}{4}$ of an inch pressure.

It was now found that when the flame of a lamp was brought near to the flask, the pith-ball which was nearest the flame was driven away, and that with a piece of ice the pith was attracted.

This experiment was repeated under a variety of circumstances, in different flasks and with different balances, the stem being sometimes of glass and sometimes of platinum; the results, however, were the same in all cases, except such variations as I am about to describe.

The pith-balls were more sensitive to the heat and cold when the flask was cold and the tension within it low; but the effect was perceptible until the gauge showed about an inch, and even after that the ice would attract the ball.

The reason why the repulsion from heat was not apparent at greater tensions, was clearly due to the convection-currents which the heat generated within the flask. When there was enough vapour, these currents carried the pith with them; they were, in fact, then sufficient to overcome the forces which otherwise moved the pith. This was shown by the fact that when the bar was not quite level, so that one ball was higher than the other, the currents affected them in different degrees; also that a different effect could be produced by raising or lowering the position of the flame.

The condition of the pith also perceptibly affected the sensitiveness of the balls. When a piece of ice was placed against the side of the glass, the nearest of the pith-balls would be drawn towards the ice, and would eventually stop opposite to it. If allowed to remain in this condition for some time, the vapour would condense on the ball near the ice, while the other ball would become dry (this would be seen to be the case, and was also shown, by the tipping of the balance, that ball against the ice gradually getting lower). It was then found, when the ice was removed, that the dry ball was insensitive to the heat, or nearly so, while that ball which had been opposite to the ice was more than ordinarily sensitive.

If the flask were dry and the tension of the vapour reduced with the pump until the gauge showed $\frac{3}{4}$ of an inch, then, although purely steam, the vapour was not in a saturated condition, and the pith-balls which were dry were no longer sensitive to the lamp, although they would still approach the ice.

From these last two facts it appears as though a certain amount of moisture on the balls was necessary to render them sensitive to the heat.

In order that these results might be obtained, it was necessary that the vapour should be free from air. If a small quantity of air was present, although not enough to appear in the gauge, the effects rapidly diminished, particularly that of the ice, until the convection-currents had it all their own way. This agrees with the fact that the presence of a small quantity of air in steam greatly retards condensation and even evaporation.

With a dry flask and an air-vacuum, neither the lamp nor the ice produced their effects; the convection-currents reigned supreme even when the gauge was as low as $\frac{1}{4}$ inch. Under these circumstances the lamp generally attracted the balls and the ice repelled them, *i.e.* the currents carried them towards the lamp and from the ice; but, by placing the lamp or ice very low, the reverse effects could be obtained, which goes to prove that they were the effects of the currents of air.

These experiments appear to show that evaporation from a surface is attended with a force tending to drive the surface back, and condensation with a force tending to draw the surface forward. These effects admit of explanation, although not quite as simply as may at first sight appear.

It seems easy to conceive that when vapour is driven off from a body there must be a certain reaction or recoil on the part of the body; Hero's engine acts on this principle. If a sheet of damp paper be held before the fire, from that side which is opposite to the fire a stream of vapour will be thrown off towards the fire with a perceptible velocity; and therefore we can readily conceive that there must be a corresponding reaction, and that the paper will be forced back with a force equal to that which urges the vapour forwards. And, in a similar way, whenever condensation goes on at a surface it must diminish the pressure at the surface, and thus draw the surface forwards.

It is not, however, wholly, or even chiefly, such visible motions as these that afford an explanation of the phenomena just described. If the only forces were those which result from the perceptible motion, they would be insensible, except when the heat on the surface was sufficiently intense to drive the vapour off with considerable velocity. This, indeed, might be the case if vapour had no particles and were, what it appears to be, a homogeneous elastic medium, and if, in changing from liquid into gas, the expansion took place gradually, so that the only velocity acquired by the vapour was that necessary to allow its replacing that which it forces before it and its giving place to that which follows.

But, although it appears to have escaped notice so far, it follows, as a direct consequence of the *kinetic* theory of gases, that, whenever evaporation takes place from the surface of a solid body or a liquid, it must be attended with a reactionary force equivalent to an increase of pressure on the surface, which force is quite independent of the perceptible motion of the vapour. Also, condensation must be attended with a force equivalent to a diminution of the gaseous pressure over the condensing surface, and likewise independent of the visible motion of the vapour. This may be shown to be the case as follows:—

According to the kinetic theory, the molecules which constitute the gas are in rapid motion, and the pressure which the gas exerts against the bounding surfaces is due to the successive impulses of these molecules, whose course directs them against the surface, from which they rebound with unimpaired velocity. According to this theory, therefore, whenever a molecule of liquid leaves the surface henceforth to become a molecule of gas, it must leave it with a velocity equal to that with which the other particles of gas rebound—that is to say, instead of being just detached and quietly passing off into the gas, it must be shot off with a velocity greater than that of a cannon-ball. Whatever may be the nature of the forces which give it the velocity, and which consume the latent heat in doing so, it is certain, from the principle of conservation of momentum, that they must react on the surface with a force equal to that exerted on the molecule, just as in a gun the pressure of the powder on the breech is the same as on the shot.

The impulse on the surface from each molecule which is driven off by evaporation must therefore be equal to that caused by the rebound of one of the reflected molecules, supposing all the molecules to be of the same size; that is to say, since the force of rebound will be equal to that of stopping, the impulse from a particle driven off by evaporation will be half the impulse received from the stopping and reflection of a particle of the gas. Thus the effect of evaporation will be to increase the number of impulses on the surface; and although each of the new impulses will only be half as effective as the ordinary ones, they will add to the pressure.

In the same way, whenever a molecule of gas comes up to a surface and, instead of rebounding, is caught and retained by the surface, and is thus condensed into a molecule of liquid, the impulse which it will thus impart to the surface will only be one-half as great as if it had rebounded. Hence condensation will reduce the magnitude of some of the impulses, and therefore will reduce the pressure on the condensing surface.

For instance, if there were two surfaces in the same vapour, one of which was dry and the other evaporating, then the pressure would be greater on the moist surface than on that which was dry. And, again, if one of the surfaces was dry and the other condensing, then the pressure would be greater on the dry surface than on that which was condensing. Hence, if the opposite sides of a pith-ball in vapour were in such different conditions, the ball would be forced towards the colder side.

These effects may be expressed more definitely as follows:—

Let v be the velocity with which the molecules of the vapour move,

p the pressure on a unit of surface,

d the weight of a unit of volume of the vapour,

w the weight of liquid evaporated or condensed in a second;

then the weight of vapour which actually strikes the unit of dry surface in a second will be

$$=\frac{dv}{6},$$

and the pressure p will be given by

$$p=2\frac{dv^2}{6g}*,$$

* See Maxwell, *Theory of Heat*, p. 294.

and f (the force arising from evaporation) will be given by

$$f=\frac{wv}{g};$$

therefore

$$f=w\sqrt{\frac{3p}{gd}}.$$

Thus we have an expression for the force in terms of the quantity of water evaporated and the ratio of the pressure to the density of the vapour; and if the heat necessary to evaporate the liquid (the latent heat) is known, we can find the force which would result from a given expenditure of heat.

Applying these results to steam, we find that, at a temperature of 60°, the evaporation of 1 lb. of water from a surface would be sufficient to maintain a force of 65 lbs. for one second.

It is also important to notice that this force will be proportional to the square root of the absolute temperature, and, consequently, will be approximately constant between temperatures of 32° and 212°.

If we take mercury instead of water, we find that the force is only 6 lbs. instead of 65 lbs.; but the latent heat of mercury is only $\frac{1}{30}$ that of water, so that the same expenditure of heat would maintain nearly three times as great a force.

It seems, therefore, that in this way we can give a satisfactory explanation of the experiments previously described. When the radiated heat from the lamp falls on the pith, its temperature will rise, and any moisture on it will begin to evaporate and to drive the pith from the lamp. The evaporation will be greatest on that ball which is nearest to the lamp; therefore this ball will be driven away until the force on the other becomes equal, after which the balls will come to rest, unless momentum carries them further. On the other hand, when a piece of ice is brought near, the temperature of the pith will be reduced, and it will condense the vapour and be drawn towards the ice.

It seems to me that the same explanation may be given of Mr Crookes's experiments; for, although my experiments were made on water and at comparatively high pressures, they were in reality undertaken to verify the explanation as I have given it. I used water in the hope of finding (as I have found) that, in a condensable vapour, the results could be obtained with a greater density of vapour (that is to say, with a much less perfect vacuum), the effect being a consequence of the saturated condition of the vapour rather than of the perfection of the vacuum.

Mr Crookes only obtained his results when his vacuum was nearly as perfect as the Sprengel pump would make it. Up to this point he had nothing but the inverse effects, viz. attraction with heat and repulsion with cold. About the cause of these he seems to be doubtful; but I venture to think that they may be entirely explained by the expansion of the surrounding gas or vapour, and the consequent convection-currents. It must be remembered that whenever the air about a ball is expanded, and thus rendered lighter by heat, it will exercise less supporting or floating power on the ball, which will therefore tend to sink. This tendency will be in opposition to the lifting of the ascending current, and it will depend on the shape and thickness of the ball whether it will rise or fall when in an ascending current of heated gas.

The reason why Mr Crookes did not obtain the same results with a less perfect vacuum was because he had then too large a proportion of air, or non-condensing gas, mixed with the vapour, which also was not in a state of saturation. In his experiments the condensable vapour was that of mercury, or something which required a still higher temperature, and it was necessary that the vacuum should be very perfect for such vapour to be anything like pure and in a saturated condition. As soon, however, as this state of perfection was reached, then the effects were more apparent than in the corresponding case of water. This agrees well with the explanation; for, as previously shown, the effect of mercury would, for the same quantity of heat, be three times as great as that of water; and, besides this, the perfect state of the vacuum would allow the pith (or whatever the ball might be) to move much more freely than when in the vapour of water at a considerable tension.

Of course this reasoning is not confined to mercury and water; any gas which is condensed or absorbed by the balls when cold in greater quantities than when warm would give the same results; and, as this property appears to belong to all gases, it is only a question of bringing the vacuum to the right degree of tension.

There was one fact connected with Mr Crookes's experiments which, independently of the previous considerations, led me to the conclusion that the result was due to the heating of the pith, and was not a direct result of the radiated heat.

In one of the experiments exhibited at the Soirée of the Royal Society, a candle was placed close to a flask containing a bar of pith suspended from the middle: at first, the only thing to notice was that the pith was oscillating considerably under the action of the candle; each end of the bar alternately approached and receded, showing that the candle exercised an influence similar to that which might have been exercised by the torsion of the thread had this been stiff. After a few minutes' observation, however, it became evident that the oscillations, instead of gradually diminishing, as one naturally expected them to do, continued; and, more than this, they actually increased, until one end of the bar passed the light, after which it seemed quieter for a little, though the oscillations again increased until it again passed the light. As a great many people and lights were moving about, it seemed possible that this might be due to external disturbance, and so its full importance did not strike me. Afterwards, however, I saw that it was only to be explained on the ground of the force being connected with the temperature of the pith. During part of its swing one end of the pith must be increasing in temperature, and during the other part it must be cooling. And it is easily seen that the ends will not be hottest when nearest the light, or coldest when furthest away; they will acquire heat for some time after they have begun to recede, and lose it after they have begun to approach. There will, in fact, be a certain lagging in the effect of the heat on the pith, like that which is apparent in the action of the sun on a comet, which causes the comet to be grandest after it has passed its perihelion. From this cause it is easy to see that the mean temperature of the ends will be greater during the time they are retiring than while approaching, and hence the driving force on that end which is leaving will, on the whole, more than balance the retarding force on that which is approaching; and the result will be an acceleration, so that the bar will swing further each time until it passes the candle, after which the hot side of the bar will be opposite to the light, and will for a time tend to counteract its effect, so that the bar will for a time be quieter. This fact is independent evidence as to the nature of the force; and although it does not show it to be evaporation, it shows that it is a force depending on the temperature of the pith, and that it is not a direct result of radiation from the candle.

Since writing the above paper, it has occurred to me that, according to the kinetic theory, a somewhat similar effect to that of evaporation must result whenever heat is communicated from a hot surface to gas.

The particles which impinge on the surface will rebound with a greater velocity than that with which they approached; and consequently the effect of the blow must be greater than it would have been had the surface been of the same temperature as the gas.

And, in the same way, whenever heat is communicated from a gas to a surface, the force on the surface will be less than it otherwise would be, for the particles will rebound with a less velocity than that of which they approach.

Mathematically the result may be expressed as follows—the symbols having the same meaning as before, ϵ representing the energy communicated in the form of heat, and δv the alteration which the velocity of the molecule undergoes on impact. As before,

$$p = \frac{dv^2}{3g} \text{ or } v = \sqrt{\frac{3gp}{d}};$$

and

$$\epsilon = \frac{dv}{6}\frac{(v+\delta v)^2 - v^2}{2g} = \frac{dv^2\delta v}{6g} \text{ nearly,}$$

$$f = \frac{dv}{6g}\delta v;$$

$$\therefore f = \frac{\epsilon}{v} = \epsilon\sqrt{\frac{d}{3gp}}.$$

Therefore, in the case of steam at a temperature of 60°,

$$f = \frac{\epsilon}{2000};$$

and in the case of air

$$f = \frac{\epsilon}{1400}.$$

It must be remembered that ϵ depends on the rate at which cold particles will come up to the hot surface, which is very slow when it depends only on the diffusion of the particles of the gas *inter se* and the diffusion of the heat amongst them.

It will be much increased by convection-currents; but these will (as has been already explained), to a certain extent, produce an opposite effect. It would also seem that this action cannot have had much to do with Mr Crookes's experiments, as one can hardly conceive that much heat could be communicated to the gas or vapour in such a perfect vacuum as that he obtained, unless, indeed, the rate of diffusion varies inversely as the density of a gas*. It will be interesting, however, to see what light experiments will throw on the question†.

* June 10. Professor Maxwell has shown that the diffusion both of heat and of the gas varies inversely as the density; therefore, excepting for convection-currents, the amount of heat communicated from a surface to a gas would be independent of the density of the gas, and hence the force f would be independent of the density; that is to say, this force would remain constant as the vacuum improved, while the convection-currents and counteracting forces would gradually diminish. It seems probable, therefore, that Mr Crookes's results are, at least in part, due to this force.

† For continuation see papers 24 and 35.

12.

ON THE SURFACE-FORCES CAUSED BY THE COMMUNICATION OF HEAT.

[*From the* "Philosophical Magazine" *for November*, 1874.]

IN a paper read before the Royal Society, June 18, I pointed out, as it seemed to me, that whenever evaporation or condensation takes place on a surface, they are attended with certain forces tending respectively to drive the surface back and urge it forward, these forces arising, according to the kinetic theory, from the momentum which is imparted from the surface to the particles driven off, and *vice versâ*. I also pointed out at the end of the paper that similar effects will be produced whenever heat is communicated from a surface to a gas, and *vice versâ*. The possibility of this latter effect only occurred to me as I was on the point of sending off the paper, and consequently was added by way of an appendix. The first part of the paper contains a description of some experiments undertaken to verify my conclusions respecting the forces of evaporation and condensation, the results of which seem to me to be fully explained by these forces; so that had I rewritten the paper after becoming aware of the possible existence of the other force, I should have had nothing to add in connexion with these experiments. I had, however, also endeavoured to show that the first class of forces afforded an explanation of Mr Crookes's experiments; and had this part of the paper been rewritten it would have been somewhat altered, as the last class of forces (those arising from the simple communication of heat) seem to afford a simpler explanation of some of the phenomena observed by Mr Crookes. I regret that this was not done, as, from some remarks in a paper published in the August Number of the *Philosophical Magazine*, I fear that Mr Crookes has not understood my meaning, and has consequently been at the trouble of making further experiments, which, however valuable from other considerations, throw no fresh light on the case in point. However, before proceeding to discuss the subject further, I would set myself straight with Mr Crookes in one or two particulars.

Mr Crookes appears to complain that I did not give him credit for having obtained evidence of repulsion by heat in a medium as dense as that which I used, viz. from $\frac{1}{2}$ to $\frac{3}{4}$ inch of mercury. Now the only account of his experiments which I had seen was the abstract published in the 'Proceedings of the Royal Society,' December 1; and in this the highest pressure at which he definitely states he obtained repulsion is 3 millimetres, or one-tenth of an inch: but this in truth was not the point. In Art. 44 of his paper he describes an experiment in which he did not obtain repulsion until the Sprengel pump had been at work for a long time after the gauge showed half a millimetre. It was the results of this experiment which I was endeavouring to explain, and consequently it was to this experiment that my remarks applied; and I had not the least intention of implying that these were the only results which Mr Crookes obtained. However, had it not been so, had I misread Mr Crookes's paper as he supposed, I think that he would have forgiven me when he sees that he has committed a similar offence against me. He commences his remarks on my paper by saying, "In my exhausted receiver he assumes the presence of aqueous vapour"; whereas nowhere in my paper do I mention any such assumption, nor did it enter into my head to make it. Nay, further, I think I have shown, however darkly, that, under the conditions under which Mr Crookes's experiments were made, aqueous vapour would not be sufficient to explain the results, since it would be to all intents a non-condensable gas. However, enough of this.

So far as I can see, the case now stands thus:—

1. Whenever a body is surrounded by a condensable medium (that is, vapour at its point of saturation), heating or cooling of the body will be respectively attended with evaporation and condensation, and hence with forces over the surface.

2. The amount of evaporation or condensation will not depend on the density of the vapour with which the surface is surrounded, provided only that it be at its point of saturation, but will depend on the amount of heat available; that is to say, it will depend on the amount of heat imparted to or taken from the body. Thus the evaporation of mercury would take place as readily, in a medium of too small density to be measured, as the evaporation of water under the pressure of $\frac{3}{4}$ of an inch.

3. The presence of a non-condensable gas will greatly retard the rate of evaporation and condensation.

4. That under the conditions (1), there will be forces arising from convection-currents in the surrounding medium, which will generally act in opposition to the forces (1), but which will diminish with the density of the medium, while the other forces remain constant and therefore must ultimately prevail.

5. That there is yet another set of forces, which act when the medium is not in a state of saturation, *i.e.* is not condensable. These forces arise from the communication of heat to or from the surface from or to the gas. These forces will be directly proportional to the rate at which the heat is communicated; and since this rate has been shown by Professor Maxwell to be independent of the density of the gas, these forces, like those arising from condensation and evaporation, will be independent of the density of the surrounding medium, and their effect will increase as the density and convection-currents diminish.

These forces would appear, if their magnitude is sufficient, to afford an explanation of all Mr Crookes's results if the medium is not in a state of saturation; but when, as in my experiments, the medium is steam, and water is present in the receiver, or, as I suppose in Mr Crookes's experiments, mercury was present, and the medium was vapour of mercury, or at any rate sulphuric acid, then it would be impossible for the medium to communicate heat to the ball or surface without condensation; and hence in such cases it seems to me that the effects must be due to the forces of condensation.

13.

ON THE EFFECT OF IMMERSION ON SCREW PROPELLERS.

[*From the* "Transactions of the Institution of Naval Architects," 1874.]

(*Read* March 27th, 1874.)

In a paper read before this Institution last year (see paper 9), I showed that the phenomena of screw propulsion called "racing" is due to the difference between the conditions under which a screw works when so far buried below the surface that it does not break the surface, and when by breaking the surface it is able to draw air down behind its blades. The present communication contains the results of some experiments which bear on the same subject, and which are of a somewhat different kind to those previously described.

In these experiments my object was to determine how far the depth of immersion affected the resistance which a screw encounters when not travelling forward—when the boat is stationary.

It has been stated by several writers—and it seems to be a very general impression—that the resistance which the water offers to the turning of a screw is greater at greater depths. This certainly is shown to be the case by the experiments of Messrs Rennie and Maudslay.

Now, neither the friction nor action of liquids against a moving vane is affected by pressure in ordinary circumstances, consequently, this increase of resistance in the case of the screw requires explanation. This explanation is to be found in the action of the air drawn in from the surface; or, rather, I should say, is due to the atmospheric pressure acting when the air is excluded.

When the screw is sufficiently near the surface to draw air down, then it will only be working on a partial stream of water, and the quantity of water which it will be able to draw within its range will depend, not upon the velocity of the screw, but upon the velocity with which fresh water from behind will replace that which the screw removes, and this will obviously depend on the head of water above the cavity, or its depth below the surface. When, however, the screw is once sufficiently below the surface not to draw air, then, owing to the pressure of the atmosphere being added to the head of water, the total will be greater than necessary, and it will be acting on a full stream of water, and no further immersion will affect its action; unless, indeed, it be drawn with sufficient velocity to cause a vacuum behind its blades. Such cases, however, do not come within the range of ordinary experience, for the exclusion of the air has the same effect as an extra immersion of 30 feet.

This explanation is fully established by the following experiments, which also confirm the results of my previous experiments on racing. For in this case the action of racing was invariably attended with frothing, and *vice versâ*.

The screw used in these experiments was 2 inches in diameter; and was connected with a spring, which, in running down, made the screw turn two hundred and forty times. The resistance which the screw encountered was shown by the time taken in running down.

First Series of Experiments, during which the same strength of Spring was used.

Number of Experiment	Depth of Immersion	Time taken to run down	Remarks
		Seconds	
1	1	19	Did not race
2	2	19	"
3	3	20	"
4	2	20	"
5	1	20	"
6	$\frac{5}{8}$	20	"
7	$\frac{1}{2}$	20	Raced a little at starting
8	$\frac{1}{4}$	12	Raced
9	$\frac{3}{16}$	12	"
10	$\frac{1}{8}$	12	"
11	0	12	"
12	0	12	"
13	$-\frac{3}{8}$	10	"
14	$-\frac{1}{8}$	7	"

SECOND SET OF EXPERIMENTS, DURING WHICH THE SAME SPRING WAS USED, BUT WHICH WAS STRONGER THAN THAT USED IN THE PREVIOUS CASE.

Number of Experiment	Depth of Immersion	Time taken to run down	Remarks
		Seconds	
1	3	10	Did not race
2	1	10	"
3	$\frac{5}{8}$	11	Raced at starting
4	$\frac{5}{8}$	11	"
5	$\frac{7}{16}$	9	Raced intermittently
6	$\frac{7}{16}$	9	"
7	$\frac{3}{8}$	6	Raced
8	$\frac{1}{8}$	4	"
9	0	4	"

From these experiments we must conclude, that so long as the screw is not frothing (is working in solid water*), the resistance is independent of the depth of immersion. Hence it follows that it is probable that in Mr Rennie's and Mr Maudslay's experiments the screw was frothing all the time. It must be remembered that, when the screw or boat is stationary, there is a much greater chance of drawing down air than when it is under way. While on one of Mc Ivor's boats—the *Palmyra*—last summer, I observed that whenever we made a start the screw frothed the water, although at the time the tips of its blades did not come within 8 feet of the surface; and when we were under way in calm water there was no froth whatever.

* The term "solid water" is used to express unbroken water, *i.e.*, water without air, not, as is sometimes assumed, undisturbed water. This latter condition is not apparent, for the water is just as clear whatever may be its natural motion, so long as there are no bubbles of air.

14.

ON THE EXTENT AND ACTION OF THE HEATING SURFACE OF STEAM BOILERS.

[*From the Fourteenth Volume of the* "Proceedings of the Literary and Philosophical Society of Manchester." *Session* 1874–5.]

(*Read* October 6, 1874.)

THE rapidity with which heat will pass from one fluid to another, through an intervening plate of metal, is a matter of such practical importance that I need not apologise for introducing it here. Besides its practical value, it also forms a subject of very great philosophical interest, being intimately connected with, if it does not form part of, molecular philosophy.

In addition to the great amount of empirical and practical knowledge which has been acquired from steam boilers, the transmission of heat has been made the subject of direct inquiry by Newton, Dulong and Petit, Péclet, Joule, and Rankine, and considerable efforts have been made to reduce it to a system. But as yet the advance in this direction has not been very great; and the discrepancy in the results of the various experiments is such, that one cannot avoid the conclusion that the circumstances of the problem have not been all taken into account.

Newton appears to have assumed that the rate at which heat is transmitted from a surface to a gas, and *vice versa*, is, *ceteris paribus*, directly proportional to the difference in temperature between the surface and the gas, whereas Dulong and Petit, followed by Péclet, came to the conclusion from their experiments that it followed altogether a different law*.

These philosophers do not seem to have advanced any theoretical reasons

* *Traité de la Chaleur*, Péclet, Vol. I., p. 365.

for the law which they have taken, but have deduced it entirely from their experiments, "à chercher par tâtonnement la loi que suivent ces résultats*."

In reducing these results, however, so many things had to be taken into account, and so many assumptions have been made, that it can hardly be a matter of surprise if they have been misled. And there is one assumption which upon the face of it seems to be contrary to general experience, this is, that the quantity of heat imparted by a given extent of surface to the adjacent fluid is independent of the motion of that fluid or of the nature of the surface†; whereas the cooling effect of a wind compared with still air is so evident that it must cast doubt upon the truth of any hypothesis which does not take it into account.

In this paper I approach the problem in another manner from that in which it has been approached before. Starting with the laws, recently discovered, of the internal diffusion of fluids, I have endeavoured to deduce from theoretical considerations the laws for the transmission of heat, and then verify these laws by experiment. In the latter respect I can only offer a few preliminary results; which, however, seem to agree so well with general experience, as to warrant a further investigation of the subject, to promote which is my object in bringing it forward in the present incomplete form.

The heat carried off by air, or any fluid, from a surface, apart from the effect of radiation, is proportional to the internal diffusion of the fluid at and near the surface, *i.e.*, is proportional to the rate at which particles or molecules pass backwards and forwards from the surface to any given depth within the fluid, thus, if AB be the surface and ab an ideal line in the fluid parallel to AB then the heat carried off from the surface in a given time will be proportional to the number of molecules which in that time pass from ab to AB—that is for a given difference of temperature between the fluid and the surface.

This assumption is fundamental to what I have to say, and is based on the molecular theory of fluids.

Now the rate of this diffusion has been shown from various considerations to depend on two things:—

1. The natural internal diffusion of the fluid when at rest.

2. The eddies caused by visible motion which mixes the fluid up and continually brings fresh particles into contact with the surface.

The first of these causes is independent of the velocity of the fluid, and, if it be a gas, is independent of its density, so that it may be said to depend only on the nature of the fluid‡.

* *Traité de la Chaleur*, Péclet, Vol. I., p. 363. † *Ibid.*, p. 383.

‡ Maxwell's *Theory of Heat*, Chap. XIX.

The second cause, the effect of eddies, arises entirely from the motion of the fluid, and is proportional both to the density of the fluid, if gas, and the velocity with which it flows past the surface.

The combined effect of these two causes may be expressed in a formula as follows:

$$H = At + B\rho vt \quad \text{(I)},$$

where t is the difference of temperature between the surface and the fluid, ρ is the density of the fluid, v its velocity, A and B constants depending on the nature of the fluid, and H the heat transmitted per unit area of the surface in a unit of time.

If, therefore, a fluid were forced along a fixed length of pipe, which was maintained at a uniform temperature greater or less than the initial temperature of the gas, we should expect the following results.

1. Starting with a velocity zero, the gas would then acquire the same temperature as the tube. 2. As the velocity increased the temperature at which the gas would emerge would gradually diminish, rapidly at first, but in a decreasing ratio until it would become sensibly constant and independent of the velocity. The velocity after which the temperature of the emerging gas would be sensibly constant can only be found for each particular gas by experiment; but it would seem reasonable to suppose that it would be the same as that at which the resistance offered by friction to the motion of the fluid would be sensibly proportional to the square of the velocity. It having been found both theoretically and by experiment that this resistance is connected with the diffusion of the gas by a formula:

$$R = A'v + B'\rho v^2 \quad \text{(II)},$$

And various considerations lead to the supposition that A and B in (I) are proportional to A' and B' in (II).

The value of v which this gives is very small, and hence it follows that for considerable velocities the gas should emerge from the tube at a nearly constant temperature whatever may be its velocity.

This, as I am about to point out, is in accordance with what has been observed in tubular boilers, as well as in more definite experiments.

In the Locomotive the length of the boiler is limited by the length of tube necessary to cool the air from the fire down to a certain temperature, say 500°. Now there does not seem to be any general rule in practice for determining this length, the length varying from 16 ft. to as little as 6, but whatever the proportions may be, each engine furnishes a means of comparing the efficiency of the tubes for high and low velocities of the air through them. It has been a matter of surprise how completely the steam-producing power of a boiler appears to rise with the strength of blast or the work required from it. And as the boilers are as economical when working with a high blast as with a low, the air going up the chimney cannot have a much higher temperature in the one case than in the other. That it should be somewhat higher is strictly in accordance with the theory as stated above.

It must, however, be noticed that the foregoing conclusion is based on the assumption that the surface of the tube is kept at the same constant temperature, a condition which it is easy to see can hardly be fulfilled in practice.

The method by which this is usually attempted is by surrounding the tube on the outside with some fluid the temperature of which is kept constant by some natural means, such as boiling or freezing, for instance the tube is surrounded with boiling water. Now although it may be possible to keep the water at a constant temperature, it does not at all follow that the tube will be kept at the same temperature; but on the other hand, since heat has to pass from the water to the tube, there must be a difference of temperature between them, and this difference will be proportional to the quantity of heat which has to pass. And again, the heat will have to pass through the material of the tube, and the rate at which it will do this will depend on the difference of the temperature at its two surfaces. Hence if air be forced through a tube surrounded with boiling water, the temperature of the inner surface of the tube will not be constant, but will diminish with the quantity of heat carried off by the air. It may be imagined that the difference will not be great: a variety of experiments lead me to suppose that it is much greater than is generally supposed. It is obvious that, if the previous conclusions be correct, this difference would be diminished by keeping the water in motion, and the more rapid the motion the less would be the difference. Taking these things into consideration the following experiments may, I think, be looked upon, if not as conclusive evidence of the truth of the above reasoning, yet as bearing directly upon it.

One end of a brass tube was connected with a reservoir of compressed air, the tube itself was immersed in boiling water, and the other end was connected with a small non-conducting chamber, formed of concentric cylinders of paper with intervals between them, in which was inserted the bulb of a thermometer. The air was then allowed to pass through the tube and paper chamber, the pressure in the reservoir being maintained by bellows, and measured by a mercury gauge; the thermometer then indicated the temperature of the emerging air. One experiment gave the following results:—With the smallest possible pressure the thermometer rose to 96° F., and as the pressure increased fell until with $\frac{1}{10}$ inch it was 87°, with

$\frac{1}{4}$ inch it was 70°, with 1 inch it was 64°, with 2 inches 60°, beyond this point the bellows would not raise the pressure.

It appears, therefore, (1) that the temperature of the air never rose to 212, the temperature of the tube, even when moving slowest; but the difference was clearly accounted for by the loss of heat in the chamber from radiation, the small quantity of air passing through it not being sufficient to maintain the full temperature, an effect which must obviously vanish as the velocity of the air increased; (2) as the velocity increased the temperature diminished, at first rapidly, and then in a more steady manner. The first diminution might be expected, from the fact that the velocity was not as yet equal to that at which the resistance of friction is sensibly equal to the square of the velocity, as previously explained. The steady diminution, which continued when the velocity was greater, was due to the cooling of the tube. This was proved to be the case, for at any stage of the operation the temperature of the emerging air could be slightly raised by increasing the heat under the water, so as to make it boil faster, and produce greater agitation in the water surrounding the tube. This experiment was repeated with several tubes of different lengths and characters, some of copper and some of brass, with practically the same results. I have not however as yet been able to complete the investigation, and I hope to be able before long to bring forward another communication before the Society.

I may state that should these conclusions be established, and the constant B for different fluids be determined, we should then be able to determine, as regards length and extent, the best proportion for the tubes and flues of boilers.

15.

ON THE ACTION OF RAIN TO CALM THE SEA.

[*From the Fourteenth Volume of the* "Proceedings of the Literary and Philosophical Society of Manchester." *Session* 1874–5.]

(*Read* January 12, 1875.)

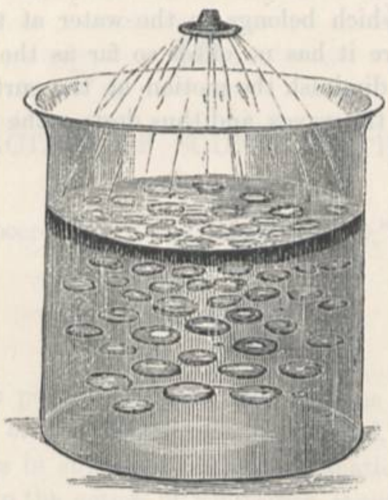
THERE appears to be a very general belief amongst sailors that rain tends to calm the sea, or as I have often heard it expressed, that rain soon knocks down the sea.

Without attaching very much weight to this general impression, my object in this paper is to point out an effect of rain on falling into water which I believe has not been hitherto noticed, and which would certainly tend to destroy any wave motion there might be in the water.

When a drop of rain falls on to water the splash or rebound is visible enough, as are also the waves which diverge from the point of contact; but the effect caused by the drop under the surface is not apparent, because the water being all of the same colour there is nothing to show the interchange of place which may be going on. There is however a very considerable effect produced. If instead of a drop of rain we let fall a drop of coloured water, or better still if we colour the topmost layer of the water, this effect becomes apparent. We then see that each drop sends down one or more masses of coloured water in the form of vortex rings. These rings descend, with a gradually diminishing velocity and with increasing size, to a distance of several inches, generally as much as 18, below the surface.

Each drop sends in general more than one ring, but the first ring is much more definite and descends much quicker than those which follow it.

If the surface of the water be not coloured, this first ring is hardly apparent, for it appears to contain very little of the water of the drop which causes it. The actual size of these rings depends on the size and speed of the drops. They steadily increase as they descend, and before they stop they have generally attained a diameter of from 1 to 2 inches, or even more. The annexed cut shows the effect which may be produced in a glass vessel.



It is not that the drop merely forces itself down under the surface, but in descending carries down with it a mass of water, which, when the ring is 1 inch in diameter, would be an oblate spheroid having a larger axis of 2 inches and a lesser of about $1\frac{1}{2}$ inches. For it is well known that the vortex ring is merely the core of the mass of fluid which accompanies it, the shape of which is much the same as that which would be formed by winding string through and through a curtain ring until it was full.

It is probable that the momentum of these rings corresponds very nearly with that of the drops before impact, so that when rain is falling on to water, there is as much motion immediately beneath the surface as above it, only the drops, so to speak, are much larger and their motion is slower.

Thus besides the splash and surface effect, which the drops produce, they cause the water at the surface rapidly to change places with that at some distance below.

Such a transposition of water from one place to another must tend to destroy wave motion. This may be seen as follows. Imagine a layer of water, adjacent to the surface and a few inches thick, to be flowing in any direction over the lower water, which is to be supposed at rest. The effect of a drop would be to knock some of the moving water into that which is at rest, and a corresponding quantity of water would have to rise up into the moving layer, so that the upper layer would lose its motion by communicating it to the water below. Now when the surface of water is disturbed by waves, besides the vertical motion, the particles move backwards and forwards in a horizontal direction, and this motion diminishes as we proceed downwards from the surface. Therefore in this case, the effect of rain-drops will be the same as in the case considered above, namely, to convey the motion, which belongs to the water at the surface, down into the lower water, where it has no effect so far as the waves are concerned; hence the rain would diminish the motion at the surface, which is essential to the continuance of the waves, and thus destroy the waves.

16.

ON THE REFRACTION OF SOUND BY THE ATMOSPHERE.

[*From the* "Proceedings of the Royal Society," No. 155, 1874.]

(*Read* April 23, 1874.)

My object in this paper is to offer explanations of some of the more common phenomena of the transmission of sound, and to describe the results of experiments in support of these explanations. The first part of the paper is devoted to the action of wind upon sound. In this part of the subject I find that I have been preceded by Professor Stokes, who in 1857 gave precisely the same explanation as that which occurred to me. I have, however, succeeded in placing the truth of this explanation upon an experimental basis; and this, together with the fact that my work upon this part of the subject is the cause and foundation of what I have to say on the second part, must be my excuse for introducing it here. In the second part of the subject I have dealt with the effect of the atmosphere to refract sound upwards, an effect which is due to the variation of temperature, and which I believe has not hitherto been noticed. I have been able to show that this refraction explains the well-known difference which exists in the distinctness of sounds by day and by night, as well as other differences in the transmission of sound arising out of circumstances such as temperature; and I have applied it in particular to explain the very definite results obtained by Professor Tyndall in his experiments off the South Foreland.

The Effect of Wind upon Sound

is a matter of common observation. Cases have been known in which, against a high wind, guns could not be heard at a distance of 550 yards*,

* *Proc. Roy. Soc.*, 1874, p. 62.

although on a quiet day the same guns might be heard from ten to twenty miles. And it is not only with high winds that the effect upon sound is apparent; every sportsman knows how important it is to enter the field on the lee side even when the wind is very light. In light winds, however, the effect is not so certain as in high winds; and (at any rate so far as our ears are concerned) sounds from a small distance seem at times to be rather intensified than diminished against very light winds. On all occasions the effect of wind seems to be rather against distance than against distinctness. Sounds heard to windward are for the most part heard with their full distinctness; and there is only a comparatively small margin between that point at which the sound is perceptibly diminished and that at which it ceases to be audible.

That sound should be blown back by a high wind does not at first sight appear to be unreasonable. Sound is known to travel forward through or on the air; and if the air is itself in motion, moving backwards, it will carry the sound with it, and so retard its forward motion—just as the current of a river retards the motion of ships moving up the stream. A little consideration, however, serves to show that the effect of wind on sound cannot be explained in this way. The velocity of sound (1100 feet per second) is so great compared with that of the highest wind (50 to 100 feet per second), that the mere retardation of the velocity, if that were all, would not be apparent. The sound would proceed against the wind with a slightly diminished velocity, at least 1000 feet per second, and with a but very slightly diminished intensity.

Neither can the effect of wind be solely due to its effect on our hearing. There can be no doubt that during a high wind our power of hearing is damaged; but this is the same from whatever direction the sound may come; and hence from this cause the wind would diminish the distance at which sounds could be heard, whether they moved with it or against it, whereas this is most distinctly not the case. Sounds at right angles to the wind are but little affected by it; and in moderate winds sounds can be heard further with the wind than when there is none.

The same may be said against theories which would explain the effect of wind as causing a heterogeneous nature in the air so that it might reflect the sound. All such effects must apply with equal force with and against the wind.

This question has baffled investigators for so long a time, because they have looked for the cause in some direct effect of the motion of the air, whereas it seems to be but incidentally due to this. The effect appears, after all, not to be due simply to the wind, but to the difference in the velocity with which the air travels at the surface of the ground and at a height above it; that is to say, if we could have a perfectly smooth surface which would not retard the wind at all, then the wind would not obstruct sound in the way it does, for it would all be moving with an equal velocity; but, owing to the roughness of the surface and the obstructions upon it, there is a gradual diminution in the velocity of the wind as it approaches the surface. The rate of this diminution will depend on the nature of the surface; for instance, in a meadow the velocity at 1 foot above the surface is only half what it is at an elevation of 8 feet, and smaller still compared with what it is at greater heights.

To understand the way in which this variation in the velocity affects the sound, it is necessary to consider that the velocity of the waves of sound does depend on the velocity of the wind, although not in a great degree. To find the velocity of the sound with the wind we must add that of the wind to the normal velocity of sound, and against the wind we must subtract the velocity of the wind from the 1100 feet per second (or whatever may be the normal velocity of the sound) to find the actual velocity. Now if the wind is moving at 10 feet per second at the surface of a meadow, and at 20 feet per second at a height of 8 feet, the velocity of the sound against the wind will be 1090 feet per second at the surface, and 1080 feet per second at 8 feet above the surface; so that in a second the same wave of sound will have travelled 10 feet further at the surface than at a height of 8 feet. This difference of velocity would cause the wave to tip up and proceed in an upward direction instead of horizontally. For if we imagine the front of a wave of sound to be vertical to start with, it will, after proceeding for one second against the wind, be inclined at an angle of more than 45°, or half a right angle; and since sound-waves always move in a direction perpendicular to the direction of the front (that is to say, if the waves are vertical they will move horizontally, and not otherwise), after one second the wave would be moving upwards at an angle of 45° or more. Of course, in reality, it would not have to proceed for one second before it began to move upwards: the least forward motion would be followed by an inclination of the front backwards, and by an upward motion of the wave. A similar effect would be produced in a direction opposite to that of the wind, only as the top of the wave would then be moving faster than the bottom, the waves would incline forwards and move downwards. In this way the effect of the wind is to lift the waves which proceeded to windward, and to bring those down which move with it.

Thus the effect of wind is not to destroy the sound, but to raise the ends of the wave, which would otherwise move along the ground, to such a height that they pass over our heads.

When the ends of the waves are raised from the ground they will tend to diverge down to it, and throw off *secondary waves*, or, as I shall call them, *diverging waves*, so as to reconstitute the gap that is thus made. These secondary waves will be heard as a continuation of the sound, more or less faint, after the primary waves are altogether above our heads. [This phenomenon of divergence presents many difficulties, and has only as yet been dealt with for particular cases. It may, however, be assumed, from what is known respecting it, that in the case of sound being lifted up from the ground by refraction, or, what is nearly the same thing, passing directly over the crest of a hill so that the ground falls away from the rays of sound, diverging waves would be thrown off very rapidly at first and for a considerable distance, depending on the wave-length of the sound; but as the sound proceeds further the diverging rays, would gradually become fainter and more nearly parallel to the direct rays, until at a sufficient distance they would practically cease to exist, or, at any rate, be no greater than those which cause the diffraction-bands in a pencil of light*. The divergence would introduce bands of diffraction or interference within the direct or geometrical path of the sound, as in the case of light. These effects would also be complicated by the reflection of the diverging waves from the ground, which, crossing the others at a small angle, would also cause bands of interference. The results of all these causes would be very complicated, but their general effect would be to cause a rapid weakening of the sound at the ground from the point at which it was first lifted; and as the sound became weaker it would be crossed by bands of still fainter sound, after which, the diverging rays, as well as the direct rays, would be lifted, and at the ground nothing would be heard.—*September* 1874.]

If we leave out of consideration the divergence, then we may form some idea as to the path which the bottom of the sound, or the rays of sound (considered as the rays of light), would follow. If the variation in the speed of the wind were uniform from the surface upwards, then the rays of sound would at first move upwards, very nearly in circles. The radii of these circles may be shown to be $1100 \times \dfrac{h}{v_1 - v_2}$, where v_1 and v_2 are the velocities of the wind in feet per second at elevations differing by h feet. In fact, however, the variation is greatest at the ground, and diminishes as we proceed upwards, so that the actual path would be more nearly that of a parabola.

Also, owing to this unequal variation in the velocity, those parts of the waves immediately adjacent to the ground will rise more rapidly than the

* Taking sound of 1 foot wave-length, and comparing it with light whose wave-length is the 50,000th part of an inch, then the divergence of the sound at a mile from the point at which it left the ground would be comparatively the same as that of the light at $\frac{1}{10}$ of an inch from the aperture at which the pencil was formed.

part immediately above them; hence there will be a crowding of the waves at a few feet from the ground, and this will lead to an intensifying of the sound at this point. Hence, notwithstanding the divergence, we might expect the waves to windward to preserve their full intensity so long as they were low enough to be heard. And this is in accordance with the fact, often observed, that sounds at short distances are not diminished but rather intensified when proceeding against the wind.

It will at once be perceived that by this action of the wind the distance to which sounds can be heard to windward must depend on the elevation of the observer and the sound-producing body. This does not appear to be a fact of general observation. It is difficult to conceive how it can have been overlooked, except that, in nine cases out of ten, sounds are not continuous, and thus do not afford an opportunity of comparing their distinctness at different places. It has often astonished me, however, when shooting, that a wind which did not appear to me to make the least difference to the direction in which I could hear small sounds most distinctly, should yet be sufficient to cover one's approach to partridges, and more particularly to rabbits, even until one was within a few feet of them—a fact which shows how much more effectively the wind obstructs sound near the ground than even a few feet above it.

Elevation, however, clearly offered a crucial test whether such an action as that I have described was the cause of the effect of wind upon sound. Having once entertained the idea, it was clearly possible to put it to the test in this way. Also, if the principles hold in sound, something analogous must hold in the case of waves on the surface of a running stream of water —for instance, waves made near the bank of a river.

I had just reached the point of making such tests when I discovered that the same views had been propounded by Professor Stokes so long ago as 1857*. Of course, after such a discovery, it seemed almost unnecessary for me to pursue the matter further; but as there were one or two points about which I was not then quite certain, and as Prof. Stokes's paper does not appear to be so well known as it might be (I do not know of one writer on sound who has adopted this explanation), it still seemed that it might be well, if possible, to put the subject on an experimental basis. I therefore made the experiments I am about to describe; and I am glad that I did not rest content without them, for they led me to what I believe to be the discovery of refraction of sound by the atmosphere.

The results of my first observation are shown in Fig. 1. This represents the shape of the waves as they proceeded outwards from a point near the

* *Brit. Assoc. Report*, 1857, Trans. of Sect. p. 22.

bank of a stream about 12 feet wide. Had the water been at rest there would have been semicircular rings; as it was, the front of the waves up the

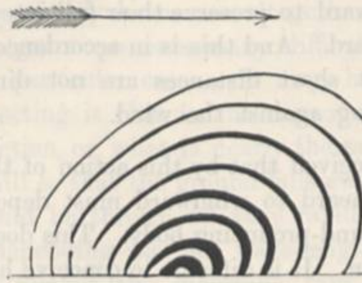


Fig. 1.

stream made an obtuse angle with the wall, which they gradually left. The ends of the waves, it will be observed, gradually died out, showing the effect of divergence. The waves proceeding down the stream were, on the other hand, inclined to the wall, which they approached.

I was able to make a somewhat better observation in the Medlock, near the Oxford Road Bridge, Manchester. A pipe sent a succession of drops into the water at a few inches from the wall, which, falling from a considerable height, made very definite waves. Fig. 2 represents a sketch of these

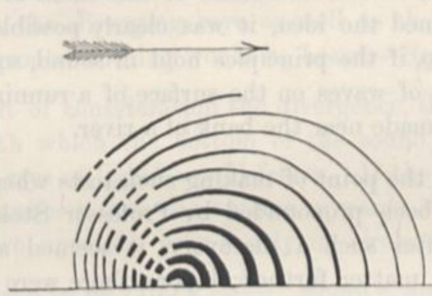


Fig. 2.

waves, made on the spot: the diverging waves from the ends of the direct waves, and also the bands of interference, are very clearly seen. Both these figures agree with what has been explained as the effect of wind on sound.

In the next place I endeavoured to ascertain the effect which elevation has on the distance to which sound can be heard against a wind. In making these experiments I discovered some facts relating to the transmission of sound over a rough surface, which, although somewhat obvious, appear hitherto to have escaped attention.

My apparatus consisted of an electrical bell, mounted on a case containing a battery. The bell was placed horizontally on the top of the case, so that it could be heard equally well in all directions; and when standing on the ground the bell was 1 foot above the surface. I also used an anemometer.

These experiments were made on four different days, the 6th, 9th, 10th, and 11th of March. On the first of these the wind was very light, on the others it was moderately strong, strongest on the second and fourth; on all four the direction was the same, viz. north. On the two last days the ground was covered with snow, which gave additional interest to the experiments, inasmuch as it enabled me to compare the effect of different surfaces. On the first two days I was alone, but on the last two I had the assistance of Mr J. B. Millar, of Owens College, whose ears were rather better than mine, although I am not aware of any deficiency in this respect. The experiments were all made in the same place, a flat meadow of considerable extent.

The General Results of the Experiments.

De La Roche*, in his experiment, found that the wind produced least effect on the sound at right angles to its direction, *i.e.* sounds could be heard furthest in this direction. His method of experimenting, however, was not the same as mine. He compared the sounds from two equal bells, and in all cases placed the bells at such distances that the sounds were equally distinct. I, on the other hand, measured the extreme distance at which the sounds could be heard, the test being whether or not the observer noticed a break in the continuity of sound, a stoppage of the bell. The difference in our method of experimenting accounts for the difference in our results. I found in every case that the sound could be heard further with the wind than at right angles to its direction; and when the wind was at all strong, the range with the wind was more than double that at right angles. It does not follow, however, nor was the fact observed, that at comparatively short distances the sound with the wind was more intense than at right angles.

The explanation of this fact, which was fully borne out by all the experiments, is that the sound which comes in immediate contact with the ground is continually destroyed by the rough surface, and the sound from above is continually diverging down to replace that which has been destroyed. These diverging waves are in their turn destroyed; so that there is a gradual weakening of the intensity of the waves near the ground, and this weakening extends upwards as the waves proceed. Therefore, under ordinary circumstances, when there is no wind the distant sounds

* *Annales de Chimie*, Vol. I. p. 177 (1816).

which pass above us are more intense than those which we hear. Of this fact I have abundant evidence. On the 6th, when the wind was light, at all distances greater than 20 yards from the bell the sound was much less at the ground than a few feet above it; and I was able to recover the sound after it had been lost in every direction by mounting on to a tree, and even more definitely by raising the bell on to a post 4 feet high, which had the effect of doubling the range of the sound in every direction except with the wind, although even in this the range was materially increased.

It is obvious that the rate at which the sound is destroyed by the ground will depend on the roughness of its surface. Over grass we might expect the sound at the ground to be annihilated, whereas over water it would hardly be affected. This was shown to be the case by the difference in the range at right angles to the wind over grass, and over the same ground when completely covered with snow. In the latter case I could hear the sound at 200 yards, whereas I could only hear it at 70 or 80 in the former.

Now, owing to the fact that the sound is greater over our heads than at the ground, any thing which slowly brings down the sound will increase the range. Hence, assuming that the action of the wind is to bring down the sound in the direction in which it is blowing, we see that it must increase its range in this direction. And it must also be seen that in this direction there will be less difference in the intensity of the sound from the ground upwards than in other directions. This was observed to be the case on all occasions. In the direction of the wind, when it was strong, the sound could be heard as well with the head on the ground as when raised, even when in a hollow with the bell hidden from view by the slope of the ground; and no advantage whatever was gained either by ascending to an elevation or raising the bell. Thus, with the wind over the grass the sound could be heard 140 yards, and over snow 360 yards, either with the head lifted or on the ground; whereas at right angles to the wind on all occasions the range was extended by raising either the observer or the bell.

It has been necessary to notice these points; for, as will be seen, they bear directly on the question of the effect of elevation on the range of sound against the wind.

Elevation was found to affect the range of sound against the wind in a much more marked manner than at right angles.

Over the grass no sound could be heard with the head on the ground at 20 yards from the bell, and at 30 yards it was lost with the head 3 feet from the ground, and its full intensity was lost when standing erect at

30 yards. At 70 yards, when standing erect, the sound was lost at long intervals, and was only faintly heard even then; but it became continuous again when the ear was raised 9 feet from the ground, and it reached its full intensity at an elevation of 12 feet.

Over the snow similar effects were observed at very nearly equal distances. There was this difference, however, the sound was not entirely lost when the head was lowered or even on the ground. Thus at 30 yards I could still hear a faint sound. Mr Millar could hear this better than I could; he, however, experienced the same increase on raising his head. At 90 yards I lost the sound entirely when standing on the ground, but recovered it again when the ear was 9 feet from the ground. Mr Millar, however, could hear the sound very faintly, and at intervals, at 160 yards; but not with his head on the ground. At this point I was utterly unable to hear it; and even at an elevation of 25 feet I gave it up as hopeless. However, as Mr Millar by mounting 10 feet higher seemed to hear it very much better, I again ascended; and at an elevation of 33 feet from the ground I could hear it as distinctly as I had previously heard it when standing at 90 yards from the bell. I could not hear it 5 feet lower down; so that it was the last 5 feet which had brought me into the foot of the wave. Mr Millar experienced the same change in this 5 feet. As the sound could now be heard as strong as at a corresponding distance with the wind, we thought we had reached the full intensity of the waves. This, however, was not the case; for the least raising of the bell was followed by a considerable intensifying of the sound; and when it was raised 6 feet I could hear each blow of the hammer distinctly, although just at that time a brass band was playing in the distance. It seemed to me that I could hear it as distinctly as at 30 yards to leeward of the bell. All these results were repeated on both days with great uniformity.

When more than 30 yards to the windward of the bell, the raising of the bell was always accompanied by a marked intensifying of the sound, and particularly over the grass. I could only hear the bell at 70 yards when on the ground; yet when set on a post 5 feet high I heard it at 160 yards, or more than twice the distance. This is a proof of what I previously pointed out, that the waves rise faster at the ground than they do high up, and crowding together they intensify. In all cases there was an unmistakable greater distinctness of the sound from short distances to windward than to leeward or at right angles.

Except when the sound was heard with full force it was not uniform. The bell gave two sounds (the beats of the hammer and the ring) which could be easily distinguished; and at times we could hear only the ring, and at others the beats. The ring seemed to preserve itself the longest above the ground; whereas near the ground at short distances the ring was lost first. This is explained by the fact that the rate at which sound-waves diverge depends upon their note: the lower the note the more will they diverge. Thus the beats diverge more rapidly than the ring, and consequently die out sooner; whereas when the head is on the ground near the bell it is only the diverging waves that are heard, and here the beats have the best chance. The intensity of the sound invariably seemed to waver; and as one approached the bell from the windward side, the sound did not intensify uniformly or gradually, but by fits or jerks; this was the result of crossing the rays' interference, such as those shown in fig. 2.

During the observations the velocity of the wind was observed from time to time at points 1 foot and 8 feet above the surface.

On the 9th, that is over grass, it varied from 4 feet per second at 1 foot and 8 feet per second at 8 feet, to 10 feet per second at 1 foot and 20 feet per second at 8 feet, always having about twice the velocity at 8 feet that it had at 1 foot above the ground.

Over the snow there was not quite so much variation above and below. On the 10th the wind varied from 3 feet per second at 1 foot to 4 feet per second at 8 feet*. On the 11th the variation was from 12 at 1 foot and 19 at 8 feet to 6 at 1 foot and 10 at 8 feet. Thus over snow the variation in the velocity was only about one-third instead of half.

Since the foregoing account was written, I have had an opportunity of experimenting on a strong west wind (on the 14th of March); and the results of these experiments are, if anything, more definite than those of the previous ones. The wind on this occasion had a velocity of 37 feet per second at an elevation of 12 feet, and of 33 at 8 feet, and 17 at 1 foot. The experiments were made in the same meadows as before, the snow having melted, so that the grass was bare.

With the wind I could hear the bell at 120 yards, either with the bell on the ground or raised 4 feet above it. At right angles to the direction of the wind it ranged about 60 yards with the bell on the ground, and 80 yards when the bell was elevated.

To windward, with the bell standing on the ground (which, it must be remembered, means that the bell was actually 1 foot above the surface), the sound was heard as follows:—

	Full.	Lost.
With the head close to the ground...	At 10 yards.	At 20 yards.
Standing	„ 30 „	„ 40 „
At an elevation of 25 feet	*Not heard* at 90 yards.	

* The wind fell rapidly towards the close of the observations on this day.

With the bell at an elevation of 4 feet 6 inches:—

	Full.	Lost.
Head to the ground	At 18 yards.	At 30 yards.
Standing up	„ 40 „	„ 60 „
At an elevation of 12 feet		„ 90 „
At an elevation of 18 feet	„ 90 „	

These results entirely confirm those of the previous experiments; and the intensifying of the sounds to windward by the raising of the bell was even more marked than before; for at 90 yards to windward, with the bell raised, I could hear it *much* more distinctly than at a corresponding distance to leeward. This fact calls for a word of special explanation; it is clearly due to the fact that the variation in the velocity of the air is much greater near the ground than at a few feet above it. When the bell is on the ground all the sound must pass near the ground, and will all be turned up to a nearly equal extent; but when the bell is raised, the rays of sound which proceed horizontally will be much less bent or turned up than those which go down to the ground; and consequently, after proceeding some distance, these rays will meet or cross, and if the head be at this point they will both fall on the ear together, causing a sound of double intensity. It is this crossing of the rays also which for the most part causes the interference seen in fig. 2.

These experiments establish three things with regard to the transmission of sound:—

1. That when there is no wind, sound proceeding over a rough surface is more intense above than below.

2. That as long as the velocity of the wind is greater above than below, sound is lifted up to windward and is not destroyed.

3. That under the same circumstances it is brought down to leeward, and hence its range extended at the surface of the ground.

These experiments also show that there is less variation in the velocity of the wind over a smooth surface than over a rough one.

It seems to me that these facts fully confirm the hypotheses propounded by Prof. Stokes, that they place the action of wind beyond question, and that they afford explanations of many of the anomalous cases that have been observed; for instance, that sounds can be heard much further over water than over land, and also that a light wind at sea does not appear to affect sound at all, the fact being that the smooth water does not destroy either the sound or the motion of the air in contact with it. When the wind and sea are rough the case is different.

The Effect of Variations of Temperature.

Having observed how the wind acts to lift the waves of sound, by diminishing their velocity above compared with what it is below, it was evident to me that any other atmospheric cause which would diminish the velocity above, or increase that below, would produce the same effect, viz. would cause the waves to rise.

Such a cause must at certain times exist in the variation in the condition of the air as we proceed upwards from the surface.

Although barometric pressure does not affect the velocity of sound, yet, as is well known, the velocity of sound depends on the temperature*, and every degree of temperature between 32° and 70° adds approximately 1 foot per second to the velocity of sound. The velocity also increases with the quantity of moisture in the air; but the quantity is at all times too small to produce an appreciable result. This vapour nevertheless plays an important part in the phenomena under consideration; for it gives to the air a much greater power of radiating and absorbing heat, and thus renders it much more susceptible of changes in the action of the sun.

If, then, the air were all at the same temperature and equally saturated with moisture, the velocity of sound would be the same at all elevations; but if the temperature is greater, or if it contains more water below than above, then the wave of sound will proceed quicker below than above, and will be turned up in the same way as against a wind. This action of the atmosphere is, strictly speaking, analogous to the refraction of light. In light, however, it is density which retards motion: temperature and pressure have little or nothing to do with it; and since the density increases downwards, the rays of light move slower below than they do above, and are therefore bent downwards, and thus the distance at which we can see objects is increased. With sound, however, since it is temperature which affects the velocity, the reverse is the case; the rays are bent upwards, and the distance from which we can hear is reduced.

It is a well-known fact that the temperature of the air diminishes as we proceed upwards, and that it also contains less vapour. Hence it follows that, as a rule, the waves of sound must travel faster below than they do above, and thus be refracted or turned upward.

* It varies as the square root of $\frac{\text{pressure}}{\text{density}}$, and consequently as the square root of the absolute temperature.

The variation of temperature is, however, by no means constant, and a little consideration serves to show that it will be greatest in a quiet atmosphere when the sun is shining. The sun's rays, acting most powerfully on that air which contains the most vapour, warms the lower strata more than those above them; and besides this, they warm the surface of the earth, and this warmth is taken up by the air in contact with it. It is not, however, only on such considerations as these that we are in a position to assert the law of variation of atmospheric temperature. Mr Glaisher has furnished us with information on the subject which places it beyond the region of surmise.

I extract the following from his "Report on Eight Balloon Ascents in 1862" (*Brit. Assoc. Rep.* 1862, p. 462):—

"From these results the decline of temperature when the sky was cloudy

For the first	300 feet was	0°·5	for every	100 feet.
From 300 to 3400	„	0°·4	„	„
„ 3400 to 5000	„	0°·3	„	„

"Therefore in cloudy states of the sky the temperature of the air decreased nearly uniformly with the height above the surface of the earth nearly up to the cloud.

"When the sky was partially cloudy the decline of temperature

In the first 100 feet was 0°·9

* * * *

From 2900 to 5000 „ 0°·3 for every 100 feet.

"The decline of temperature near the earth with a partially clear sky is nearly double that with a cloudy sky.

"In some cases, as on July 30th, the decline of temperature in the first 100 feet was as large as 1°·1."

We may say, therefore, that when the sky is clear the variation of temperature, as we proceed upwards from 1 to 3000 feet, will be more than double what it is when the sky is cloudy. And since for such small variations the variation in the velocity of sound, that is the refraction, is proportional to the temperature, this refraction will be twice as great with a clear sky as when the sky is cloudy.

This is the mean difference, and there are doubtless exceptional cases in which the variations are both greater and less than those given; during the night the variations are less than during the day, and again in winter than in summer.

This reasoning at once suggested an explanation of the well-known fact that sounds are less intense during the day than at night. This is a matter of common observation, and has been the subject of scientific inquiry. F. De La Roche discusses the subject, and exposes the fallacies of several theories advanced to account for it. Amongst others there are some remarks by Humboldt, in which he says that the difference is not due to the quietness of the night, for he had observed the same thing near the torrid zone, where the day seemed quieter than the night, which was rendered noisy with insects.

It is, however, by the experiments of Prof. Tyndall that this fact has been fully brought to light; and from their definite character they afford an opportunity of applying the explanation, and furnish a test of its soundness.

Neglecting the divergence of the bottom of the waves, a difference of 1 degree in the 100 feet would cause the rays of sound, otherwise horizontal, to move on a circle, the radius of which by the previous rule is:

$$1100 \cdot \tfrac{100}{1} \text{ or } 110{,}000 \text{ feet.}$$

A variation of one-half this would cause them to move on a circle of 220,000 feet radius. From the radii of these circles we can calculate the range of the sound from different elevations.

With a clear sky, *i.e.* with a radius 110,000 feet, from an elevation of 235 feet the sound would be audible with full force to 1·36 mile; the direct sound would then be lifted above the surface, and only the diverging sound would be audible. From an elevation of 15 feet, however, the direct sound might be heard to a distance of ·36, or $\frac{1}{3}$ mile further, so that in all it could be heard 1·72 ($1\frac{3}{4}$) mile.

With a cloudy sky, *i.e.* with a radius 220,000 feet, the direct sound would be heard to 2·4 miles from an elevation of 15 feet, or 1·4 times what it is with the clear sky. These results have been obtained by taking the extreme variations of temperature at the surface of the earth. At certain times; however, in the evening, or when it was raining, the variation would be much less than this, in which case the direct sound would be heard to much greater distances.

[So far I have only spoken of the direct or geometrical rays of sound, that is, I have supposed the edge of the sound to be definite, and not fringed with diverging rays; but, as has been already explained, the sound would diverge downwards, and from this cause would be heard to a considerable distance beyond the point at which the direct rays first left the ground. From this point, however, the sound would become rapidly fainter until it was lost. The extension which divergence would thus add to the range of the sound would obviously depend on the refraction—that is to say, when the direct rays were last refracted upwards, the extension of the range due to divergence would be greatest. It is difficult to say what the precise effect of this divergence would be; but we may assume that it would be similar to that which was found in the case of wind, only the refraction being so much smaller the extension of the range by divergence would be greater. On the whole the results calculated from the data furnished by Mr Glaisher agree in a remarkable manner with those observed; for if we add $\frac{1}{4}$ mile for the extension of the range by divergence, the calculated distance with a clear sky would be two miles from a cliff 235 feet high. —*September* 1874.]

Now Prof. Tyndall found that from the cliffs at the South Foreland, 235 feet high, the minimum range of sound was a little more than 2 miles, and that this occurred on a quiet July day with hot sunshine. The ordinary range seemed to be from 3 to 5 miles when the weather was dull, although sometimes, particularly in the evening, the sounds were heard as far as 15 miles. This was, however, only under very exceptional circumstances. Prof. Tyndall also found that the interposition of a cloud was followed by an almost immediate extension of the range of the sound. I extract the following passages from Prof. Tyndall's Report:—

"On June 2 the maximum range, at first only 3 miles, afterwards ran up to about 6 miles.

"Optically, June 3 was not at all a promising day; the clouds were dark and threatening, and the air filled with a faint haze; nevertheless the horns were fairly audible at 9 miles. An exceedingly heavy rain-shower approached us at a galloping speed. The sound was not sensibly impaired during the continuance of the rain.

"July 3 was a lovely morning: the sky was of a stainless blue, the air calm, and the sea smooth. I thought we should be able to hear a long way off. We steamed beyond the pier and listened. The steam-clouds were there, showing the whistles to be active; the smoke-puffs were there, attesting the activity of the guns. Nothing was heard. We went nearer; but at two miles horns and whistles and guns were equally inaudible. This, however, being near the limit of the sound-shadow, I thought that might have something to do with the effect, so we steamed right in front of the station, and halted at $3\frac{3}{4}$ miles from it. Not a ripple nor a breath of air disturbed the stillness on board, but we heard nothing. There were the steam-puffs from the whistles, and we knew that between every two puffs the horn-sounds were embraced, but we heard nothing. We signalled for the guns; there were the smoke-puffs apparently close at hand, but not the slightest sound. It was mere dumb-show on the Foreland. We steamed in to 3 miles, halted, and listened with all attention. Neither the horns nor the whistles sent us the slightest hint of a sound. The guns were again signalled for; five of them were fired, some elevated, some fired point-blank at us. Not one of them was heard. We steamed in to two miles, and had the guns again fired: the howitzer and mortar with 3-lb. charges yielded the faintest thud, and the 18-pounder was quite unheard.

"In the presence of these facts I stood amazed and confounded; for it had been assumed and affirmed by distinguished men who had given special attention to this subject, that a clear, calm atmosphere was the best vehicle of sound: optical clearness and acoustic clearness were supposed to go hand in hand * * *.

"As I stood upon the deck of the 'Irené' pondering this question, I became conscious of the exceeding power of the sun beating against my back and heating the objects near me. Beams of equal power were falling on the sea, and must have produced copious evaporation. That the vapour generated should so rise and mingle with the air as to form an absolutely homogeneous mixture I considered in the highest degree improbable. It would be sure, I thought, to streak and mottle the atmosphere with spaces, in which the air would be in different degrees saturated, or it might be displaced by the vapour. At the limiting surfaces of these spaces, though invisible, we should have the conditions necessary to the production of partial echoes, and the consequent waste of sound.

"Curiously enough, the conditions necessary for the testing of this explanation immediately set in. At 3.15 P.M. a cloud threw itself athwart the sun, and shaded the entire space between us and the South Foreland. The production of vapour was checked by the interposition of this screen, that already in the air being at the same time allowed to mix with it more perfectly; hence the probability of improved transmission. To test this inference the steamer was turned and urged back to our last position of inaudibility. The sounds, as I expected, were distinctly though faintly heard. This was at 3 miles distance. At 3¾ miles we had the guns fired, both point-blank and elevated. The faintest thud was all that we heard; but we did hear a thud, whereas we had previously heard nothing, either here or three-quarters of a mile nearer. We steamed out to 4¼ miles, when the sounds were for a moment faintly heard, but they fell away as we waited; and though the greatest quietness reigned on board, and though the sea was without a ripple, we could hear nothing. We could plainly see the steam-puffs which announced the beginning and the end of a series of trumpet-blasts, but the blasts themselves were quite inaudible.

"It was now 4 P.M., and my intention at first was to halt at this distance, which was beyond the sound range, but not far beyond it, and see whether the lowering of the sun would not restore the power of the atmosphere to transmit the sound. But after waiting a little, the anchoring of a boat was suggested; and though loth to lose the anticipated revival of the sounds myself, I agreed to this arrangement. Two men were placed in the boat, and requested to give all attention, so as to hear the sound if possible. With perfect stillness around them, they heard nothing. They were then instructed to hoist a signal if they should hear the sounds, and to keep it hoisted as long as the sounds continued.

"At 4.45 we quitted them and steamed towards the South Sand Head light-ship. Precisely fifteen minutes after we had separated from them the flag was hoisted. The sound, as anticipated, had at length succeeded in piercing the body of air between the boat and the shore.

"On returning to our anchored boat, we learned that when the flag was hoisted the horn-sounds were heard, that they were succeeded after a little time by the whistle-sounds, and that both increased in intensity as the evening advanced. On our arrival of course we heard the sounds ourselves.

"The conjectured explanation of the stoppage of the sounds appeared to be thus reduced to demonstration; but we pushed the proof still further by steaming further out. At $5\frac{3}{4}$ miles we halted and heard the sounds. At 6 miles we heard them distinctly, but so feebly that we thought we had reached the limit of the sound range; but while we waited the sound rose in power. We steamed to the Varne buoy, which is $7\frac{3}{4}$ miles from the signal-station, and heard the sounds there better than at 6 miles distance.

"Steaming on to the Varne light-ship, which is situated at the other end of the Varne shoal, we hailed the master, and were informed by him that up to 5 P.M. nothing had been heard. At that hour the sounds began to be audible. He described one of them as 'very gross, resembling the bellowing of a bull,' which very accurately characterizes the sound of the large American steam-whistle. At the Varne light-ship, therefore, the sounds had been heard towards the close of the day, though it is $12\frac{3}{4}$ miles from the signal station."

Here we see that the very conditions which actually diminished the range of the sound were precisely those which would cause the greatest lifting of the waves. And it may be noticed that these facts were observed and recorded by Prof. Tyndall with his mind altogether unbiassed with any thought of establishing this hypothesis. He was looking for an explanation in quite another direction. Had it not been so he would probably have ascended the mast, and thus found whether or not the sound was all the time passing over his head. On the worst day an ascent of 30 feet should have extended the range nearly $\frac{1}{4}$ mile.

The height of the sound-producing instruments is apparently treated as a subordinate question by Prof. Tyndall. At the commencement of his lecture he stated that the instruments were mounted on the top and at the bottom of the cliff; and he subsequently speaks of their being 235 feet above him. He does not, however, take any notice of the comparative range of those on the top and those at the bottom of the cliff; but wherever he mentions them he speaks of them as on the cliff, leading me to suppose that for some reason those at the bottom of the cliff had been abandoned, or that they were less efficient than those above. If I am right in this surmise, if the sounds from below did not range so far as those from above, it is a fact in accordance with refraction, but of which, I think, Prof. Tyndall has offered no explanation.

[Besides the results of Prof. Tyndall's experiments there are many other phenomena which are explained by this refraction. Humboldt could hear the falls of Orinoco three times as loud by night as by day at a distance of one league; and he states that the same phenomenon has been observed near every waterfall in Europe. And although Humboldt gave another explanation*, which was very reasonable when applied to the particular case at Orinoco†, yet it must be admitted that the circumstances were such as would cause great upward refraction; and hence there can be but little doubt that refraction had a good deal to do with the diminution of the sound by day.

In fact if this refraction of sound exists, then, according to Mr Glaisher's observations, it must be seldom that we can hear distant sounds with anything like their full distinctness, particularly by day; and any elevation in the observer or the source of the sound above the intervening ground will increase this range and distinctness, as will also a gentle wind, which brings the sound down and so counteracts the effect of refraction. And hence we have an explanation of the surprising distances to which sounds can sometimes be heard, particularly the explosion of meteors, as well as a reason for the custom of elevating church-bells and sounds to be heard at great distances.—*September* 1874.]

* "That the sun acts upon the propagation and intensity of sound by the obstacles met in currents of air of different density, and by the partial undulations of the atmosphere arising from unequal heating of different parts of the soil. . . . . During the day there is a sudden interruption of density wherever small streamlets of air of a high temperature rise over parts of the soil unequally heated. The sonorous undulations are divided, as the rays of light are refracted wherever strata of air of unequal density are contiguous. The propagation of sound is altered when a stratum of hydrogen gas is made to rise over a stratum of atmospheric air in a tube closed at one end; and M. Biot has well explained, by the interposition of bubbles of carbonic acid gas, why a glass filled with champagne is not sonorous so long as that gas is evolved and passing through the strata of the liquid."—*Humboldt's Travels*, Bohn's Series, Vol. II., p. 264.

† The sounds proceeded over a plane covered with rank vegetation interspersed with black rocks. These latter attained a very considerable elevation of temperature under the effects of the tropical sun, as much as 48° C., while the air was only 28°; and hence over each rock there would be a column of hot air ascending.

17.

ON THE EFFICIENCY OF BELTS OR STRAPS AS COMMUNICATORS OF WORK.

[*From* "The Engineer," Nov. 27, 1874.]

It has often been remarked that it seems to be impossible so to construct belts that they should drive without slipping. I am not aware that any reason has ever been given for this; but, on the other hand, most writers seem to have assumed that if the belt is made sufficiently tight, so that the tension on the slack side is from one-half to one-quarter that on the tight side, according as the strap is in contact with one-half or the whole of the pulleys, it will not slip. The object of this communication is to show that not only is a reason to be given for this residual slipping, but that it follows a definite law, depending on the elasticity of the strap, and independent of its tightness over and above what is necessary to prevent it slipping bodily round the wheel.

When a pulley, A, is connected with another pulley B by a belt, so that A drives B, it is usual to assume that the surfaces of the two pulleys move with the same velocity, namely, the velocity of the strap; and that the work communicated from A to B equals this velocity multiplied by the difference in the tension on the two sides of the belt. This law would doubtless be true if the strap were inelastic, and did not stretch at all under the tension to which it is subjected; but as all straps are more or less elastic, it can be shown that this law does not hold rigorously, although with such an elastic material as leather it is not far from the truth.

Owing to its elasticity, the tight side of the belt will be more stretched than the slack or slacker side, and will, in consequence, have to move faster. This is easily seen when we consider that each point on the strap completes its entire circuit in the same time, so that if at any instant a number of marks were made on the strap at different points, these marks would all return to the same points in precisely the same time; for the velocity at each point would be equal to the length of strap which passes that point, and on the tight side this would be the stretched length; whereas on the other side it would be the unstretched length, and hence the two sides of the strap would move with different velocities, according to the degree in which the strap is more stretched on the one side than on the other.

Now the stretching of a strap will be proportional to the tension, although the degree will depend on its size and the material of which the strap is composed. Let $\lambda\tau$ represent the increase in length per foot in a certain strap, caused by a tension of τ lb. Then, if τ_1 and τ_2 represent the tensions on the two sides of the belt respectively, the stretching on these two sides will be respectively proportional to $\lambda\tau_1$ and $\lambda\tau_2$ and the difference will be proportional to $\lambda(\tau_1 - \tau_2)$. Therefore the velocities of the two sides will be in the ratio: $\dfrac{1 + \lambda\tau_1}{1 + \lambda\tau_2}$ or $1 + \lambda(\tau_1 - \tau_2)$ nearly.

Again, it is easy to see that the velocity of the tight side of the strap must be equal to that of the surface of the pulley A which drives it; whereas the velocity of the pulley B which is driven by the strap, will be the same as that of the slack side of the strap; and hence the velocities of the two pulleys differ in the ratio $\dfrac{1 + \lambda(\tau_1 - \tau_2)}{1}$. And since the turning effort of the strap on either pulley is the same, namely, $\tau_1 - \tau_2$, the difference of its tensions, the work done by A, which equals its velocity multiplied by this effort, will be greater than that taken up by B in the ratio $\dfrac{1 + \lambda(\tau_1 - \tau_2)}{1}$. This excess of work will have been spent in the slipping, or more properly the creeping of the strap round the pulleys. The manner in which this creeping takes place is easily seen, as follows:—The strap comes on to A tight and stretched, and leaves it unstretched. It has therefore contracted while on the pulley. This contraction takes place gradually from the point at which it comes on to that at which it leaves, and the result is that the strap is continually slipping over the pulley to the point at which it first comes on. In the same way with B; the strap comes on unstretched and leaves it stretched, and has expanded while on the wheel, which expansion takes place gradually from the point at which the strap comes on until it leaves.

The proportion which the slipping bears to the whole distance travelled by the strap $= \lambda(\tau_1 - \tau_2)$, which, as previously shown, is the proportion which the work lost bears to the whole work done by A. From this it appears that the slipping and work lost are proportional to λ, *i.e.* to the increase which a tension of 1 lb would cause in 1 ft. length of the strap; and hence, the more inextensible the material is, the better it is suited for belts.

The actual amount of this slipping may be calculated when we know the elasticity of the belts. With leather it is very small. One belt, which had been in use about two years, and was 1·25 in. wide and $\frac{3}{16}$ thick—the usual thickness—increased in length by sixteen thousandths under a tension of 100 lb. From this example it appears that, for a leather belt of breadth b inches,

$$\lambda = \frac{20}{100000} \cdot \frac{1}{b}.$$

Hence the ratio of slipping $= \cdot 0002 \frac{1}{b} (\tau_1 - \tau_2)$; and in practice $\tau_1 - \tau_2$ varies from 20 lb. to 60 lb. per inch width of belt; therefore the slipping $= \cdot 008$, or nearly 1 per cent. With new straps it would probably be more. With soft elastic materials, such as india-rubber, the slipping is very much greater. In some instances I have been able to make the driving pulley A turn twice as fast as the pulley B, simply in virtue of this expanding and contracting on the pulleys. This shows at once how it is that elastic straps, such as can be made of soft india-rubber, have never come into use, a fact which is otherwise somewhat astonishing, considering for how many purposes an elastic connection of this sort would be useful. A similar explanation to the above may also be given for the friction occurring when elastic tires are used for the wheels of carriages and engines. The tire is perpetually expanding between the wheel and the ground. As the wheel rolls on to the tire, it is continually elongating the part between it and the ground which is in front of the point at which the pressure is greatest. This elongation can only be accomplished by sliding the tire over both the surface of the wheel and the ground, against whatever friction there may be; and similarly, towards the back of the wheel, the tire is contracting, also against friction. Even when there is no tire, if either the wheel or the ground is elastic, a similar action takes place; and hence we may probably explain what is usually called rolling friction*, which has been observed to take place no matter how true or hard the surface of the wheel and the plane on which it rolls may be.

* See paper 18.

18.

ON ROLLING-FRICTION.

[*From the* "Philosophical Transactions of the Royal Society of London," vol. 166, part 1.]

(*Read* June 17, 1875.)

Introduction.

ALTHOUGH the motion of wheels and rollers over a smooth plane is attended with much less resistance or friction than the sliding of one flat surface over another, however smooth, yet practically it has been found impossible to get rid of resistance altogether. Coulomb made some experiments on the resistance which wooden rollers meet with when rolling on a wooden plane, from which experiments he deduced certain laws connecting this resistance with the size of the rollers and the force with which they are pressed on to the plane. These laws have been verified and extended to other materials by Navier and Morin, and are now set forth in many mechanical treatises as "*the laws of resistance to rolling.*" It does not appear, however, that any systematic investigation of this resistance has ever been undertaken, or any attempts made to explain its nature. When hard surfaces are used it is very small, and it has doubtless been attributed to the inaccuracies of the surfaces and to a certain amount of crushing which takes place under the roller. On closer examination, however, it appears that these causes, although they doubtless explain a great part of the resistance which occurs in ordinary practice, are not sufficient to explain the resistance altogether; and that, if they could be removed, there would still be a definite resistance depending on the size and weight of the roller and on the nature of the material of which it and the plane are composed. If it were not so, a perfectly true roller when rolling on a perfectly true surface ought to experience no resistance, however soft the roller and the plane might be, provided both were made of perfectly elastic material so that the one did not permanently crush the other; and we might expect, although these conditions are not absolutely fulfilled, that a roller of iron would roll as easily on a surface of india-rubber as on one of iron, or that an india-rubber roller would experience no more resistance than one of iron when rolling on a true plane. Such, however, is not the case. The resistance with india-rubber is very considerable; my experiments show it to be ten times as great as with iron. I am not aware that this fact has been previously recognized; and that it has often been overlooked is proved by the numerous attempts which have been made to use india-rubber tires for wheels, the invariable failure of which may, I think, in the absence of any other assigned cause, be fairly attributed to the excessive resistance which attends their use. Another fact which I do not think has been hitherto noticed, but of which I have had ample evidence, and which clearly shows the existence of some hitherto unexplained cause of resistance to rolling, is the tendency which a roller has to oscillate about any position in which it may be placed on a flat surface.

However true and hard the roller and the surface may be, if the roller is but slightly disturbed it will not move continuously in one direction until it gradually comes to rest, but it will oscillate backwards and forwards through a greater or less angle, depending on the softness of the material. These oscillations are not due to the roller having settled into a hollow. This is strongly implied by the fact that the more care is taken to make the surfaces true and smooth the more regular and apparent do the oscillations become. But even if this is not a sufficient proof—if it is impossible to suppose that an iron roller on an iron plane can be made so true that when the one is resting on the other it will not be able to find some minute irregularities or hollows in which to settle—still we must be convinced when we find the same phenomenon existing when india-rubber is substituted for iron, and in such a marked degree that no irregularities there may be in the surface produce any effect upon it, much less serve to account for it.

These phenomena, with others, have led me to conclude that there is a definite cause for the resistance to rolling besides the mere crushing of the surface or accidental irregularities of shape, a cause which is connected with the softness of the material as well as with the size and weight of the roller.

Such a force, if its existence be admitted, must either be considered as exhibiting some hitherto unrecognized action of matter on matter, or must be supposed to arise in some intelligible manner from the known actions. The latter is the most natural supposition; and it is my object in this paper to show that this force arises from what is ordinarily known as friction. It is to imply this connexion that I have gone back to the name Rolling-Friction in place of the more general title resistance to rolling ("résistance au roulement").

which Coulomb and subsequent writers have chosen avowedly because they did not wish to imply such a connexion.

The assumption that this force is due to friction necessarily implies that there is slipping between the roller and the plane at the point of contact; and on the other hand, if it can be shown that there is slipping, it follows as a natural consequence that there must be friction or resistance to rolling. Therefore the question as to whether the resistance to rolling is due to friction, reduces itself into a question as to whether there is any evidence of slipping between the roller and the surface on which it rolls.

My attention was first called* to the possibility of such slipping while considering a phenomenon in the action of endless belts when used to transmit rotary motion from one pulley to another, namely, that it is impossible to make the belt tight enough entirely to prevent slipping and cause the surfaces of the two pulleys to move with identically the same velocity. It appears that this slipping is due to the elasticity of the belt, and, since all material is more or less elastic, cannot altogether be prevented. This becomes apparent when we consider that of the two parts of the belt which stretch from pulley to pulley, the one is tighter and hence more stretched than the other, that is, when the belt is transmitting power. For that side which is most stretched, and consequently thinner, will have to move faster than the slacker side in order to prevent the belt accumulating at one pulley; and the speed of the driving-pulley will be equal to that of the tight side of the belt, while the speed of the following pulley will be equal to that of the slack side. This difference of speed requires that the belt shall slip over the pulleys; and this slipping takes place by the expansion and contraction of the belt on the pulleys as it passes from the tight side to the slack side, and *vice versâ*. With leather belts this slipping is very small; but with soft india-rubber it becomes so great as practically to bar the use of this material for driving-belts.

The recognition of this slipping at once suggested to me that there must be an analogous slipping when a hard roller rolls on a soft surface, or when an india-rubber wheel rolls on a hard surface. A single experiment was sufficient to prove that such was the case—an iron roller rolled through something like three-quarters of an inch less in a yard when rolling on india-rubber than when rolling on wood or iron.

Having made this discovery, I proceeded to investigate the subject, and have obtained what I think to be satisfactory evidence that, whatever may be the material of which the plane and the roller are composed, the deformation at the point of contact always causes slipping, although, owing to the hardness of the materials, it may be far too small to be measured.

* (See the preceding paper.)

In the following pages I shall first show that the deformation at the point of contact caused by the weight of the roller must affect the distance rolled through, that it must cause slipping, and that this slipping will be attended with friction. I shall then show that the friction will itself considerably modify the deformation which would otherwise take place, and endeavour to trace the exact nature of the actual deformation. The result of my experiments will then be given, together with the description of certain other causes of rolling-friction which appear under certain circumstances to exist. In conclusion, I shall indicate the direction in which I hope to continue the investigation, consider its bearing on the laws discovered by Coulomb, and discuss certain phenomena connected with the wear of railway-wheels which have been hitherto unexplained, and which serve to illustrate the importance of the subject.

The Distance Rolled through.

If a perfectly hard cylinder rolled on a perfectly hard plane and there were no slipping, then the distance which the cylinder would pass over in one revolution would be exactly equal to its circumference; but if, from the weight of the cylinder or any cause, the length of the surface either of the cylinder or the plane underwent an alteration near the point of contact, then the distance traversed in one revolution would not be equal to the natural length of the circumference. For example, suppose that an iron cylinder is rolling on a surface of india-rubber across which lines have been drawn at intervals of ·01 of an inch, and suppose that as the cylinder rolls across these lines the surface of the india-rubber extends so that the intervals become equal to ·011 of an inch, closing again after the cylinder is past, then the cylinder will measure its circumference, so to speak, on the extended plane, and the actual distance rolled through when measured on the contracted surface will be one-tenth less than the circumference. In the same way there would be an alteration in the distance rolled through if the surface of the roller extended or if either of the surfaces contracted.

In the subsequent remarks I shall call the distance which the roller would roll through if there were no extension or contraction its geometrical distance.

Since no material is perfectly hard, when a heavy roller rests on a surface, the weight of the roller will cause it to indent the surface to a greater or less extent, according to the softness of the latter; and in the same way the surface of the cylinder will be flattened at the point of contact in the manner shown in Fig. 1.

This indentation and flattening will alter the lengths of the surfaces at the point of contact, and will therefore affect the progress of the roller. When a body of any shape is compressed in one direction it extends in the other directions; hence the weight of the roller resting on the plane will, by com-

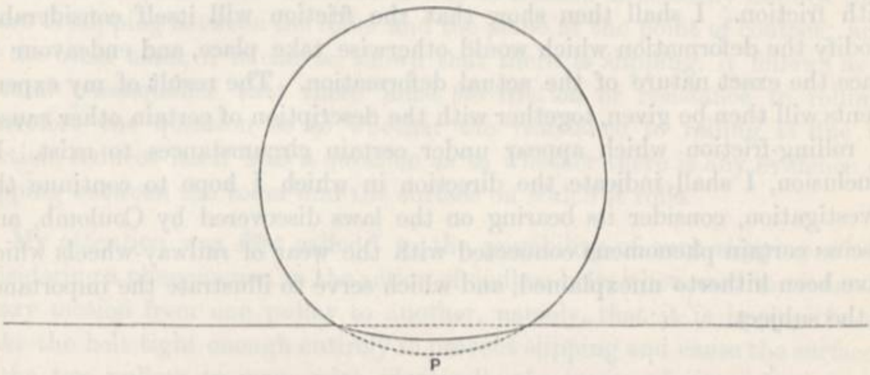


Fig. 1.

pressing the material of the plane in a vertical direction, cause it to extend laterally at the point of contact, and thus the length of the surface which the cylinder actually rolls over would be greater than the length measured on the undisturbed plane. From this cause, therefore, the cylinder would roll through less than its geometrical distance.

On the other hand, the surface of the roller would also be extended (squeezed out) in a similar manner by the pressure of the plane at the point of contact; and hence the surface of the roller would be greater than its natural length, and this would cause the roller to roll through more than its geometrical distance.

To a certain extent, therefore, the expansion of the surface of the roller would counteract the expansion of the plane; and if the two were of the same material, then the one of these extensions would, if nothing interfered to prevent it, exactly counteract the other. But if the one was harder than the other, then the effect on the harder one would be least. Thus an iron cylinder rolling on an india-rubber plane would roll through *less than* its geometrical distance; whereas, inversely, an india-rubber roller on an iron plane would roll through *more than* its geometrical distance.

These things actually take place. But there is, besides softness, another circumstance, not hitherto mentioned, which affects the lateral extension of the surface when compressed by the roller, viz. the shape of the surface.

A little consideration will be sufficient to show that a curved indent in a flat surface will have a greater effect to extend the surface than a flat indent on a rounded surface. In the case of the rounded surface it will be seen that the effect of vertical compression to a certain extent counteracts the effect of lateral expansion; whereas in the case of the flat surface these things are reversed, and the effect of the surrounding material to uphold that which is depressed will increase the lateral expansion.

From this cause, therefore, even if the cylinder and the plane were made of the same material, there would still be a difference in the lateral extension of the surfaces at the point of contact, depending on the smallness of the diameter of the cylinder, and this difference would still cause the cylinder to roll through less than its geometrical distance.

If, instead of on a plane, the one cylinder rolled on another parallel cylinder under a force tending towards the centre, then, if the two cylinders were of the same material and their diameters were equal, they would roll through their geometrical distance; but if the one was larger than the other, the largest would be most retarded.

It appears, therefore, that there are two independent causes which affect the progress of a roller on a plane—the relative softness of the materials and the diameter of the roller. Of these the curvature of the roller always acts to retard its progress; while the other (the relative softness) to retard or to accelerate, according as the plane is softer than the cylinder, or *vice versâ*. These two causes will therefore act in conjunction or in opposition, according to whether the roller is harder or softer than the plane. In the former case the roller will be retarded, whereas in the latter it will depend on the relation between the relative softness and the diameter of the cylinder, whether its progress is greater than, less than, or equal to its geometrical progress. Thus an iron roller on an india-rubber plane will make less than its geometrical progress; while an india-rubber cylinder on an iron plane will make more than, less than, or exactly its geometrical progress, according to the relation between its diameter and softness, or, what comes to the same thing, its weight, which conclusions are borne out by experiment.

The Slipping.

The lateral extension of the material, and the effect this has on the progress of the roller, causes slipping between the surface of the roller and that of the plane; for the surface of the roller, owing to the indentation and flattening, really touches the surface of the plane over an area of some extent; and the pressure between these surfaces, which is greatest towards the middle of the area in which they touch, will shade off to nothing at the edges. Thus deformation is allowed to go on between the surfaces after they have come in contact, and is performed by the slipping of the one over the other.

The Friction.

The slipping is performed against friction, and therefore gives rise to resistance to the motion of the roller.

This resistance will obviously be proportional to the work spent in overcoming the friction between the surfaces during a certain extent of motion; and at first sight it appears as if this would be proportional to the coefficient of friction between these surfaces. When I first commenced this investigation I was under the impression that such would be the case, and that by oiling the surfaces the resistance to rolling might be considerably reduced. Finding by experiment, however, that this was not the case, that although in certain cases the effect of oiling or blackleading the surfaces does reduce the resistance to rolling, yet this reduction is never great, and in some cases the effect appeared to be reversed, it occurred to me that the friction would itself modify the deformation which would otherwise take place after contact had commenced, and by preventing slipping might diminish the work that would otherwise have been spent.

The Deformation.

The action of friction to prevent the deformation at any point of the surfaces in contact will obviously depend on two things—the magnitude of the friction, and the force tending to slide the one surface over the other. If P (Fig. 1, p. 114) be the point of greatest pressure, the possible friction will gradually diminish with the pressure as the distance from P increases; whereas we may assume that the tendency of the one surface to slip over the other will be nothing at P, and will gradually increase with the distance; so that for a certain distance the friction may be sufficient to prevent slipping altogether, but beyond this distance slipping will go on in an increasing ratio.

The effect of oiling the surface would therefore be to diminish the region of no slipping, and increase the area over which slipping goes on, as well as the extent of slipping at each point. These effects would to a certain extent counteract the advantage gained by the reduced coefficient of friction; and it may well be conceived that under certain circumstances they would overbalance it, and that the oil would actually increase the resistance.

The effect which friction has upon the deformation beneath the roller, as well as the general nature of this deformation, will be rendered clearer by examining the effect of friction under circumstances of a less complicated nature than those of rolling.

A Soft Bar between Hard Plates.

Let Fig. 2 represent the end or a section of a long rectangular bar of india-rubber, or any elastic material, placed between two flat plates. Suppose

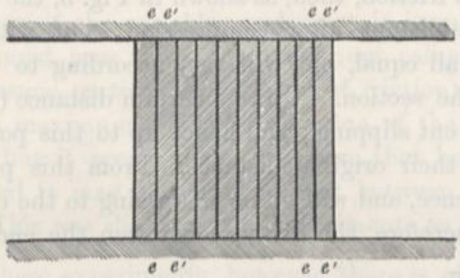


Fig. 2.

these plates to approach each other, compressing the india-rubber, which will extend laterally. Now if there were no friction between the rubber and the plates, then the surfaces in contact with the plates would extend in the same proportion as the rest of the bar, and the section would preserve its rectilinear form, as shown in Fig. 3.

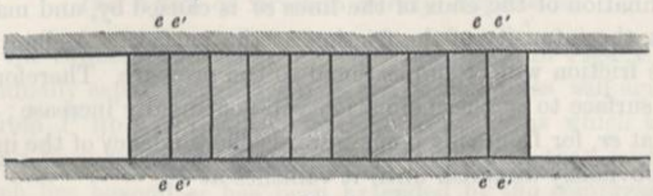


Fig. 3.

With friction, however, the case would be different. The friction would prevent the surface of the india-rubber expanding laterally to the same extent

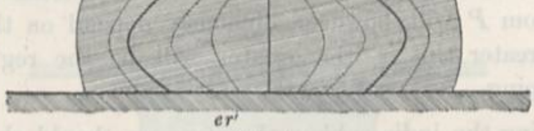


Fig. 4.

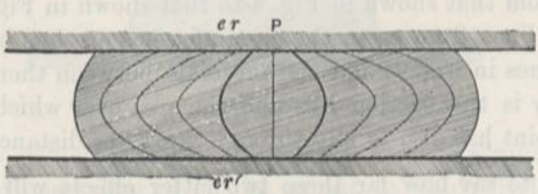


Fig. 5.

as the rest of the bar, and the section would lose its rectilinear form, and bulge out in the middle, as shown in Figs. 4 and 5.

If we imagine the section of the bar to have been marked with a series of lines (ee') initially vertical and at equal intervals apart, these lines will, when the bar is compressed, assume the form shown in the figures.

If there were no friction, then, as shown in Fig. 3, the ends of these lines would still be equidistant after compression; but with friction the intervals will not be all equal, but will vary according to their distance from P, the middle of the section. Up to a certain distance (er) the friction will be sufficient to prevent slipping; and hence up to this point the ends of the lines will preserve their original distance. From this point (er), however, slipping will commence, and will go on increasing to the edge of the surface. From this point, therefore, the distance between the ends of the lines will continually increase.

With regard to the distribution of the pressure between the india-rubber and the plates:—Without friction this will obviously be uniform over the whole surface. Friction, however, will not only increase the mean intensity of the pressure, but will also alter its distribution, causing it to be greatest at P and gradually diminish towards the edge.

The inclination of the ends of the lines ee' is caused by, and may be taken to represent, the intensity of the friction at the surface. As long as there is slipping, the friction will be proportional to the pressure. Therefore from the edge of the surface to er the inclination will continually increase; and it will be greatest at er, for from this point inwards the tendency of the india-rubber to slip will obviously diminish until it vanishes at P.

The distance of er from P will not depend on the degree of compression, at all events so long as this is but small, for the tendency to extend laterally will be proportional to the intensity of the pressure; and since the friction is proportional to the pressure, it will increase at all points in the same ratio as the forces tending to extend the rubber laterally. The distance of er from P will, however, obviously depend on the coefficient of friction. The greater this is, the greater will be the region over which there is no slipping.

By blackleading the india-rubber, therefore, we should change the shape of the section from that shown in Fig. 4 to that shown in Fig. 5, in which all the ends of the lines from er to the circumference are less inclined than the corresponding lines in Fig. 4, and the intervals between them greater, showing that not only is the friction less and the area over which it acts greater, but that each point has also to slip through a greater distance.

It is difficult to say how far these two latter effects will compensate for the former. We may, however, show that there must be some value of the coefficient of friction for which the work spent in overcoming the friction will be a maximum; for when the coefficient was very great, er would be at the circumference, and there would be no slipping, and hence no work spent in friction; whereas if the coefficient were zero, *er* would be at P, and there would be no friction and consequently no work lost in overcoming it. Therefore the work spent in friction, which is a function of the coefficient of friction, is zero for two values of the variable; and since it is positive of all intermediate values, it must pass through a maximum value. Hence for some position of *er* (for some particular coefficient of friction) the work spent in friction would be a maximum. What this value of the coefficient is it is impossible to say; but it seems to be less than that between clean india-rubber and iron, and it may be less than that between blackleaded india-rubber and iron. This was shown by the experiments on rolling-friction.

In considering these experiments, however, there is another thing to be taken into account besides the work spent in friction during compression, and that is the effect of friction during restitution; for the action of a roller as it passes over the india-rubber will be first to compress it and then to allow it to expand again in a corresponding manner.

The Effect of Friction during Expansion.

If, after the rubber has been compressed as shown in Figs. 4 and 5, the surfaces gradually separate again, the shape of the lines will again change. The lines from P up to *er* will assume the same forms which they had at corresponding periods of the compression; but since that portion of the surface which lies beyond *er* has been extended by the compression, it will have to contract as the surfaces recede, and the friction of the surface will oppose such contraction. Hence the lines, which during compression were curved outwards, will gradually straighten and curve inwards, as shown in Fig. 6. Those at the edges will take the form first, and then those nearer to *er*, until the expansion has become complete.

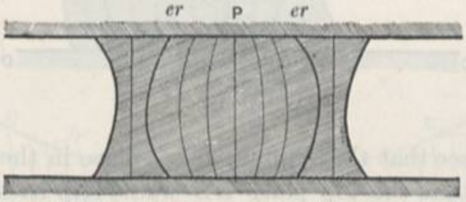


Fig. 6.

The extent to which friction will deform the india-rubber during this operation will obviously depend on the extent to which friction has allowed the surfaces to expand during compression. The smaller the friction the greater will be this expansion, and consequently the further they will have to contract, and the greater will be the pressure under which contraction must take place. It is obvious, therefore, that the work spent in friction during the recoil will increase up to a certain point as the coefficient of friction diminishes; and it would appear to be this increase which mainly balances the advantage which is gained during compression by reducing the coefficient.

It is evident that the action of friction to prevent contraction during restitution, will tend to reduce not only the mean pressure, but also the whole pressure, for exactly the same reason as by preventing expansion the friction increases these pressures during compression. Therefore, for every distance between the plates, after the curves become inclined inwards, the pressure on the surface would be less than at the same distance with no friction, and in a still greater degree than during compression with friction. We can see at once, therefore, that of the work spent in compressing the material only a part would be returned during restitution. The difference is what is spent in overcoming the friction.

The Direction of the Friction.

In Figures 5 and 6 the direction of slipping is opposite on opposite sides of P. If, however, we conceive one half of the bar, that towards A, to have been compressed and to be expanding again, while the other half, that towards B, is being compressed, and the distance between the plates which hold both parts to be the same, we may imagine the plate AB to have been first inclined towards A and then towards B so as to raise the end A. Then the lines would assume the form shown in Fig. 7.

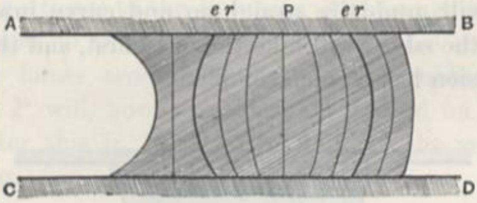


Fig. 7.

In this case we see that the slipping takes place in the same direction on both sides of P, so that the top plate AB would slip backwards in direction A over the india-rubber, while, on the other hand, the india-rubber would slip forwards in the direction D over the lower plate.

The turning of the plate AB, which has been supposed to be going on in Figure 7, represents very closely the action of a roller in compressing the material beneath it; and this case affords us an illustration of the way in which the lateral extension of the material under the roller, or of the roller itself, will, by causing slipping, alter the distance travelled by the roller. If the roller be hard and the surface on which it rolls soft, then the top plate AB may be taken to represent the roller, and, as has just been explained, this slips back; whereas if the roller be soft and the surface hard, then we may take the india-rubber to represent the roller, and this slips forward.

A Continuous Surface.

It is clear that in the case of the bar shown in Fig. 7 the slipping will diminish as the coefficient of friction increases. There is, however, an important difference between this case and that of a roller, in which it is not the entire breadth of a bar that is compressed, but a portion of a continuous surface; for whatever lateral extension there may be immediately under the roller must be compensated by a lateral compression immediately in front and behind it. The greater the lateral extension under the roller, the greater will be the lateral compression; and since the action of the roller is continually to change the one for the other, the one effect will to a certain extent counteract the other; so that in this case we need not expect to find the diminution attended with a corresponding increase in the ostensible slipping. This will be rendered clearer by examining these circumstances as they affect rolling.

The Deformation caused by a Roller.

Fig. 8 may be taken to represent a section of an iron cylinder on an india-rubber plane. The lines on the plane are supposed to represent lines initially vertical and at equal distances apart. The motion of the roller is towards B. P is the point of greatest compression directly below the centre of the roller; er and fr' limit the surfaces over which there is no slipping; D is the point at which contact commences, and C that at which it ceases.

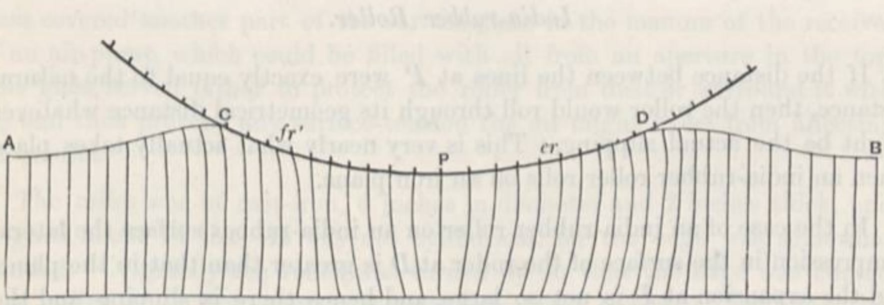


Fig. 8.

The portions of the india-rubber immediately without C and D are laterally compressed; this, as has already been pointed out, is to make room for the lateral extension under the roller from C to D. From D towards B, therefore, and from C towards A the parallel lines are somewhat distorted, and at something less than their natural distance apart. From D to er vertical compression and lateral expansion are going on, and the lines are convex outwards. From er to P there is no slipping and the lines straighten. From P to fr', which is greater than the corresponding distance from P to er, there is no slipping, and at fr' the lines are convex outwards. From fr' to C vertical expansion and lateral contraction take place, so that the lines are all concave outwards. The lateral expansion from D to er and the lateral contraction from fr' to C can only take place by the slipping of the india-rubber over the iron. Its extent is shown by the distance between the corresponding lines on the india-rubber and those on the iron, which latter have been set out equal to the distance between the lines on the rubber where greatest, namely from er to fr'.

The Actual and Apparent Slipping.

Since there is no slipping at P, it is clear that the roller will roll through less than its geometrical distance, inasmuch as the geometrical distance between the lines on the plane at P is greater than their natural distance. Therefore the ostensible slipping will be equal to the difference between the intervals marked on the roller and the initial distance between those on the rubber. The actual slipping, however, is equal to the difference between the intervals on the roller and the intervals on the rubber at D or C, which latter are less than the natural distance; therefore the actual slipping is greater than the ostensible in proportion to the compression at C and D; and since this is increased by diminishing the coefficient of friction, such a diminution will affect the actual slipping in a greater degree than it affects the ostensible. This is in accordance with what has already been stated.

India-rubber Roller.

If the distance between the lines at P were exactly equal to the natural distance, then the roller would roll through its geometrical distance whatever might be the actual slipping. This is very nearly what actually takes place when an india-rubber roller rolls on an iron plane.

In the case of an india-rubber roller on an india-rubber surface the lateral compression in the surface of the roller at D is greater than that in the plane, and the expansion at P is not so large, and hence there is slipping, and the roller will not accomplish its geometrical distance.

In this explanation I have referred to india-rubber because it is much more easy to conceive the effects on it than on a hard substance like iron, the expansion and contraction of which is quite inappreciable to our senses; the reasoning, however, applies equally well to all elastic substances, and is quite independent of their hardness or softness. That friction is sufficient to prevent the expansion of iron at a surface against which it is squeezed out is amply proved by the fact that when a block of iron, hot or cold, is squeezed on an anvil the iron bulges out in the middle, as shown in Fig. 4.

Experimental Verification of the Figures.

The figures which illustrate the foregoing remarks are not altogether ideal, for they have been verified to a certain extent by experiments on india-rubber; for instance, by drawing vertical lines on the edge of a plate of india-rubber, and then observing these lines as the roller passed along as near as possible to this edge; also by observing lines drawn in the same way on the edge of an india-rubber roller. The effect of friction to prevent expansion, shown in Figures 4 and 5, was verified by marking the surface of the india-rubber under the plate AB with parallel lines in chalk, which left a mark on the iron and showed how far there had been slipping. The figures are nevertheless intended rather to illustrate the nature of the slipping and various effects than their extent, which latter must be judged of by the experimental results which I now proceed to describe.

The Experiments.

My first object in making these experiments was to ascertain if, and how, oiling the surfaces in contact affected the resistance to rolling.

The apparatus employed consisted of a wooden slab or table-top supported on three set-screws for legs, so that it could be tipped in any required direction. On this table rested one of Whitworth's surface-plates. On the surface-plate was placed a surveyor's level, which read divisions to the thousandth of a foot on a staff erected at 50-feet distance; also a bell-glass covered another part of the surface-plate in the manner of the receiver of an air-pump, which could be filled with oil from an aperture in the top. This glass served either to protect the roller from dust or surround it with oil, and thus prevent any surface-tension the oil might exert from affecting the results.

The roller was of cast-iron, 6 inches in diameter and 2 inches thick, and weighed about 14 lbs. It was not cylindrical, for the edge was somewhat rounded. Originally the roller was turned up so that the edge was curved to a radius of 1 foot; but subsequent grinding somewhat modified this shape.

In the first instance the roller was turned up and polished in the ordinary manner; but some preliminary experiments showed that the surface thus formed was far from perfect, as indeed was apparent when it was examined with a magnifying-glass. The roller was therefore again turned, and ground very carefully with Turkey-stone for several days, until the surface appeared through the glass to be as perfect as the iron would allow; there were still some small pits, but these appeared to be in the iron itself.

The roller when thus finished was rolled on various surfaces. First of all it was tried on the cast-iron surface-plate already mentioned; but this surface, which had been formed by scraping, was altogether too rough. Thus when the roller was placed on the plate it immediately rolled into a hollow. Surfaces were then formed by grinding two plates together with powdered Turkey-stone. In this way the plates were made so true that the roller would remain in any position, and would roll either way with an inclination of 1 in 5000, or about 1 foot in a mile. It appeared impossible, however, to produce surfaces altogether free from inequalities, which may be seen from the results of the experiments.

The Effect of Oiling the Surface.

In the first experiments the surface on which the roller was to roll was brought into a level position, so that the roller when placed on it remained at rest. A line of sights, consisting of a mark on the glass and a pin-hole in a plate fixed at some distance, was then brought to bear on a mark on the top of the roller, so that the least motion could be detected, and the position of the roller could be recovered after it had been allowed to roll in one direction. The level was then adjusted to read zero on the staff, and the table tipped until the roller rolled off in one direction. The reading of the level was then noted, and the same operation repeated in the opposite direction, the roller having in the meantime been brought back into its former position. Sundry observations were then taken with different points of the plane and roller in contact. After a considerable number of observations had thus been taken oil was poured into the glass until the roller was covered, and then the observations were repeated. Table I. shows a series of such observations for a surface of plate-glass both with and without oil. In these particular experiments, however, the surface was simply oiled, it having been found by experience that the effect was the same as when the glass was filled with oil. It will be seen that in these experiments the advantage is slightly in favour of the oiled glass.

The results contained in the second part of this Table were obtained by starting the roller in one direction against the inclination of the plane with just sufficient velocity to carry it up to a certain point, the inclination of the plane being adjusted until it would roll back. In this way the advantage is against the oil. This, however, I think is due to the surface-tension or fluid-friction arising from the motion of the roller.

TABLE I.

Cast-iron Roller on Plate-glass. (The distance of the Staff from the Object-glass of the Level = 50 feet. The Divisions on the Scale = $\frac{1}{100}$ foot.)

	Clean. Readings. To.	Clean. Readings. From.	Clean. Difference.	Oiled. Readings. To.	Oiled. Readings. From.	Oiled. Difference.
Starts from rest.	−5·0	1·2	6·2	−5·0	3·2	8·2
	−2·3	3·5	5·8	−3·3	2·5	5·8
	−2·6	2·0	4·6	−4·0	2·0	6·0
	−4·5	1·4	5·9	−4·1	1·0	5·1
	−4·7	2·0	6·7	−1·8	4·0	5·8
	−2·8	3·5	6·3	−3·0	2·0	5·0
	−3·2	4·5	7·7	−5·8	0·5	6·3
	−4·0	3·4	7·4	−5·2	0·3	5·5
	Mean...............		6·3	Mean...............		6·0
Rolls back when set in motion.	−2·6	−0·7	1·9	−3·0	−0·4	2·6
	−3·5	−1·5	2·0	−1·8	+1·0	2·8
	−3·5	−2·0	1·5	−2·5	0·0	2·5
	−4·2	−2·2	2·0	−2·5	+0·2	2·7
	−1·5	+0·6	2·1	−3·9	−1·0	2·9
	−2·5	−0·5	2·0	−3·0	−0·8	2·2
	−1·9	0·0	1·9	−4·4	−2·0	2·4
	−0·7	+1·5	2·1	−1·0	+1·5	2·5
	Mean..............		1·9	Mean..............		2·6

There is a very marked difference between these inclinations and those required to start the roller from rest, a difference which appears to exist with all the materials tried, and which I think is only in part explained by the roughness of the surface.

In these experiments with a surface of glass, the friction was so small that the inequalities of the surface rendered the results very irregular and uncertain. To obviate this a surface of box-wood cut across the grain was next tried. This, being softer, allowed the roller to indent it more than the glass and gave rise to greater friction, and hence the inequalities in the surface are less apparent in the results, which are shown in Table II. These observations were made in the same way as those with the glass, except that *blacklead* was

TABLE II.

Cast-iron Roller on Box-wood.

	Clean. Readings. To.	Clean. Readings. From.	Clean. Difference.	Blackleaded. Readings. To.	Blackleaded. Readings. From.	Blackleaded. Difference.
Starts from rest.	− 3·0	+ 5·0	8·0	− 4·8	+ 6·0	10·8
	− 3·0	+ 8·0	11·0	− 3·2	+ 7·8	11·0
	− 4·0	+ 5·8	9·8	− 7·6	+ 2·4	10·0
	− 4·0	+ 6·0	10·0	− 0·5	+ 8·0	8·5
	− 10·0	0·0	10·0	+ 0·8	+ 10·0	9·2
	+ 3·4	+ 12·8	9·4	+ 1·0	+ 8·9	7·9
	− 4·0	+ 7·0	11·0	− 9·0	− 1·2	7·8
	− 3·2	+ 8·0	11·2	− 9·8	− 1·0	8·8
	Mean		10·05	Mean		9·25
Rolls back when set in motion.	+ 7·0	+ 12·2	5·2	− 1·0	+ 1·0	3·5
	− 5·0	+ 0·4	5·4	− 1·2	+ 1·2	4·0
	− 2·0	+ 4·2	6·2	0·0	+ 2·0	2·0
	− 2·1	+ 3·8	5·9	+ 2·0	+ 5·0	3·0
	+ 3·2	+ 9·0	5·8	+ 1·2	+ 4·0	2·8
	+ 1·3	+ 7·0	5·7	+ 3·0	+ 6·2	3·2
	− 2·0	+ 4·0	6·0	− 6·4	− 3·0	3·6
	− 2·6	+ 2·9	5·5	− 7·6	− 3·0	4·6
	Mean		5·71	Mean		3·34

substituted for oil. The effect of the blacklead seems to have been slightly to diminish friction, not only when starting from rest, but when rolling back, which confirms me in the opinion that the contrary result with oil was due to its obstructive action.

India-rubber was then tried. A plate of this substance, three-eighths of an inch thick, was glued to a piece of wood to prevent it working forward. The results are shown in Table III. The friction was very much greater than in the previous experiments, and the advantage lies with the clean surface.

These results leave no doubt that rolling-friction does not depend greatly on the coefficient of sliding-friction between the roller and the surface. They are, however, completely in accordance with the explanation previously

TABLE III.

Cast-iron Roller on India-rubber.

	Clean.			Blackleaded.		
	Readings.		Difference.	Readings.		Difference.
	To.	From.		To.	From.	
Starts from rest.	−22·0	+14·0	36	−24	+18	42
	−28·0	+15	43	−19	+15	34
	−12·0	+18	30	−18	+19	37
	−19·0	+15	34	−23	+17	40
	−16·0	+16	32	−22	+17	39
	−18·0	+15	33	−23	+14	37
	−25·0	+12	37	−25	+17	42
	−23·0	+15	38	−24	+15	39
	Mean...............		35·4	Mean..............		38·75
Rolls back when set in motion.	−2	+28	30	0	+26	26
	−5	+26	31	−2	+22	24
	−6	+27	33	−1	+25	26
	−4	+28	32	−2	+22	24
	−3	+30	33	−10	+22	32
	−4	+30	34	−14	+19	33
	−6	+24	30	−6	+24	30
	−7	+25	32	−6	+23	29
	Mean......		31·9	Mean................		28

given of the manner in which sliding-friction acts to prevent the deformation of the surfaces at the point of contact.

The Tendency to Oscillate.

Another circumstance which was observed while making these experiments also offers strong evidence of this deformation, namely the tendency which the roller has to oscillate. This was always exhibited whenever the roller was slightly disturbed from rest on the level plane, and it was certainly not due to the fact of its having settled into a hollow; for when on india-rubber it would make several considerable oscillations in *whatever* position it was placed. By blackleading the surface this tendency was considerably reduced, although not altogether destroyed. These oscillations could not have been caused by the mere resistance which the one surface offered to the sliding of the other over it, unless also this resistance threw the surfaces into constraint from which they are constantly endeavouring to free themselves.

The Effect of the Softness of the Materials.

Having found that oil did not reduce the resistance, the experiments were continued with a view to ascertain how far the softness of the material had anything to do with it. As materials of several degrees of softness had already been tried, the only question was to settle how far the difference in the results

TABLE IV.

Cast-iron Roller on Brass.

	Clean.			Oiled.		
	Readings.		Difference.	Readings.		Difference.
	To.	From.		To.	From.	
Starts from rest.	−13·2	−5·5	7·7	−2·0	+3·8	5·8
	− 5·5	+2·0	7·5	−4·5	+1·2	5·7
	− 3·2	+5·0	8·2	−2·8	+3·8	6·6
	− 3·5	+4·5	8·0	−2·9	+5·2	8·1
	− 3·5	+3·8	7·3	−1·7	+5·8	7·5
	− 7·0	+1·5	8·5	−5·0	+1·0	6·0
	− 5·0	+2·2	7·2	−3·0	+3·5	6·5
	− 4·6	+3·0	7·6	−3·0	+2·9	5·9
	Mean		7·75	Mean		6·5
Rolls back when set in motion.	−2·4	−0·8	1·6	−1·5	+1·0	2·5
	−2·4	−0·4	2·0	0·0	+1·8	1·8
	−1·8	+0·7	2·5	−1·2	+1·6	2·8
	−2·0	−0·4	1·6	−2·0	+0·6	2·6
	−2·8	−1·0	1·8	−2·3	+0·5	2·8
	−2·5	+0·2	2·7	−2·0	+1·0	3·0
	−0·2	+2·0	2·2	−1·2	+1·5	2·7
	−2·2	0·0	2·2	−0·9	+1·6	2·5
	Mean		2·07	Mean		2·58

observed was due to their softness and how far it might be due to some other difference in their nature. To show this cast-iron and brass were tried, which are of much the same hardness as glass, and yet of an altogether different nature in other respects, the surface of the glass being highly polished, while that of the metal was dull as it had been left by the grinding. The results of these experiments are contained in Tables IV. and V.

Table V.

Cast-iron Roller on Cast-iron.

	Clean.			Oiled.		
	Readings.		Difference.	Readings.		Difference.
	To.	From.		To.	From.	
Starts from rest.	−6·5	+0·3	6·8	−1·3	+4·0	5·3
	−2·8	+2·4	5·2	−2·8	+2·5	5·3
	−2·6	+3·5	6·1	−3·5	+2·5	6·0
	−2·5	+2·3	4·8	−2·5	+3·8	6·3
	−0·6	+4·5	5·1	−2·2	+3·2	5·4
	−0·9	+3·9	4·8	−2·3	+3·0	5·3
	−3·0	+2·5	5·5	−5·0	+0·8	5·8
	−2·8	+4.2	7·0	+1·0	+6·5	5·5
	Mean..............		5·66	Mean..............		5·61
Rolls back when set in motion.	+4·0	+6·9	2·5	0·0	+2·3	2·3
	−0·7	+1·6	2·3	−1·8	+0·8	2·6
	−3·5	−0·8	2·7	−1·0	+1·3	2·3
	−3·8	−1·0	2·8	+0·2	+2·3	2·1
	−0·5	+1·8	2·3	0·0	+2·2	2·2
	−2·0	+0·1	2·1	−0·6	+2·0	2·6
	−0·8	+2·2	3·0	−0·6	+1·8	2·4
	−1·3	+1·6	2·9	+0·5	+2·9	3·4
	Mean..............		2·57	Mean..............		2·36

The means of the results for all the materials are contained in Table VI. Comparing these we see at once the effect of softness: the cast-iron, brass, and glass are very nearly the same, and the slight difference is not greater than may be accounted for by a slight difference in the smoothness of the surfaces. Of the three, according to hardness cast-iron should have given the least results; and so it does, as far as starting from rest is concerned, although when rolling back the result is the other way. Box-wood appears to offer about double the resistance of cast-iron; and india-rubber about ten times as much in the case of rolling back, and six times as much in starting from rest.

TABLE VI.

Showing the Mean of the Results for the various conditions of the Surface and manner of Starting.

The nature of the Surface.	Starts from rest.		Started in the opposite direction.		Mean.
	Clean.	Oiled or blackleaded.	Clean.	Oiled or blackleaded.	
Cast-iron	5·66	5·61	2·57	2·36	4·05
Glass	6·32	5·96	1·93	2·57	4·19
Brass	7·75	6·53	2·07	2·587	4·73
Box-wood	10·05	9·25	5·71	2·34	7·09
India-rubber	35·37	38·75	31·87	28·00	33·24

Experiments on Actual Slipping.

My object in the second series of experiments was to find by actual measurement how far the roller rolled short of its geometrical distance. Since the exceedingly small slipping on a hard surface precluded all chance of measuring it, these experiments were made on strips of india-rubber glued to wood: these were in general long enough to allow of two complete revolutions of the roller. The strips were of different thicknesses. This difference of thickness has an effect to vary the degree of indentation and the intensity of the pressure, as well as the lateral extension. On the thick india-rubber the indentation was considerable; and, owing to the large bearing-surface thus obtained, the intensity of the pressure beneath the roller must have been comparatively small, as must also the lateral extension; whereas with the thin strips the indentation was small, but the pressure and consequent lateral extension must have been correspondingly great. These considerations serve to explain the differences in the results of the experiments, which are given in Tables VII. and VIII.

TABLE VII.

Showing the Actual Slipping of a Cast-iron Roller.

The nature of the Surface.	The distance travelled.		The amount of the slipping.
	In one revolution.	In two revolutions.	
A steel bar (polished)	17·82	35·64	·00
India-rubber, 0·015 inch thick, glued to wood...	......	35·2	·44
„ 0·08 inch thick	......	34·8	·84
„ 0·36 inch thick	......	35·15	·49

TABLE VIII.

Showing the Actual Slipping with an India-rubber Tire 0·75 inch thick glued on to the Roller.

The nature of the Surface.	Distance travelled in one revolution.	Circumference of the ring.	The amount of the slipping.
A steel bar	22·55	22·5	−0·05
India-rubber 0·156 inch thick (clean)	22·55	"	−0·05
" " (blackleaded)	22·55	"	−0·05
" 0·08 inch thick (clean)	22·5	"	0·0
" " (blackleaded)	22·52	"	−0·02
" 0·36 inch thick (clean)	22·39	"	+0·11
" " (blackleaded)	22·42	"	+0·08
" 0·75 inch thick (clean)	22·4	"	+0·1
" " (blackleaded)	22·4	"	+0·1

These experiments show that a hard roller on a soft surface rolls short of its geometrical distance, whereas a soft roller on a hard plane rolls more than its geometrical distance, but to a smaller degree, and that when the roller and the plane are of equal hardness the roller rolls through less than its geometrical distance, which results are in exact accordance with what has previously been explained.

The Effect of Heat and Viscosity to cause Friction.

While making the experiments which have been described, two other causes of resistance to rolling besides friction suggested themselves to me, and were to a certain extent verified. The first of these is the transference of heat which takes place within both the plate and the roller in the neighbourhood of the point of contact. As the roller moves forward it is continually compressing the material in front of the point of greatest pressure, and this material expands again so soon as the roller is past. During compression there will be a change in the temperature of the material compressed, which change will be readjusted again as the material expands, supposing that in the interval between compression and expansion there has been no heat communicated to or taken from the portion of material affected. But since the change of temperature caused by compression will place the part compressed out of accord with that immediately surrounding it, a transference of heat will necessarily take place. The quantity of heat thus transferred will depend on the length of the interval, *i.e.* the speed of the roller, and on the conducting-power of the material.

This transference will cause resistance to the roller, for the material will not expand to the same temperature, and hence to the same volume, as that from which it was compressed, and hence it will take more work to compress it than it will give out in expanding.

It does not, however, follow that the greater the transference of heat the greater the resistance; for if a sufficient time be allowed the transference of heat will readjust the temperature as fast as expansion takes place. There is some speed, therefore, for which the resistance arising from this cause will be a maximum. If, therefore, the material be a good conductor and the motion slow, the transference of heat will prevent any variation of temperature during either compression or expansion. When such was the case the resistance would increase with the speed, a fact which was very evident when the rolling took place on india-rubber; for it was possible to give the plane such an inclination that the motion of the roller was scarcely perceptible, and any increase in the inclination was followed by a corresponding increase in the speed of the roller.

As already stated, there is another cause of resistance; and this may partly explain the result: this is viscosity.

If we stretch a piece of india-rubber, or any material, when released it does not *immediately* come back to its original length, but at once comes back a certain distance and then recovers the rest more or less slowly. Hence as the roller moves forward the compressed material will require time for its complete expansion, and hence will offer less resistance to the roller when the motion is slow than when it is rapid.

Conclusion.

The foregoing remarks must be regarded as relating only to the *nature* of rolling-friction. I have not attempted to ascertain the laws which connect its magnitude with the various circumstances which affect it. As far as they go I can see no reason to doubt the two laws propounded by Coulomb, viz. that for the same material the resistance to rolling is proportional to the weight of the roller, and inversely proportional to its diameter. In addition to these laws, however, it appears clear to me that there must be another law connecting rolling-friction in some way with the softness of the tires of the wheels and the road. In addition to the instance of india-rubber tires already mentioned, there are several other phenomena connected with wheels which point to such a law, and can be explained by the recognition of the slipping under the roller.

Steel and Iron Rails.

The very great advantage in point of durability of steel rails over iron has been a matter of much surprise, it not being sufficiently accounted for by the greater hardness of the steel, supposing it to be subjected to the same wearing action as the iron. This is at once explained, however, by the recognition of the fact that hardness tends to reduce the slipping and hence the wearing action, as well as to enable the rail the better to withstand the wear to which it is subjected.

That rails should wear at all in places where they are straight and where brakes are not applied is a matter which calls for an explanation, and this, so far as I am aware, has not hitherto been given; mere crushing, however much it might deform the rail, would not cause such a reduction of weight as actually takes place. The explanation of this phenomenon also at once follows the recognition of the slipping which attends rolling.

A little consideration also serves to show that the scaling of wrought-iron rails is the result of the repeated lateral extension of the surface in the rail under the action of the wheel. The systematic way in which this takes place shows that it is due to something more than the mere imperfection in the iron. There is no doubt that the grain of the iron has a great deal to do with it; but considering the multitudinous ways in which iron is used and that this is the only one in which scaling takes place, it is clear that it must be due to some cause directly connected with the action to which the rail is subjected. Now every time a wheel passes over a point in a rail it tends to slide the upper strata of the rail over those beneath them, and thus causes tangential stress. If the rail were homogeneous this would hardly cause it to scale; but owing to the grain in the iron some strata are stronger than others, and the weaker strata are called upon to do more than their share of the yielding, and so become still weaker and eventually give way.

There are other phenomena which, having been hitherto unnoticed or unexplained, might be shown to arise from the slipping which takes place during rolling; but perhaps those I have mentioned are sufficient to show that the effects of the action are not altogether without practical importance.

19.

ON THE STEERING OF SCREW-STEAMERS.

From the "Report of the British Association," 1875.

THERE does not appear, as far as my observation goes, to be any particular difficulty in steering screw-steamers so long as they are going ahead under steam, but rather the other way; they then seem to be better to steer than almost any other class of ships. Great difficulty often occurs, however, when they are stopping, starting, or otherwise manœuvring. Their vagaries are then so numerous as to give the idea that there is a certain degree of capriciousness and uncertainty about their behaviour. This is, of course, mere fancy; and did we but know them, it is certain that there are laws which these steamers follow under all circumstances. In the hope of arriving at these laws, I have been investigating this subject now for twelve years as opportunity offered; and I had come, as I thought, to some leading facts, when the failure of the 'Bessemer' to enter Calais Harbour on the 8th of May last seemed to establish them.

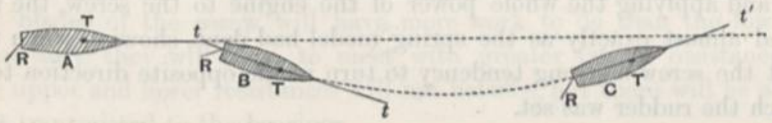
It will be remembered that the ship entered between the piers at a speed of 12 or 13 knots, the tide running strong right across the mouth of the harbour, that on her entering between the piers the engines were reversed, and that the ship turned, under the influence of the current, in spite of her rudder; so that Capt. Pittoch, in his letter to the *Times*, attributed the accident entirely to her failing to steer at the time.

On reading of the accident I thought it would be a good opportunity to call attention to the subject of steering steamers; and I wrote a paper, which was published in the *Engineer* of June 4th, 1875, in which I explained why the act of stopping a ship must necessarily affect her power of steering—pointing out that when a ship is stopping the water will be following her stern relatively faster than when she is moving uniformly, and consequently that the effect of the rudder will be diminished; that the longer the ship the greater will be the difference; also that this effect is greatly increased when a ship is stopping herself with her propellers, as was the 'Bessemer'; for then not only is the retardation of the vessel much more rapid, but the water has a forward motion imparted to it by the propellers, which motion, if the propellers are near the rudder, may be greater than that of the ship, under which circumstance the effect of the rudder's action will be reversed. Since publishing this paper in the *Engineer* I have carried the investigation further; and the object of the present paper is to give an account of some experiments on model boats driven by screws, and the conclusions to which these experiments have led me.

Two models were used in making these experiments; the one 2′ 6″ long, driven by a spring, and the other 5′ 6″, driven by steam. In both models the rudders were broad in proportion to the boats. In the clockwork model the rudder was almost close to the screw, there being no stern-post. In the steam model there was a wide stern-post, and the rudder was an inch and a half behind the screw.

Both boats went straight with their screws driving them ahead and with their rudders straight, and they both answered their rudders easily with their screws going, turning in circles of from four to six feet radius. When the screws were stopped and the boats carried on by their own way, they both answered their rudders, but much more slowly than when their screws were going, the smallest circle being now, as near as I could estimate, from twelve to fifteen feet radius.

In order to try the effect of the screw, when reversed, on the steering of the spring-model, the model was towed by a cord attached (as shown in the



accompanying figure) to a point T amidship about one-third of her length from her bow, so that the towing had little or no tendency either to keep her straight or turn her. The rudder was then set at an angle of 45° or thereabouts, so as to turn her head to the right, towing was commenced, the boat turning in a circle to the right. The screw was then started in the reverse direction; whereupon the boat ceased to turn to the right, and commenced turning to the left to an extent depending on the slowness with which she was being towed. When towed very quickly, at from two to three miles an hour, she came nearly straight forward, but at the fastest speed showed no tendency to turn to the right.

The rudder was then set so as to turn the boat to the left, and the operation was repeated with very nearly corresponding results so long as the screw did not race; but the action of the reversed screw on the rudder when set to the left was not so great as when set to the right. This difference led me to suppose that the screw itself might exert an influence to turn the boat to the left when it was reversed, although it had been found to exert no such influence when going ahead. This was at once shown to be the case by setting the rudder straight and starting the screw reversed; the boat immediately turned to the left, but not fast unless the screw raced, then she turned very rapidly.

These direct effects of the screw to turn the ship appear to me to account for several of the anomalies which have hitherto beset the subject; and further on in the paper I shall discuss them at length.

The steam model was provided with paddles as well as screw, and the screw could be reversed without reversing the paddles, in which case the paddles overpowered the screw, and the boat moved forward somewhat slowly. In this boat the screw was so deeply immersed that it would not race, and it had no direct effect to turn the boat when reversed like that of the spring model.

When the screw was reversed and the boat drawn slowly forward by the paddles, the effect on the rudder was almost to destroy its action, it having only a slight power to turn the boat in the opposite direction to that in which it would have turned the boat had the screw been going ahead. Practically the boat had lost all power of steering. Coupled in this way with the paddles the screw turned but slowly, the engine being held up by the opposing actions. On releasing the paddles and allowing them to turn freely, and applying the whole power of the engine to the screw, the model behaved almost exactly as the spring model had done, showing when towed against the screw a strong tendency to turn in the opposite direction to that in which the rudder was set.

The screw was then set full speed ahead; and when the boat had acquired way the rudder was set, so that she began to turn rapidly to the right; the screw was then reversed, and by the time the boat had lost all forward way she had turned to the left through an angle of 30°, so great was the effect of the screw on the rudder when stopping the boat.

This completed the list of the experiments, which, however, were repeated over and over again with exactly the same results.

Conclusions to be drawn from the experiments. The general conclusion is that in screw-steamers the effect of the rudder depends on the direction of motion of the screw rather than on the direction of motion of the boat. Or we have the three following laws:—

1. That when the screw is going ahead the steamer will turn as if she were going ahead, whether she have stern-way on or not.

2. That when the screw is reversed the rudder will act as if the vessel were going astern although she may be moving ahead.

3. That the more rapidly the boat is moving in the opposite direction to that in which the screw is acting to drive it, the more nearly will the two effects on the rudder neutralize each other, and the less powerful will be its action. It would appear reasonable to suppose that a boat may move fast enough to overcome the effect of the reversal of the screw; but this was not the case with the models.

The effect of the screw to turn the boat independently of the rudder. It seems to be supposed by some that a screw necessarily tends to force the stern of the boat in a direction opposite to that in which the tips of its lower blades are moving. This is undoubtedly the case when the screw is racing or acting in broken water (*i.e.* water mixed with air), also when the screw is not completely covered with water. When, however, the screw is properly immersed and is working in unbroken or continuous water, and is not affected by dead water, it has not the least tendency to move itself laterally, whatever it may have on the ship. Under these circumstances the screw-shaft can exert no lateral pressure on its bearings; and in ships with fine runs this is the case.

Owing to the effect of the dead water, however, it may happen that even when the screw is properly immersed it will tend to move laterally. If the water be following the ship faster above than below (which it often is), the upper blades of the screw will have more work to do than the lower, and consequently they will have to meet with greater lateral resistance; and hence upper and lower resistances will not balance, but there will be a lateral thrust transmitted to the bearings.

Besides the lateral pressure which may be transmitted through the bearings, the screw may also tend to turn the ship by the lateral motion which it imparts to the water, which is again communicated to the ship or the rudder. If the form of the ship and the rudder were symmetrical above and below the screw-shaft, then the effect of the lateral motion which the screw imparts to the water below would exactly balance the effect above the screw-shaft; but owing to the fact that the surface both of the ship and the rudder is in general much greater above than below, the water which is driven laterally by the upper blades has much more surface to act upon than that which is driven in the contrary direction by the lower blades, and therefore drives the stern of the ship laterally, or tends to turn the ship. This effect is in the opposite direction to that which arises from the unequal rate at which the water is following the ship, as long as both the ship and the screw are going ahead; and consequently these two effects tend to counteract each other. When, however, the screw is reversed, and the vessel is still moving forwards, the two effects are in conjunction; and consequently they are more likely to become apparent and important. This was the case in the experiments with the spring model. When screwing ahead she went straight enough, but when towed ahead with the screw reversed she turned to the left. In this case the effect was small; and I imagine that it must always be so, particularly when the ship has a fine run. In the steam model, of which the run is very fine, the screw-way very large, and the screw small (being only three inches while the boat draws five), the effect of the screw to turn the boat when not racing was altogether imperceptible. I conclude, therefore, that these effects may be left out of consideration with reference to steering; and in opposition to a popular notion I derive law 4.

4. That when not breaking the surface the screw has no considerable tendency to turn the ship so long as the rudder is straight.

The effect of racing. Although the direct effect of the screw is insignificant when it is not racing or breaking the surface, this is not the case when it is racing. It then exerts a very decided and important effect; and it is doubtless experience of this which has given rise to the popular notion above referred to.

In the experiments with the spring model when the screw was drawing air down, the stern always showed a tendency to move in the opposite direction to that in which the tips of the lower blades were moving, even when the boat was going ahead at full speed and the quantity of air very small; and when the screw regularly raced, frothing the water, its effect to turn the stern of the boat was very great.

The screw of the steam model was so deeply immersed that it would not race; but if the stern of the boat was raised by a string it then raced, and the effect of the screw to turn the stern of the boat was the same as with the spring model.

The screw of the spring model showed a much greater tendency to draw air when reversed (the boat being towed) than when it was driving the boat ahead; but its greatest tendency to race was when the boat was stationary, or nearly so. This latter tendency I have observed in large steamers; in fact I have never seen a large steamer start or reverse her screw when moving but slowly without frothing the water. It appears, therefore, that the effect of racing on the steering may be stated in the following laws:—

5. That when the screw is frothing the water, or only partially immersed, it will have a tendency to turn the stern in the opposite direction to that in which the tips of the lower blades are moving.

6. That when the boat is going ahead its effect will be easily counteracted by the rudder; but when starting suddenly, either forward or backward, at first the effect of the screw will be greater than that of the rudder, and the ship will turn accordingly.

7. That if when the boat is going fast ahead the screw is reversed, at first it almost destroys the action of the rudder, what little effect it has being in the reverse direction to that in which it usually acts. If, then, the screw draws air or breaks the surface, it will exert a powerful influence to turn the ship.

In accounts of collisions it may be frequently noticed that there is contrary evidence given of the steering of one or both of the ships (if they both happen to be steamers). In the instance of the collision between the 'Ville du Havre' and the 'Loch Earn' the captain of the 'Loch Earn' stated that the steamer altered her course almost at the last moment, thus rendering the collision inevitable. The officers of the steamer asserted that such was not the case; they state, however, that the screw was reversed just before the collision. In this case, therefore, the evidence is to show that the reversal of the screw caused the steamer to change her course, either by its direct effect or by its action on the rudder. The latter effect would be sufficient to explain the facts; and my experiments leave no doubt but that this must have taken place. With regard to the former I have no evidence; although, considering that the ship was moving rapidly at the time, it seems probable that the screw may have raced on being reversed, and added its direct effect to turn the ship to its effect on her rudder. In this case, therefore, the reports of what took place are strictly in accordance with what was to be expected from my experiments; and I think that from the light these throw upon the subject in many cases, the accounts may be less contradictory than they have hitherto appeared; and I am in hopes that in the future these experiments may assist not only in the discovery of the causes of accidents, but, as these become recognized, in the prevention of the accidents themselves.

As an illustration of how important a clear conception of the whole circumstances of the effect of the screw on the rudder may be, I will read an account with which I have been kindly furnished by Mr Henry Deacon; from which account it appears that a ship was saved by a combination of accidents, which led to her being handled in the very manner in which she would have been had the conduct of the officer in charge been governed by the laws laid down in this paper.

Mr Deacon says:—

"I have been reading your communication to the *Engineer* of the 4th inst. about the 'Bessemer's' steering, and think the following narrative may have some interest for you. A friend of mine came from Philadelphia, U.S., early in May to Liverpool in the S.S. 'Ohio.' To avoid ice the vessel went out of her course 160 or 170 miles, and encountered very bad weather. The captain spent one or two days without taking off his clothes; and whilst lying down one day, leaving the chief officer in command of the deck, amongst fogs and rain, an iceberg was sighted right ahead and quite close when seen. The officer stopped and reversed the engines, and put the helm hard round. The cessation of motion awoke the captain, who rushed up the bridge. The excitement had spread, the officer's orders had been strictly obeyed. The captain took all in at a glance, put the engines on ahead at full speed, and the 'Ohio,' breaking through the thin ice always skirting the icebergs, passed so close to the solid mass, that my American friend, who is fond of horses and was on deck, says he could have struck the ice from the ship with a tandem whip. The captain afterwards explained the matter thus:—the steering-gear was the now usual parallel screws, *i.e.* exerting the least force when the rudder is most moved, but of course retaining the rudder in any position with little or no effort. To put the rudder hard round when the ship is under full way and the engines working is an almost physical impossibility; but to put it hard round when the engines are stopped, and especially to put it round when they are reversed, is comparatively easy. The chief officer's order, therefore, enabled the rudder to be put round to the utmost; he both stopped and reversed the engines. The captain's arrival and comprehension completed the manœuvre. The 'way' was but slightly interrupted, but the helm was put hard round and the ship turned from her course in the shortest possible distance.

"I have all this at second hand from my friend; but this fact of the easy movement of the helm, whilst the ship was under way with the engines reversed, appeared to be one well understood; and of course if no power be required to move the helm, no power can be exerted in steering the vessel; and the whole tale seems to me so illustrative of your remarks on the 'Bessemer,' that I venture to trouble you with it."

(For continuation see papers 27, 31, 34.)

20.

IMPROVEMENTS IN TURBINES AND CENTRIFUGAL PUMPS.

[*From the* "Specification of Patent No. 724." 1875.]

WHEN the available pressure of fluid for driving a turbine is very great, and the quantity of fluid is small, the diameter of the wheel has to be small and its speed inconveniently great, so that in the best class of turbines one hundred feet is the practical limit of the fall which can be utilized, and in raising or forcing fluids by means of centrifugal pumps, it is almost impossible, by reason of the great speed required in the ordinary centrifugal pump, to raise fluids to any considerable height. The object of my invention is to overcome the difficulties above referred to, and my invention consists in the construction, combination, and arrangement of apparatus which will utilize the largest pressures of fluids to the fullest extent in obtaining motive power, while keeping the speed of rotation within practicable limits, and to obtain the greatest pressures or lifts by centrifugal machinery while keeping down the speed of rotation. In order to obtain motive power from the fluid, it is caused to traverse a passage or passages (which for distinction may be called fixed passages) so formed that it is discharged from them with a rotary motion or velocity of "whirl" about a certain axis or shaft. In impressing this rotary motion on the fluid, part of its pressure is spent, so that it emerges from the passages at a lower pressure than that at which it entered them. It is then received into a passage or passages moving round the said axis or shaft. These passages are so formed that the fluid on leaving them has as far as practicable no velocity of "whirl," that is, no rotary motion about the shaft. This modification in the motion of the fluid is effected as far as possible without shock or friction, by so forming the moving vanes (as is well understood) that the fluid shall be forced from them in a direction opposite to that in which they are moving, and with a velocity relative to the moving vanes at the place it leaves them, equal to that with which they are moving. To give the fluid this relative velocity a further portion of its initial pressure has to be spent, so that the fluid will emerge from the moving passages with a pressure still lower than that at which it entered them, and the pressure which has thus forced the fluid back will have produced an equal effect to urge the moving passages forward, and thus give out the motive power. So far the apparatus above described is identical with some forms of what is known as the turbine, and in this respect no improvement is claimed. The novelty of my invention, however, consists in so arranging the size and motion of the passages, that on emerging from the moving passages the fluid shall not, as in the case of the ordinary turbine, have spent the whole or nearly the whole of its available pressure, but that it shall still have sufficient pressure to carry it through one or more additional sets of passages similar to those already described; that is to say, on emerging from the first moving passages, it shall again be received into other fixed passages, so that on being forced through them it shall emerge with a velocity of whirl or rotary motion round an axis—not necessarily the same as before—with a reduced pressure, and again be received into another similar set of moving passages from which it may emerge with no velocity of whirl. This may complete the entire cycle of operation to which the fluid is subjected, but this will depend upon circumstances (*videlicet,* the magnitude of the initial pressure and the character of the motive power required). It may, however, be desirable so to arrange the size and velocity of the passages, that the fluid shall have to pass through more than two sets of moving passages before all its available pressure is spent. In fact there is no limit to the number of such sets of passages that may be employed when desirable. On emerging from the last set of passages the fluid will be allowed to flow away into such receptacle, channel, or tail-race as may be provided. So far then my invention might be described shortly to consist in using two or more turbines in combination instead of one, the same fluid being made to pass through them successively, and spend a portion of its pressure in producing motive power in each. The pressure necessary to drive the later turbines being transmitted through the fluid in the earlier turbines, the passages in which have therefore to act the part of pipes to resist the pressure. In order that this pressure may be transmitted, it is essential that the fluid should entirely fill the passages along which it passes, so that those turbines in which the water only partially fills these passages would not be available, and could not be used in carrying out my invention. Since the stream of water passing through the several sets of passages, or the several turbines, is continuous, the size of the passages in the different turbines or sets must be carefully adjusted so as to prevent undue loss of pressure, as must also the velocity of the moving passages. For forcing or raising fluids, the apparatus employed is similar to that already described for obtaining motive power, taken in the inverse order, that is to say, the fluid is first taken up by passages carried by a rotating axis, in which passages it has a velocity of "whirl" impressed upon it, and in receiving this velocity of "whirl" it is driven against a certain amount of pressure, so that it will emerge from the moving vanes at a greater pressure than that at which it entered them; it then enters the fixed passages or directors, which are so formed and placed that its entrance may be without shock or friction, and in which it again loses its velocity of whirl and forces itself against further pressure. So far, the last apparatus is identical with certain so-called centrifugal pumps and fans, but in the case of obtaining motive power hereinbefore explained, the novelty of my invention consists in repeating the action, and again causing the fluid to traverse one or more additional sets of moving passages alternating with fixed passages. In this case, as in that previously described for obtaining motive power, it is essential that the size of all the passages should be properly adjusted, as well as the velocity of the moving passages. In both cases, that is, in obtaining motive power and raising and forcing fluids, instead of alternate sets of fixed and moving passages, all the passages may be in motion, but in that case the alternate sets of passages must move in opposite directions. It is not necessary that the several sets of moving passages should be connected with or move round the same axis or shaft, but such an arrangement considerably simplifies the apparatus. My invention applies to all fluids, liquids, vapours, and gases; and one important application is that of producing blasts of air at considerable pressure. In obtaining motive power by my improvements from fluids, or in forcing fluids such as gases, where the density of the fluid varies with the pressure, the passages must be arranged to increase in capacity as the pressure diminishes, when obtaining motive power, and on the contrary the passages must diminish in capacity as the pressure increases, when forcing gaseous fluids. My invention further relates to a mode of constructing apparatus for obtaining motive power from fluids, and is applicable to turbines where the whole of the power of the fluid is imparted to the turbine by once passing through one set of moving passages, as well as in carrying out my improvements for obtaining motive power hereinbefore described, and my invention consists in attaching flat or curved vanes to radiate from a rotating shaft after the manner of the vanes of a common blowing fan. The vanes are surrounded by a fixed casing having its ends perpendicular to the shaft, one or both of which may have openings round the centre; the space between the two ends is rather greater than the width of the vanes to admit of their free rotation. The circumference of the case is formed by a plate or plates perpendicular to the two ends, and in the form of a spiral or spirals having an opening or openings into the case, which openings form the fixed or directing passages for the fluid to enter, the fluid escaping from the casing through the holes or openings in the ends of the casing. The spiral plates may be made moveable, so that the size of the passages may be adjusted, and the fluid may be introduced between them in any convenient manner. The accompanying sheet of drawings is intended to explain an application of my invention. Fig. 1 is a diagram showing two sets of fixed and two sets of moving passages, or two

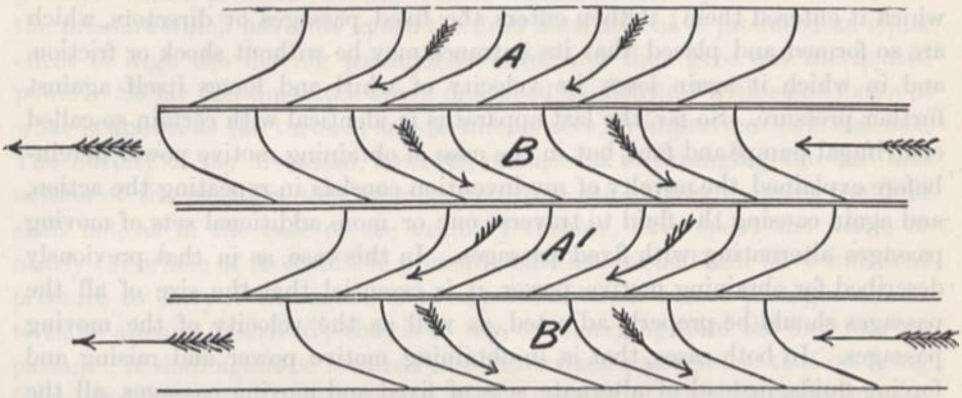


Fig. 1.

sets of passages moving in one direction and two sets in another. The passages are for simplicity shown as arranged in straight lines, but really they have to be placed in circles, round an axis, either side by side as in what is known as the parallel flow turbine, or one set within the other as in radial flow turbines. *A* is a set of fixed and *B* a set of moving passages; the fluid enters through *A* in the direction of the arrows, and passing through *B* enters a second set of fixed passages *A′*, on emerging from which it enters a second set of moving passages *B′*, or *A* and *A′* sets moving to the right, and *B* and *B′*

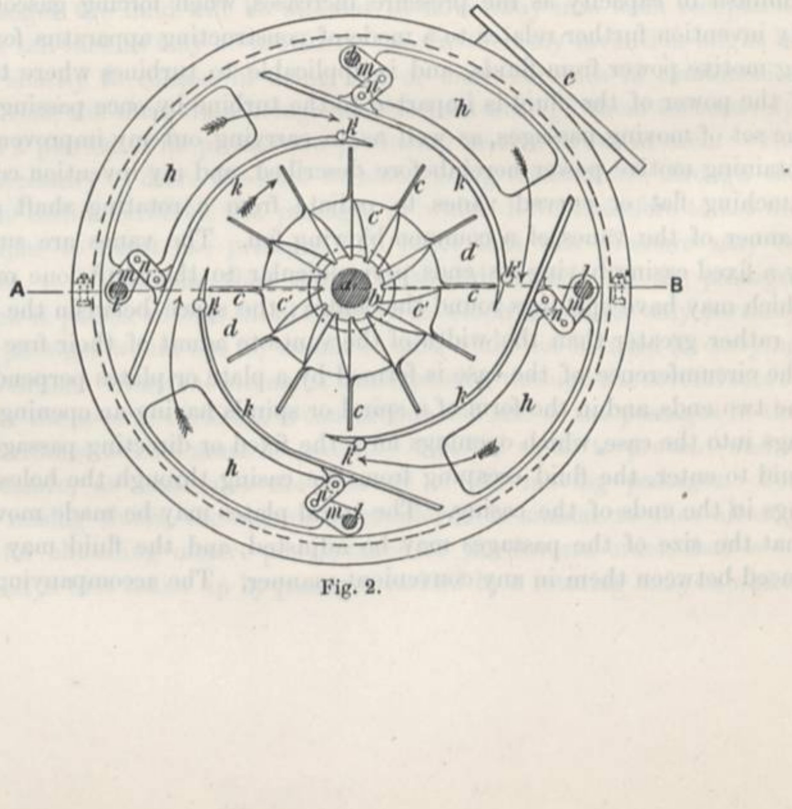


Fig. 2.

sets moving to the left. For centrifugal pumps and fans, the direction of the fluid and of the moving passages is reversed, the fluid entering the moving set B' will be discharged into the fixed set A', and then pass to the moving set B to be discharged into the fixed set A. Fig. 2 is the end view of a turbine constructed according to my invention, which may be used where the fluid is to impart all its power by one passage through the turbine. In the view shown by Fig. 2 the end part of the casing is supposed to be removed to show the inner parts, and that side of the turbine is represented by Fig. 2 at which the fluid leaves the vanes after having acted upon them; a is the shaft, b the boss keyed upon it, to which the vanes c are secured. These vanes are straight, but a part c' of each vane near the axis is of the form shown and is turned round until it is at an angle of 70° or thereabouts with the face of the vane and axis of the shaft a. The vanes c revolve between the end casings d, there being a slight clearance between the edges of the vanes and the surface of the end casings. The motive fluid may first enter by a pipe e into a chamber or space f formed at one end, at which end there may be a stuffing box g round that end of the shaft (see Fig. 3), as the fluid at that end

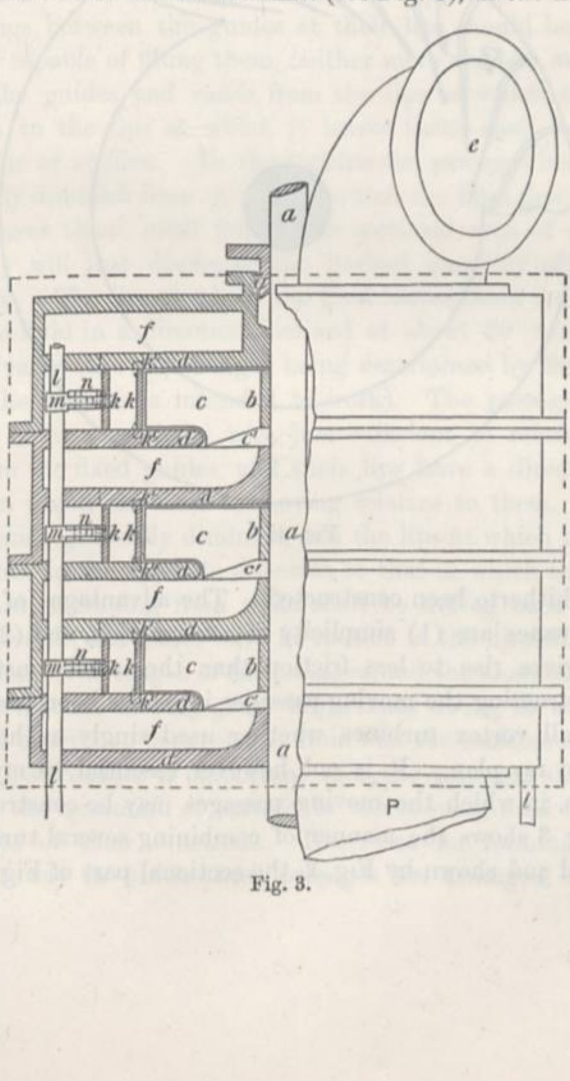


Fig. 3.

is under pressure. The fluid then passes through holes *h* in the end plate or casing, and is directed by guides (fixed or adjustable) upon the vanes *c* carried by the shaft *a*, and then leaves the vanes by passing from them through a hole in the centre part of the end casing. The guides *k*, if adjustable, have pivots *k'* upon which they turn, which pivots have bearings in holes in the end casings, and the guides are adjusted by shafts *l* having arms which have links *n* jointed to them, the links being connected by their other ends to projections on the guides. This turbine is of a class known as Thompson's vortex turbines, and with the exception of the moving vanes is similar in construction to these turbines. The vanes, however, are shown as constructed on my improved plan, being only connected with each other by the boss, which connects them with the axis, and not connected with two parallel discs as shown in Fig. 4, which is the manner in which the moving passages of such

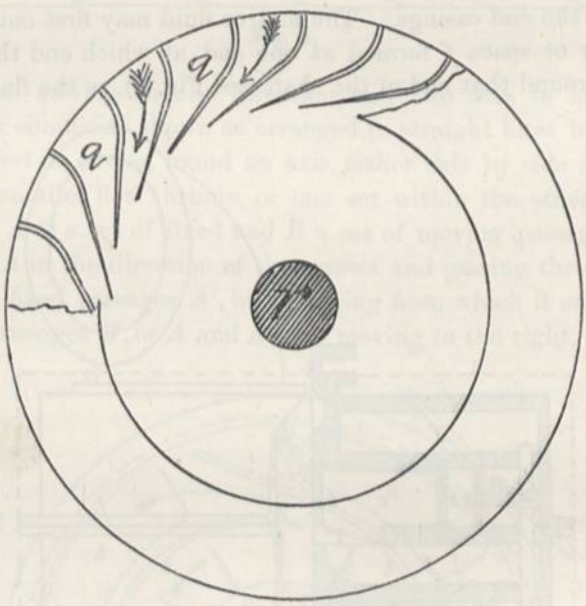


Fig. 4.

turbines have hitherto been constructed. The advantages of my method of arranging the vanes are (1) simplicity of construction, and (2) that such an arrangement gives rise to less friction than the usual construction. This method of constructing the moving passages is (as has been previously stated) applicable to all vortex turbines whether used singly as heretofore, or in combination on my plan. It is not, however, essential to my plan of combining turbines, in which the moving passages may be constructed as shown in Fig. 4. Fig. 3 shows the manner of combining several turbines like that above described and shown by Fig. 2, the sectional part of Fig. 3 being taken on the line AB (Fig. 2). Three turbines are shown combined in Fig. 3 upon the same shaft a, the casings of the turbines being secured to each other end to end by flanges and bolts. The casings may be constructed in two halves bolted together as shown in dotted line, or in any other suitable manner. The fluid first enters at the pipe e, then passes into the chamber f, then from this chamber through the holes h to the vanes c, then through the central hole into the next chamber f of the next turbine, and so on, leaving finally by the pipe p in communication with a chamber f into which the third turbine discharges, l are shafts which come to the exterior of the casings and can be turned to adjust the directors k. In turbines and pumps combined on my plan, (as when used singly) the guides and vanes which form the passages may have various forms, which will depend to some extent on whether the direction of flow is parallel to the axis or radial. It is essential, however, that the lips of the guides or vanes at which the fluid enters between them should be inclined in the direction of the motion of the fluid relative to them, so that it may glide in without having the direction of its motion suddenly altered. It is also essential that the openings between the vanes and the openings between the guides at their lips should be such that the fluid is exactly capable of filling them, neither more nor less, and also that the curvature of the guides and vanes from the lips at which the fluid enters between them to the lips at which it leaves them shall be gradual, and nowhere angular or sudden. In the turbine the passages between the fixed guides gradually diminish from the lips at which the fluid first enters to those at which it leaves them, until finally the sectional area of the passages is such that they will just discharge the desired quantity of fluid with the desired velocity. The lips at which the fluid leaves them must be so formed as to direct the fluid in a direction inclined at about 20° to the direction of motion of the vanes (the exact angle being determined by the circumstances under which the turbine is intended to work). The passages between the moving vanes have a sectional area just sufficient to receive the fluid as discharged from the fixed guides, and their lips have a direction parallel to the direction in which the fluid is moving relative to them. The openings between the vanes gradually diminish, and the lips at which the fluid leaves them are inclined in the opposite direction to that in which they are moving, so that the fluid on issuing from them shall be driven back as fast as the vanes move forwards, and hence have no motion in the direction in which the vanes are moving. The length of the passages, and the closeness of the guides and vanes, are matters of very great importance owing to the loss of work which is caused by friction between the fluid and the surfaces over which it is gliding, which loss renders it important that the passages should be formed so as to expose the minimum of surface to the moving fluid consistent with the performance of their functions. The rules for forming the passages depend on the way in which these passages are arranged, or the class to which the turbines or pumps belong, but in all cases they are the same as those for forming a single turbine or pump to work with the pressure and with the same quantity of fluid as the several individuals of the combined turbines. The best proportions for a vortex turbine are shown in the drawings, Figs. 2 and 3. The size of the turbine depends on the quantity of fluid which is to pass through it, and is given by the formula

$$A = \sqrt{\frac{W}{24p}} \cdot Q,$$

where A is the area of all the openings between the guides where the fluid emerges from them, W is the weight of one cubic foot of the fluid, p is the difference in the pressure of the fluid on entering and leaving the turbine in pounds (lbs.) per square foot, Q is the quantity of fluid in cubic feet per second, and the diameter of the wheel is given by $D = \sqrt{20A}$, where D is the diameter in feet. I have now particularly described the nature of my invention and the mode of carrying the same into effect, and claim as my invention—Firstly, the arrangement and combination of two or more turbines together so that the pressure necessary to work the second turbine may be transmitted through the fluid in the first, and the pressure or power of the fluid will in passing through the combined turbines in succession impart a portion of its entire pressure or power to each turbine substantially as hereinbefore described. Secondly, The arrangement and combination of two or more centrifugal pumps or fans in which the fluid after leaving the moving passages is received into fixed passages, or passages moving in the opposite direction, so formed as to deprive it of all velocity of whirl, or give it a velocity of whirl in the opposite direction as hereinbefore described. Thirdly, The construction, combination and arrangement of turbine apparatus as hereinbefore described and illustrated by Fig. 2 of the accompanying drawings. Fourthly, The combination and arrangement of two or more turbines similar to that hereinbefore described and illustrated by Fig. 2 of the drawings substantially as hereinbefore described and illustrated by Fig. 3 of the accompanying drawings. Fifthly, The combination and arrangement of two or more turbines as hereinbefore described and illustrated by Fig. 4 of the accompanying drawings.

21.

ON THE UNEQUAL ONWARD MOTION IN THE UPPER AND LOWER CURRENTS IN THE WAKE OF A SHIP; AND THE EFFECTS OF THIS UNEQUAL MOTION ON THE ACTION OF THE SCREW-PROPELLER.

[*From the* "Transactions of the Institution of Naval Architects," 1876.]

(*Read* April 7, 1876.)

THE very important part which the tendency of the water to follow in the wake of a ship plays in the action of the screw-propeller has often been the subject of remark. It has been very prominently brought forward by Mr Froude and others, and is, I believe, now very generally accredited a place in all considerations of the very complicated phenomena which envelop the action of the screw. There is one effect of this wake, however, which I think has not hitherto received the attention which its importance demands, and this is the subject of my present communication.

Of the various phenomena which have been developed during our experience of screws, none have given more trouble than their tendency to cause vibrations; and although certain causes have been suggested for this, it has never received a satisfactory explanation. This, I think, arises from the fact, that in the calculations and estimates which have been hitherto made respecting the screw, it has been uniformly assumed that the blades of the screw act with equal effect in all positions—that the screw acts equally on all the water through which it sweeps. If this assumption were correct there could be no tendency in the screw to cause vibrations except by throwing water against parts of the ship. But this equal action can only be assumed to exist on the supposition either that the water through which the screw moves is initially at rest, or that it is all moving with the same velocity. Now, this supposition will, I think, on closer examination, be seen to be very far from true; and in recognising what is the actual condition of the water, I think we can see what are the causes of general phenomena which have been hitherto only partially explained, besides the above mentioned tendency to cause vibrations which is the *péché habituel* of the screw-propeller.

Last year, while investigating the action of a screw on the steering of a vessel, my attention was drawn to the tendency which the screw has to turn the ship out of her direct course. This tendency I found was very generally recognised, and was attributed, like the tendency to cause vibrations, to the action of the water thrown by the screw obliquely against the stern-post.

That this explanation was not the true one, I was at once able to convince myself by removing the stern-post, when I found that the tendency of the screw to turn the ship out of her course was increased. I was thus led to conclude that the upper blade, or blades of the screw, experienced greater lateral resistance than the lower blade or blades; for the stern of the ship was always driven in a direction opposite to that in which the upper blades were moving. On looking for the cause of this resistance it appeared that it might arise from the water in which the upper blades worked following the ship faster than that below; and on comparing the various tendencies which the ship had to turn when moving at different velocities, with what might be expected to result from such an unequal motion, I found sufficient agreement to confirm me in this opinion. Being at that time concerned with the steering, I only examined this phenomenon so far as it related to the investigation in hand, the results of which investigation were contained in a Paper read before Section G at the British Association last year. Subsequently, however, it occurred to me that this difference in the speed of the following currents must play an important part in the action of the screw-propeller, particularly as regarded the vibrations.

The Relative Speed of the Upper and Lower Currents in the Wake.

As is well known, a ship imparts an onward motion to the water in its wake in two ways—by the friction of the skin, and by the wave which follows the ship. From neither of these causes does it appear that the motion imparted to the water will be equally distributed through the whole area of the wake; but, on the other hand, it appears that both causes will act to give the water near the surface a greater onward velocity than that which is on a level with the keel of the ship, and that water which is directly behind the stern-post a greater velocity than that which is more on one side.

When a long narrow plane is dragged through the water in the manner adopted by Mr Froude in his experiments on surface friction, its only effect, in the way of setting the water in motion, is that of skin-resistance; but even here the upper water will be made to move faster than the lower. The motion imparted to the water in the immediate vicinity of the plane is rapidly communicated to the adjacent water, and so becomes more or less dissipated. Now at the top of the plane the only direction in which this dissipation can extend is laterally, whereas towards the bottom of the plane the dissipation can extend downwards as well as sideways, and is therefore much more rapid, leaving the water near the bottom of the plane moving with less velocity than that near the top. So that, looking at a ship as a long narrow plane, we see that even so there would be not only a difference between the velocity of the water in the middle of the wake and that towards the outside, but that there would also be a difference in the velocity at different elevations. A ship, however, differs considerably from a plane, and its form tends further to increase the inequality in the motion of the water.

The water which fills the opening left by the ship in large part rises up from beneath its bottom, and in rising carries up to the surface that water which has received the greatest onward motion from rubbing against the skin, supplying its place below by fresh water without any onward velocity. This would be the case even if the run of the ship were in the form of a vertical wedge, and the actual form of the ship, which is more like an inclined plane than a vertical wedge, tends greatly to increase this action, for the water moves upwards along what are called the geodetic lines.—See Rankine's *Shipbuilding*, p. 83.

The fact that the lines of a ship are much fuller near the surface than those below tends also to give the upper water greater forward motion.

Again, the form of a ship is such as to cause a wave to follow it, the crest of the wave being not far from the stern-post. This wave also causes a greater onward motion in the particles of water near the surface than those which are below.

We see therefore, taking all the causes together, that there is probably a very considerable difference in the relative onward velocity imparted by the ship to the water in which the upper and lower blades of the screw work. There is also a difference in the velocity of the water at different lateral distances from the middle of the wake, but this latter variation is not of any direct importance as regards the object of this communication and therefore will not be considered farther.

The Actual Velocity of the Wake.

Before we can form an estimate of the probable magnitude of the actual difference in the velocity with which the upper and lower currents move, it is necessary to arrive at some conclusion as regards the proportion which the velocity of the wake bears to that of the ship. The actual motion imparted by a ship to the water in its wake has never, so far as I am aware, been experimentally investigated; there are however two ways in which estimates have been formed;—by observations on the surface, and by calculations based on the resistance of the ship.

If one may judge from various incidental comments, one finds that the observation of the motion at the surface of the wake has led to much higher estimates of its velocity than the calculations from the ship's resistance.

Mr G. B. Rennie remarks, "The current caused by the onward motion of the ship has a velocity at the stern equal to that of the ship itself*."

Mr Griffith says, "The water in which the screw works is an eddy which follows the ship at the same speed or nearly so...if a patent log were placed in the screw opening, it would not even approximately indicate the speed of the ship†."

I would remark here that I do not make these quotations in order to show that they are wrong, but simply to show that observation of the surface has led those who have had the best opportunities of judging, to form a high estimate of the onward motion imparted to the wake for the purpose of comparing this estimate with that based on the resistance of the ship.

In his *Marine Engineering*, Rankine gives a rule for calculating the velocity of the wake (see p. 249); and, applying this rule to the 'Warrior' (a very long ship), he finds that the speed of the water near the stern-post is ·09 the speed of the ship.

We see, therefore, how widely this estimate differs from the estimates formed from observations at the surface. The previous argument, however, regarding the difference between the velocity at the surface and that below will go a long way to reconcile these estimates.

Rankine's estimate is based on the supposition that all the water following the ship has the same onward motion imparted to it. A very different result, however, is arrived at, if instead of the entire mass of water in the wake, it is only, or principally, the upper layers that are supposed to be set in motion. The speed imparted to the water must be inversely proportioned to the volume acted on, so that if the motion only extends to the bottom of the ship, and gradually dies out, instead of the velocity being 10 per cent. it will be 20 per cent. of that of the ship.

This seems to me to agree with what may be observed on looking over the stern of a paddle steamer, or a sailing ship. In the case of the steamer, the inner ends of the lines of foam left by the paddles become curved forwards as

* *Modern Screw Propulsion*, p. 19. By N. P. Burgh. † *Ibid.* p. 45.

they approach the stern, where they join the wake, and are violently dragged forward with a velocity of certainly more than one-tenth that of the ship.

As a rough estimate, therefore, I should conclude that in a ship with a fairly fine run the velocity of the wake, at the surface, is not less than ·20 that of the ship, while at the level of the keel the water is practically stationary. And, in the cases of ships, moving at an abnormal speed, carrying a high stern wave, or having full sterns, the difference may be increased to almost any extent.

The Effect on the Screw.

Having now shown that there is a considerable difference in the onward velocity in different parts of the ship's wake, it only remains to consider what effect this difference would have on the action of the screw.

Compared with the speed of the ship the difference is after all but small, and if the thrust of the screw depended on the speed of the ship, this small difference might well be neglected; all that would result would be a difference of some 20 per cent. in the pressure on the upper and lower blades. I cannot help thinking that it is owing to some such confusion as this between the action of the screw and the speed of the ship that the unequal motion of the water in the wake has remained unattended to for so long. When we realize the fact that the thrust of the screw does not depend on the speed of the ship, but on the difference between the speed of the ship and the geometrical speed of the screw—the speed at which it would have to move forward were the water unyielding; and that this difference, called the slip, is somewhere between one-tenth and one-fourth the speed of the ship, we see at once what an important influence an increase in the speed of the water anything like one-fourth the speed of the ship would have. Hence, although the inequality in the motion of the water is small as compared with the speed of the ship, as compared with the slip it is very large, and this is the essential comparison.

The slip of a screw would be somewhere between one-tenth and one-fourth the speed of the ship if it were equally distributed over the entire area through which the screw acts. But if there is a difference in the rate at which the water is following the ship at different parts of the section of the screw race, then the slip, and consequently the pressure on the blades of the screw, will be greatest at those places where the water is following the ship fastest.

Taking the mean slip at ·2, and supposing the upper blades to be working in a current which has an onward velocity ·2 greater than that in which the lower blades are working, then the slip at the tops of the upper blades would be ·3, and that at the tops of the lower blades only ·1; so that the resistance at the tips of the upper blades would be three times as great as that at the tips of the lower blades. Or, to put it roundly, the area of the water on which the screw acts to drive the ship forward would be virtually reduced, it would be the blades above the shaft that principally drive the ship, the lower blades merely passing through the water.

The Tendency to cause Vibrations.

Under these circumstances it is clear that the lateral resistances which the upper and lower blades encountered would no longer balance each other. For example, on a two-bladed screw the pressure on the blades would only be equal when they were both on a level with the shaft. As the one rose towards the vertical position the resistance would increase, while that on the lower blade would diminish. The action of the screw would, therefore, be to cause an intermittent force, urging the stem in the direction opposite to that in which the tips of the upper blades were moving.

The magnitude of this intermittent force would be very considerable—under the circumstances assumed above, it would, while it acted, be comparable to the entire lateral resistance encountered by the screw. It would, therefore, afford sufficient explanation of the screw's tendency to cause vibrations, which the shock caused by the water thrown by the screw against the stern-post does not. It would also fully accord with what experience has shown respecting the effect of the screw on the steering of the ship.

Effect on the Efficiency.

Such an inequality in the action of the screw as that described above, would not necessarily reduce its efficiency as a propeller. So long as there was some small slip left to the bottom blades there could be no actual retardation of the ship. But if the inequality in the motion of the water should at any time bear such proportion to the mean slip that the lower blades could not, as it were, screw themselves through the water fast enough to keep up with the ship, then they would have to be dragged through the water, and would retard the ship. Such a result would only be experienced when the inequality of motion in the water was more than double the mean slip of the screw. Such a state of things, it would appear, could only be brought about by a vessel moving at an abnormal speed and carrying a large stern wave, or by a vessel having a very full stern, conditions which are invariably found to result in loss of efficiency and excessive vibration, and very often in what is called negative slip. The loss of efficiency which usually attends negative slip, has received what appears to be a satisfactory explanation as being due to the back suction, or reduction of pressure which the action of the screw causes on the stern of the ship. And that it is in some part at least due to this cause has been proved by Mr Froude by actual experiment. But, considering that when this action occurs, all the conditions which would cause the lower blade to drag back are known actually to exist, it would seem to be highly probable that at least in part, the loss of efficiency, as well as the excessive vibration, is due to the unequal motion of the water on which the upper and lower blades of the screw act.

Disadvantage of Large Screws.

It can be easily seen that the effects which have been attributed to the unequal motion of the water would be greater with screws, which are large in proportion to the draught of the ship, than with those which are smaller.

There are two reasons for this. In the first place the larger the screw the smaller must be the mean slip, and consequently the greater would be the proportion which the inequality of the motion of the water would bear to it. On the smaller would be the margin, allowed for the difference in the slip at the top and bottom of the blades. And, secondly, the larger the screw the greater would be the difference in the motion of the water in which the upper and lower blades worked.

Now I believe it has been found, as a matter of experience, that there is a limit to the size of the screws which give the best results for each ship. This limit is doubtless in part due to the increased friction which large screws experience, owing to their increased surface; but the friction must be much larger than what we have reason to suppose it is, if this alone can account for the limit. It seems probable therefore that this limit is another result of the inequality of the motion of the following waters.

A few years ago a large Atlantic steamer was fitted with a screw, which could be lowered until its blades extended below the bottom of the ship. Various advantages would appear as likely to result from such an arrangement. But it seems to me to be probable that the disadvantages resulting from the inequality in the motion of the wake would be considerably increased, for the lower blades of the screw would descend into the water with no following motion at all, while the upper blades would still be high up in the wake. I do not know what was the result of the experiment, but I have heard that the plan had to be abandoned on account of the excessive vibration.

Conclusion.

It is not my object in this Paper to enter upon the question as to what modification in the construction or dispositions of screws might be suggested by the recognition of the unequal motion of the wake and its effects. Any suggestions I might make would be premature. My endeavour has been solely to elucidate further the actual conditions or circumstances of the problem of screw propulsion, it being my conviction that a complete knowledge of the conditions of any problem must be conducive to its eventual solution. My opportunities of studying the action of screws are limited, and in venturing to come before you, my inducement has been that my ideas would be criticised by those who have much better opportunities. If, through ignorance, I have been occupying time by dilating on what is unimportant or already known, I can only hope that I may have your indulgence; a claim which I feel entitled to make, as it is only the importance which you were pleased to attach to my former communication which has emboldened me to come forward again.

22.

ON THE REFRACTION OF SOUND BY THE ATMOSPHERE.

[*From the* "Philosophical Transactions of the Royal Society of London," Vol. CLXVI., pt. 1.]

(*Read* January 6, 1876.)

In a paper read before the Royal Society, May 1874, I pointed out that the upward diminution of temperature in the atmosphere (known to exist under certain circumstances) must refract and give an upward direction to the rays of sound which would otherwise proceed horizontally; and it was suggested that this might be the cause of the observed difference in the distinctness with which similar sounds are heard on different occasions, particularly the very marked advantage which night has over day in this respect. At the time at which that paper was written no direct experiments or observations had been made to verify the truth of this suggestion, and therefore its probability rested on its reasonableness. Since that time, however, I have carried out a series of observations and experiments which, although far from complete, throw some light on the subject, besides revealing some remarkable facts. I hope to be able to continue the investigation; but since its nature is such as to render the chance of bringing it to anything like a final conclusion very uncertain, it seems to me that it may be well to publish an account of what has been already done; and this is the object of the present communication.

In order to render the object of the various experiments clear, it may be well to recapitulate here some of the theoretical considerations previously explained. It will be remembered that the idea that the variations of temperature would cause refraction of sound occurred to me while making experiments on the effect of wind upon sound, from which it was shown that when sound proceeds in a direction contrary to that of the wind, it is not, as had been thought, destroyed or stopped by the wind, but that it is lifted, and that at sufficiently high elevations it can be heard to as great distances as in other directions, or as when there is no wind—thus confirming the hypothesis first propounded by Professor Stokes and afterwards by myself, that the effect is owing to the retardation of the velocity of the wind near the earth, which allows the sound moving against the wind to move faster below than above, and thus causes the fronts of the waves to incline upwards, and consequently to move in that direction. Having clearly shown that this was the case, it became apparent that anything which would cause an upward diminution in the velocity at which sound proceeds would cause a similar effect to that of the wind and lift the sound, and that since the speed of the sound depends on the temperature of the air in which it is moving, an upward diminution in the temperature must cause such an effect. That such a diminution of temperature does very often exist was proved by Mr Glaisher's balloon ascents in 1862, in which he found that when cloudy the mean rate of diminution for the first 300 feet was $0^\circ{\cdot}5$ for each 100 feet, and that when clear it was 1°, and that on some occasions it was greater and on others less than this. A variation of 1° in the temperature of the air alters the velocity of sound nearly 1 foot per second, so that with a clear sky the sound instead of moving horizontally would move upwards on a circle of 110,000 feet radius, and with a cloudy sky on a scale of 220,000 feet radius. This rate of refraction is very small compared with that caused even by a very moderate wind; and consequently in order to verify it by experiment it is necessary to observe sounds at much greater distances. This renders the experiment very difficult to carry out; and to make it worse we have no means of determining what the upward variation of temperature is, which therefore can only be surmised by the behaviour of the sound.

The method of experimenting which first suggested itself was the same as that which I had previously employed for wind—namely, to obtain a means of producing a sound of certain intensity, and proceeding to such a distance that it could no longer be heard at the ground or on the level, and then ascertaining whether the range was extended by attaining a greater elevation or elevating the source of sound.

The difficulty in every item of the experiments was greatly enhanced by the increased distance. For the wind an electric bell had answered very well, the range on the level being always less than a quarter of a mile; but where the range was to be measured in miles, something in the nature of an explosion was the only sound available. A place in which to make the experiments was also difficult to find; for it involved a range of several miles of level and unobstructed country, and thus the time occupied in moving from place to place became a matter of serious inconvenience. The greatest difficulty of all, however, was the effect of the wind; since this was much greater than anything to be expected from the temperature, it was absolutely necessary that the air should be quite calm, a circumstance which no precaution will insure, and for which, as I know from experience, one may have to wait a long while. These various circumstances rendered the results of the first series of experiments less conclusive than I had hoped they might prove.

Experiments with rockets.

I obtained a quantity of rockets capable of rising to a height of 1000 feet and exploding a charge of 12 ounces of powder. The first experiments with these rockets were made at Debach, a village lying between Ipswich and Framlingham, where the country is tolerably flat and traversed by roads in all directions.

I. On the 14th of July, at about 3 P.M., three rockets and three cartridges were fired from the same spot, observers being stationed at three-quarters of a mile and a mile and a half respectively. There was no wind, but the sky was covered with a thick haze, the day being very hot. All six discharges were heard at the nearer station, but only the rockets the distance of a mile and a half, although these were heard very distinctly, even their hiss as they ascended.

II. On the 16th of July, at 3 P.M., the day being very hot with no wind, a single rocket was sent up, an observer being stationed at four miles and a half on the Woodbridge road. The explosion was very distinct, but the hiss was not heard.

III. On the 18th a series of rockets were compared with the discharges of a gun capable of firing $\frac{1}{4}$ lb. of powder, and which made a much louder report than the rockets. The observers drove along the Framlingham road, the times of the discharges having been determined beforehand. This road was chosen because at the commencement of the experiments the wind was blowing almost at right angles to it. The wind was very light when the start was made, but before the first gun was fired it had considerably strengthened and changed in direction so as to blow against the sound. It was to this cause I attribute the fact that the first two guns were not heard at a distance of a mile and a half and two miles respectively. After this the direction of the wind again changed, and the two next guns were heard distinctly, although at greater distances; but, strange to say, the rockets at the same distance were not heard. The wind remained constant in this direction until the end of the experiments, and a rocket was heard at four miles. Owing to the changes in the wind the results of these last experiments have shown nothing as regards the refraction of sound, although they show (what was, indeed, shown by the previous ones) that it is possible on a very hot day when there is little or no wind to hear the discharge of a small cartridge, such as that carried by the rockets, distinctly for a distance of four or five miles, and this when the lower stratum of the atmosphere was so heterogeneous that all distant objects near the ground appeared to waver and twinkle as they do when seen over the top of a furnace.

In the hope of improving the conditions of the experiments, I accepted the invitation of my friend Major Hare, of Docking, in West Norfolk, to accompany him in his yacht the 'Feronia' during a cruise on the east coast, taking rockets with me. Here I spent three weeks without having a single calm day.

Experiments in Lynn Deeps.

On the evening of the 18th of August, however, the weather improved; and being then in Lynn Deeps, I made some preliminary experiments so as to get the men into the way of firing the rockets. The yacht was at anchor in what is called the Upper Road, and at 9.50 P.M. I rowed with two men in a direction slightly to leeward of the yacht. The wind was very light: at a distance of two miles they fired a large pistol; the interval between the flash and the report was eleven seconds (which gave us our distance); the report was loud and accompanied with prolonged reverberation; a rocket was also heard distinctly, but was not so loud as the pistol, and was not accompanied with any echoes or reverberation. The hails from the yacht were heard by us in the boat quite distinctly, but our answers were not heard on board the yacht. As there was a light mist it was not thought safe to go further away from the yacht, so we returned and waited in hope of being able to do something the next day. In this we were not disappointed; for on this day we observed what I have no doubt will be thought an extraordinary phenomenon, although not of the kind anticipated.

The morning was perfectly calm, with only a few local breaths, which, measured with the anemometer, never registered more than two miles an hour, and came first from the east and then from the west, but not from the north. Up to 12 o'clock the sky was completely covered with a white cloud, which did not show the least sign of movement. The land from four to eight miles distant was hazy; the thermometer stood at 65° in the cabin with all the lights open. The Upper Road, in which the 'Feronia' was anchored, is two miles below the ends of the stone banks which terminate the Lynn Cut, and five miles from Lynn (see accompanying chart, page 169). From this station sounds in Lynn were distinctly heard. Steamers could be heard leaving the dock.

About 12 o'clock the sky cleared, and a slight breeze (four miles) sprang up. We then weighed anchor and proceeded down the Bull-dog Channel. Soon after the sky became perfectly clear, and the breeze died away until the yacht had not steering-way. I then had a boat lowered (with the same two men), and proceeded to row to the Roaring Middle Buoy, while the yacht still continued her course as well as she could down the Bull-dog Channel; she was going north by east in a curve, while we were going north-west. Before leaving the yacht I arranged that on our showing two flags they should send up a rocket, and when one they should fire a pistol, and that whenever they heard us call they should answer. When about half a mile distant I commenced calling, and the answers came back quite distinct; when a little further some one on the yacht commenced tapping the anchor, and we heard this quite distinctly until we were nearly two miles off them; then the tapping was discontinued, and I commenced calling again. Each time the answer came back quite distinct at the instant it was expected, and afforded a good means of checking our distance, which we also knew from the buoys. At two miles, although the calls were quite distinct, I signalled for a pistol; the report was loud. The sun was very hot to us in the boat—so hot, indeed, that it blistered the skin on my hands and face.

The next time I called, the answer was doubtful; but on my calling again, it came quite distinct in thirty seconds. I then signalled for a pistol, and heard a report which we took to be a pistol, but afterwards found to be a rocket, we being too far off for them to distinguish our signals. I then asked for a rocket, and had one, of which we heard the hiss as well as the report. We now proceeded up to the Roaring Middle Buoy and signalled for rockets and pistols, but could get neither, so we judged that they could not see our signals. Although it seemed hopeless, I called from this point, and to my surprise we all heard the answer faint but quite distinct after an interval of thirty-five seconds. It was now about 3 P.M., so that we had been rowing about two hours and a half. We waited at the buoy and kept calling; but as there were now a number of fishing-boats which answered our calls we could not be certain of an answer. At this time our calls appear to have been heard on the yacht but not answered. When we heard the last call, to be sure of it, the yacht was close by the Sunk Buoy; she was now approaching the Well light-ship, which is six miles from the Roaring Middle Buoy. There was now a very light breeze again, so we set up our sail to get steering-way on, and fell down with the tide. We presently heard a rocket go up and explode, but we could make no impression with our signals: we found on returning that they had completely lost sight of us; nor was this surprising considering that we were in a small boat and the sun was directly behind us. A breeze sprang up, so we returned to the yacht, where on comparing notes we found that we had heard every call as well as report. During the interval in which we had no answers, Major Hare, who had been answering my calls, having completely lost sight of us, had gone below to get some lunch; in the mean time the men on deck had heard our calls, but not having instructions had not answered them.

To sum up the results of our excursion:—We had called and been answered up to three miles and a half, and our calls, as well as the reports of the rockets, had been heard to more than five miles.

Incidentally, I noticed that we could occasionally hear the reports of guns from the shore, which was more than eight miles distant; and once while listening for an answer to one of my calls, I distinctly heard a dog bark, which must have been on shore, as there was no boat between us and it except the yacht. All the time we could distinctly hear the paddles of a steamer, which at the time we were at the Roaring Middle was in the Wisbech Channel, or nine miles from us and fifteen from the yacht, on which her paddles were also distinctly heard.

It appears to me that the distances at which sounds of such comparative low intensity were heard over the water this day is beyond anything definitely on record. One hears casually, however, of remarkable instances: once in this district I heard of a clergyman who from the Hunstanton side of the Wash heard a man hammering a boat on the Wisbech side. When one thinks, however, of the extreme difficulty of identifying a sound with its source at three or four miles distance, it is no matter of surprise that such phenomena should for the most part escape notice. On this day, had we not been purposely on the look-out, I do not think anything we heard would have attracted our attention. I have often heard the rifles of volunteers over tolerably flat country seven miles; and, as I have previously stated, the guns of the naval review at Portsmouth were heard by many persons, including myself, in Suffolk, over a distance of 170 miles*.

With regard to the cause of the exceptional distances over which we heard the sounds on the 19th of August, 1874; as was only natural, my attention was all the while directed to this. For the sake of my experiments, what I had been in hope of was a state of the atmosphere which would cause great upward refraction of the sound, and I was naturally on the *qui-vive* for any indications of such a state. All the morning I had been watching the distant objects to see whether they were lifted or depressed by the refraction of light. They loomed to a remarkable degree, which showed that the upward variation of temperature was the reverse of what I wanted; and before leaving the yacht I had my doubts of our finding much upward refraction of sound—of our being able to hear the rockets further than the guns. I was in hopes, however, that as the sun came out matters might change, and while in the boat I kept looking out for signs of depression in the distant objects. These,

* They were also heard by Sir William Thomson, who was on board his yacht about 10 or 15 miles to the west of Portland, and therefore 180 miles from Dover.

however, never came; they loomed all the time, and very considerably. From the boat we could see the water for five or six miles. The yacht's hull was visible to us all the time. On one occasion we had two buoys and a ship in a line, the nearest buoy being two miles from us; we could see the water between this and the second, and again between this and the ship.

It seems to me, therefore, that although in a manner the reverse of what was expected, our observations this day prove the very great effect which upward refraction has on the distances at which sounds can be heard. The looming of the distant objects showed that the air was colder below than above. This would tend to bring the sound down and intensify it at the surface of the water—in fact convert the sea into a whispering-gallery.

No other explanation appears to hold good. The conditions were exactly those which have been described as favourable to acoustic opacity; the sea was calm, there was no wind, and an August sun was shining with its full power, and, having evaporated the clouds, must have been raising vapour from the sea.

During the experiment I particularly noticed the echoes. Except the first and only pistol, none of the reports were attended with echoes or reverberation. But in most cases, though not in all, after calling I could hear the ring of my voice for ten or eleven seconds; and on one or two occasions when there were boats within half a mile of us, I could distinctly hear the echoes from them. Without attempting to explain the reverberation and echoes which have been observed, I will merely call attention to the fact that in no case have I heard any attending the reports of the rockets, although they seem to have been invariable with the guns and pistols. This fact suggests that these echoes are in some way connected with the direction given to the sound. They are caused by the voice, trumpets, and the siren, all of which give direction to the sound; but I am not aware that they have ever been observed in the case of a sound which has no direction of greatest intensity.

Arago's Experiments.

These observations in Lynn Deeps were the last I made in 1874. In the spring of this year my attention was called to a phenomenon recorded by Arago, which was noticed during the celebrated experiments on the velocity of sound made by Humboldt, Arago, Prony, Gay-Lussac, and others, on the nights of the 21st and 22nd of June, 1822, between Villejuif and Montlhéry. On both these nights the sounds from Montlhéry were heard more distinctly at Villejuif than the sounds from Villejuif at Montlhéry, although the wind was blowing (very lightly) from Villejuif to Montlhéry, the speed of the wind being about one foot per second, or, roughly, three-quarters of a mile an hour. This remarkable want of reciprocity was much commented on by the observers, although they appear to have been entirely at a loss to account for it.

On reading M. Arago's report*, I noticed that the observations on the barometer showed Montlhéry to be about 80 feet above Villejuif, and it occurred to me that this difference of elevation might afford a clue to the mystery. I had observed, in my observations of the effect of wind upon sound, that a difference of a few feet in the height of the observer or in the source of sound, especially when near the ground, often made all the difference between hearing distinctly and not hearing at all. It appeared to me probable, therefore, that there might be something advantageous in the situations of the gun at Montlhéry, and the observers at Villejuif, over the situations of the gun at Villejuif and the observers at Montlhéry. I was confirmed in this impression by a fact mentioned by Arago, viz. that on the first night the gun at Villejuif had been pointed upwards at a considerable angle, but that thinking this might have had something to do with its not being heard so well as the other, on the second night it was brought down to the horizontal. The result, however, was that the gun was not heard so well on the second night as it had been on the first. This remark concerning the gun at Villejuif seemed to imply that it was fired from level ground and at no great elevation, whereas at Montlhéry it seemed possible that the gun might have been fired over a parapet. To settle this question I took an opportunity last Easter of walking over the ground from Villejuif to Montlhéry, and by the aid of a map made a section of it.

The two stations are visible from each other; that at Villejuif is on the top of a gently rising hill, whereas that at Montlhéry is on the top of a very steep sugar-loaf hill, terminating in the mound of an old castle, which is supported on the side facing Villejuif by a wall some 20 feet vertical, and then so steep that Villejuif can be seen over the tops of the trees surrounding the castle. Part of the old parapet wall is left, and it is impossible to believe but that anyone firing a gun from that spot would place it with its muzzle over the parapet. It seems very probable, therefore, that the gun at Montlhéry was fired over the parapet, which would be the most favourable position for being heard, as the direct sound would be strengthened by that reflected from the wall below it, while the observers, standing somewhat behind the parapet, would not have the advantage of any reflected sound, and would therefore be in a disadvantageous position as compared with the muzzle of the gun. At Villejuif the case would be different; the gun, as fired on level ground, would be at a disadvantage compared with the observers, whose ears would be considerably above it. That this difference was sufficient to affect the results

* *Annales de Chimie*, 1822, p. 211.

seems to have been proved by the evil effect of lowering the muzzle of the gun*.

These differences in the conditions of the guns and the observers would seem to afford good reason why the guns from Montlhéry should have been better heard than those at Villejuif, supposing other conditions for the transmission of sound to be equally favourable both ways; but the wind was blowing from Villejuif to Montlhéry, and that this should not have reversed the effect is the most remarkable part of the phenomenon. This is remarkable, however, only on the supposition that the effect of the wind upon sound is invariable. As it seemed to me that there were several good reasons for supposing that this is not the case, I thought it might be worth while trying a few observations. I accordingly made some experiments with my electric bell on some very calm nights in May and June, with the following results:—

When the sky was cloudy and there was no dew, the sound could invariably be heard much further with the wind than against it, even when the wind was not more than one foot per second.

But when the sky was clear and there was a heavy dew, the sound could be heard as far against a light wind as with it, and sometimes much further. On one occasion, when the wind was very light (about 1 foot per second at 6 feet above the ground) and the thermometer showed 39 degrees at 1 foot above the grass and 47 at 8 feet, the sound was heard at 440 yards against the wind, and 270 yards with it.

Now the nights on which Arago made his experiments were clear; there was a heavy dew, and the thermometer at Montlhéry showed that at that

* From my previous experiments on the effect of wind upon sound, I had been led to the conclusion that under certain circumstances there may be an absence of reciprocity in the passage of sound backwards and forwards between two points. Lord Rayleigh, however, pointed out to me that there are strong reasons for believing that this is not the case. To prove the force of these reasons, I made some observations behind a large wheat-stack standing alone on level ground, experience having shown me that a wheat-stack from its rough surface is a most effectual barrier to sound—sound produced close to one side of the stack being quite inaudible on the other side. On this occasion, however, I found the most perfect reciprocity; sounds produced close behind the stack could be heard at a distance just as well, and no better, than similar sounds at a distance could be heard behind the stack, provided always that great care was taken to bring the ear behind the stack into exactly the same position as that previously occupied by the source of sound. It appears, however, that a few inches difference in the position of the ear or the source of sound was sufficient to make all the difference as to the audibility of the sound. These experiments therefore, although they confirmed Lord Rayleigh and showed my previous idea to have been wrong, suggested another explanation of the phenomenon which had led me to it. They show that the apparent absence of reciprocity was in reality caused by my not having taken sufficient notice of small differences in the position of the ear and the bell, and they suggest that the apparent want of reciprocity in the experiments made at Villejuif and Montlhéry was due in the same way to the small differences in the positions of the guns and the ears of the auditors, as pointed out in the text.

elevation the temperature was 2° F. greater than at Villejuif; so that after the experiments just described there is nothing surprising in the fact that the wind did not produce much effect on the sound.

A good reason (as I have previously stated) may be given in explanation of these changes in the effects of the wind. The wind tends to lift the sound proceeding against it and to bring down that which is travelling with it. These effects are greatest near the earth and diminish as we proceed upwards (for the simple reason that the retardation of the wind is greater near the surface). The effect of the wind, therefore, will be to intensify the sound proceeding against it at sufficiently high elevations (this was found to be the case in my first experiments) and to weaken the sounds proceeding with it at points at some height above the surface—that is, when the sound which is brought down is destroyed by the roughness of the surface, though over a calm sea, the sound brought down would roll along the surface as in a whispering-gallery. Now when the temperature diminishes upwards, as it does generally during a calm day, the effect of the refraction thus caused will be to increase the effect of the wind on sound moving against it, and to diminish that on the sound moving with it. But when the diminution of temperature is downwards, as it was at Villejuif and Montlhéry, and as it always is near the earth on a clear dewy night, it will directly diminish the effect on sound moving against the wind, and increase it on the sound moving with the wind. That is to say, it will prevent the wind lifting the sound in one direction and will aid it in bringing it down in the other. Thus it will prolong the distance to which sound can be heard against the wind, and diminish that at which it can be heard with the wind (when the surface is rough); and when the downward diminution of temperature bears a certain relation to the strength of the wind, it is easy to see that it may neutralize or even reverse its effect.

These facts, all taken together, appear to me to afford a satisfactory explanation of the phenomenon observed by Arago. There was, however, one other phenomenon observed during the same experiments on which I will venture a word in explanation.

The reports of the guns at Montlhéry as heard at that station were attended with prolonged echoes, but it was not so with those at Villejuif. This phenomenon was not explained by the experimenters; but I think it admits of a simple explanation. The ground surrounding Villejuif towards Montlhéry is very flat with not a tree upon it for miles, and being all arable would at that time of the year be covered with crops. Around Montlhéry the country is hilly, some of the hills rising 100 feet above Montlhéry itself; their sides are in many places precipitous, and are largely covered with trees. From the flat country around Villejuif there would arise no echoes, but from the hills and trees around Montlhéry it is quite certain that there must arise very considerable echoes; and hence it seems to me that the phenomenon becomes simple enough.

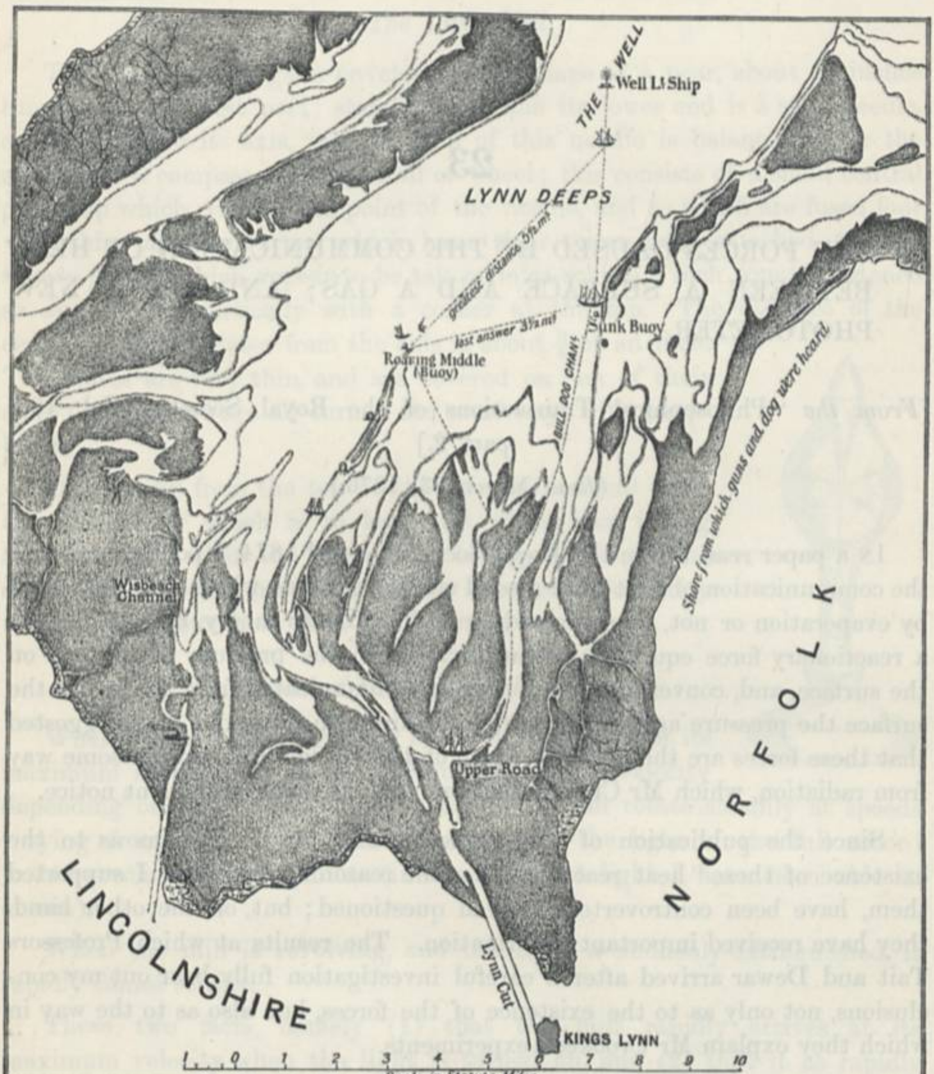
The Report of the American Lighthouse Board.

I may remark, in conclusion, that I have just received a copy of the Report of the American Lighthouse Board, kindly sent me by Dr Henry, the Chairman of the Board. In an appendix to this Report Dr Henry has given an account of his experiments on the transmission of sound, undertaken for the Board, and extending over the last thirty years. These experiments have led him to the conclusion that the differences in the distances at which the same sound can be heard at different times are in all cases to be explained by refraction. He has ascribed the cause of the refraction to the wind; and to explain cases in which the refraction did not accord with the direction of the wind, he points out that it is not sufficient to know the direction of the wind at the surface, but that in order to say what would be its effect upon sound we should know in what direction it is blowing above; for it is not the simple motion of the wind which affects sound, but the difference between its motion above and below. This is very true; and I have met with instances at night which have led me to apply the same explanation. Many of the phenomena, however, to which Dr Henry has applied this explanation are, I feel sure, to be attributed to the effect of the upward variation of temperature. Dr Henry does not appear to have been aware of this cause of refraction of sound while making his experiments or drawing up his Report; but in a note at the end he expresses his general agreement with the views stated in my previous paper.

The Heterogeneity of the Atmosphere.

With respect to the stoppage of the sound by the heterogeneity of the atmosphere, Dr Henry expressly states that through all his long experience he has never met with a single phenomenon which he can fairly ascribe to this cause; and so far as my experience goes it agrees with that of Dr Henry. I am far, however, from thinking that there is no such effect; on the contrary, under circumstances such as those which Humboldt describes as having led him to the idea, it seems to me that it must exist, but that it must at all times be confined to a very small distance above the earth's surface and be over land. That it is the principal cause, or even an important cause of the phenomena under discussion, appears to be more than doubtful; for not only does the necessary effect of refraction appear to be a sufficient cause for these phenomena, and therefore to afford a complete explanation of them, but it is very difficult to conceive the existence of a state of heterogeneity in a calm clear atmosphere at a considerable elevation above the level of the sea.

In the first place such a state of heterogeneity could hardly fail to be observed; for it would necessarily impart a flickering and unsteady appearance to objects seen through it—an effect which may be observed any hot summer's day when looking at objects low down over dry land. Over the sea, however, such an appearance has not been recorded; and although I have often looked for it, I have been entirely unable to detect it. And in the second place, even supposing the air to be in a heterogeneous state at any given instant, such a state could not be maintained many minutes; for different gases, or different portions of the same gas at different temperatures, mix and diffuse very rapidly. It is true that the heterogeneity might be maintained by upward streams of heated air or vapour, and this is doubtless the cause of the heterogeneity of air over dry hot ground; but this heterogeneity, although very apparent near the ground, is never observed at any considerable height. Upward streams of heated air must tend to mix and diffuse rapidly, and the air as it rises is cooled by expansion until it must soon cease to be lighter than the surrounding air. That, as a rule, there are no streams of heated air ascending to any considerable height over land, is definitely proved by the fact that the light smoke from burning weeds never, or very seldom, attains an elevation of anything like 100 feet. I have often been struck with the way in which such smoke will creep along the ground for the distance of half a mile, and even then not extend to an elevation of more than 20 or 50 feet. Over the sea the cause of such streamlets must be much less potent than over land, and their existence still more unlikely.



THE WELL
Well Lt Ship
LYNN DEEPS
greatest distance 5½ m.s
last answer 3½ m.s
Sunk Buoy
Roaring Middle
(Buoy)
BULL DOG CHANL
Shore from which guns and dog were heard
Wisbeach Channel
Upper Road
NORFOLK
LINCOLNSHIRE
Lynn Cut
KINGS LYNN
1 0 1 2 3 4 5 6 7 8 9 10
Scale in Statute Miles.

23.

ON THE FORCES CAUSED BY THE COMMUNICATION OF HEAT BETWEEN A SURFACE AND A GAS; AND ON A NEW PHOTOMETER.

[*From the* "Philosophical Transactions of the Royal Society," Vol. 166, part 2.]

(*Read* March 23, 1876.)

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

* *Proc. Roy. Soc.* 1874, vol. xxii. p. 401 (Paper 11).

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 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 lamp-black.

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.

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

* 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.

It will be seen that at very small velocities the effect of two candles is rather more than double that of one; 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 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,

$$Ad\tau = 2vdv,$$

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

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 the 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

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

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

and by the foregoing

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

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

Therefore if, as we have calculated,

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

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

and taking $\tau=520^\circ$ 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.

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

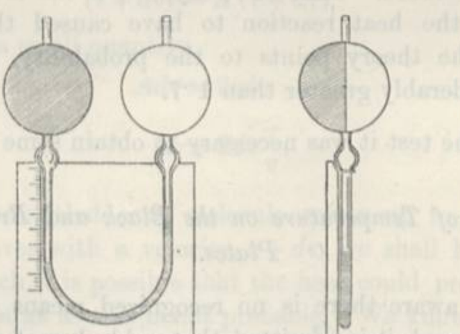
While taking part with Dr Schuster in his experiments, I held an ordinary thermometer containing some dark red fluid, in the place which the mill had occupied, exposed to the light. This came from a lime-light, and was condensed by an ordinary lantern.

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 $1/\pi$. 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 quantitative 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 suspension more delicate than that then adopted, I was forced to believe that the effect found by Mr Crookes was due to some

* 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.

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 $\frac{1}{100}$ of the force on the mill.

24.

ON VARIOUS FORMS OF VORTEX MOTION.

[*From the* "Proceedings of the Literary and Philosophical Society of Manchester," Feb. 1877.]

(*Read* February 6, 1877.)

PROFESSOR OSBORNE REYNOLDS exhibited various forms of vortex motion in a large glass tank by means of colour, or bubbles of air, the vortex lines behind an oblique vane, the vortex ring behind a circular disc, the vortex rings caused by raindrops, and the vortex rings caused by a puff of water. The various ways in which these vortices move were also shown. But Professor Reynolds' object in showing these experiments was to illustrate the importance of the method of study rather than the intrinsic importance of the results already obtained, which are not as yet sufficiently complete for publication.

(*For continuation see* p. 184.)

25.

ON VORTEX MOTION.

[*From the* "Proceedings of the Royal Institution of Great Britain," Feb. 1877.]

(*Read* February 2, 1877.)

In commencing this discourse the author said: whatever interest or significance the facts I hope to set before you may have, is in no small degree owing to their having, as it were, eluded the close mathematical search which has been made for them, and to their having in the end been discovered in a simple, not to say commonplace, manner. In this room you are accustomed to have set before you the latest triumphs of mind over matter, the secrets last wrested from nature by gigantic efforts of reason, imagination, and the most skilful manipulation. To-night, however, after you have seen what I shall endeavour to show you, I think you will readily admit that for once the case is reversed, and that the triumph rests with nature, in having for so long concealed what has been so eagerly sought, and what is at last found to have been so thinly covered.

The various motions which may be caused in a homogeneous fluid like water, present one of the most tempting fields for mathematical research. For not only are the conditions of the simplest, but the student or philosopher has on all hands the object of his research, which, whether in the form of the Atlantic waves or of the eddies in his teacup, constantly claims his attention. And, besides this, the exigencies of our existence render a knowledge of these motions of the greatest value to us in overcoming the limitations to which our actions are otherwise subject.

Accordingly we find that the study of fluid motion formed one of the very earliest branches of philosophy, and has ever since held its place, no subject having occupied the attention of mathematicians more closely. The results have been, in one sense, very successful; most important methods of reasoning have been developed, mathematical methods, which have helped to reveal numberless truths in other departments of science, and have taught us many things about fluids which most certainly we should not otherwise have found out, and of which we may some day find the application. But as regards the direct object in view, the revelation of the actual motion of fluids, the research has completely failed. And now that generations of mathematicians have passed away, now that the mysteries of the motions of the heavenly bodies, of the earth itself, and almost of every piece of solid matter on the earth have been explained by mathematicians, the simplest problems of fluid motion are yet unsolved.

If we draw a disc flatwise through the water, we know by a process of unconscious geometrical reasoning that the water must move round the disc; but by no known mathematical process could the motion be ascertained from the laws of motion. If we draw the plate obliquely through the water we experience a greater pressure on the one side than on the other. Now this case, representing as it does the principle of action of the screw propeller, is of the very highest importance to us; and yet, great as has been the research, it has revealed no law by which we may in a given case calculate the resistance to be obtained, or indeed tell from elementary principles in what way the water moves to let the plate pass. Again, the determination of the resistance which solid bodies, such as ships, encounter, is of such exceeding economic importance, that theory, as shipbuilders call it, having failed to inform them what to expect, efforts have been, and are still being made to ascertain the laws by direct experiment. Instances might be multiplied, but one other must suffice. If we send a puff of fluid into other fluid we know that it will travel to a considerable distance, but the manner in which it will travel and the motion it will cause in the surrounding fluid, mathematics has not revealed to us.

Now the reasons why mathematicians have thus been baffled by the internal motions of fluids appear to be very simple. Of the internal motions of water or air we can see nothing. On drawing the disc through the water there is no evidence of the water being in motion at all, so that those who have tried to explain these results have had no clue; they have had not only to determine the degree and direction of the motion, but also its character.

But although the want of a clue to the character of the motion may explain why so little has been done, it is not so easy to understand how it is that no attempts were made to obtain such a clue. It would seem that a certain pride in mathematics has prevented those engaged in these investigations from availing themselves of methods which might reflect on the infallibility of reason.

Suggestions as to the means have been plentiful. In other cases where it has been necessary to trace a particular portion of matter in its wanderings amongst other exactly similar portions, ways have been found to do it. It may be argued that the influences which determine the path of a particular portion of water are slight, subtle, and uncertain, but not so much so as those which determine the path of a sheep. And yet thousands of sheep belonging to different owners, have been from time immemorial turned loose on the mountains, and although it probably never occurred to anyone to reason out the paths of his particular sheep, they have been easily identified by the aid of a little colour. And that the same plan might be pursued with fluids, every column of smoke has been evidence.

But these hints appear to have been entirely neglected, and it was left for nature herself, when, as it were, fully satisfied with having maintained her secret so long, and tired of throwing out hints which were not taken, at last to divulge the secret completely in the beautiful phenomenon of the smoke ring. At last; for the smoke ring is probably a phenomenon of modern times. The curls of smoke, as they ascend in an open space, present to the eye a hopeless entanglement; and although, when we know what to look for, we can see as it were imperfect rings in almost every smoke cloud, it is rarely that anything sufficiently definite is formed to attract attention, or suggest anything more important than an accidental curl. The accidental rings, when they are formed in a systematic manner, come either from the mouth of a gun, the puff of a steam engine, or the mouth of a smoker, none of which circumstances existed in ancient times.

Although, however, mathematicians can in no sense be said to have discovered the smoke ring, or the form of motion which it reveals, they were undoubtedly the first to invest it with importance. Had not Professor Helmholtz some twenty years ago called attention to the smoke ring by the beautiful mathematical explanation which he gave of its motion, it would in all probability still be regarded as a casual phenomenon, chiefly interesting from its beauty and rarity. Following close on Helmholtz came Sir William Thomson, who invested these rings with a transcendental interest by his suggestions that they are the type after which the molecules of solid matter are constituted.

The next thing to enhance the interest which these rings excited, was Professor Tait's simple and perfect process of producing them at will, and thus rendering them subjects for lecture-room experiments. Considering that this method will probably play a great part in perfecting our notions of fluid motion, it is an interesting question how Professor Tait came to hit upon it. There is only one of the accidental sources of these rings which bears even a faint resemblance to this box, and that is the mouth of a smoker as he produces these rings. This might have suggested the box to Professor Tait. But since this supposition involves the assumption that Professor Tait sometimes indulges in a bad habit, and as we all know that Professor Tait is an eminent mathematician, perhaps we ought rather to suppose that he was led to his discovery by some occult process of reasoning which his modesty has hitherto kept him from propounding.

But however this may be, his discovery was a most important one, and by its means the study of the actual motion of these rings has been carried far beyond what would otherwise have been possible.

But it has been for their own sake, and for such light as they might throw on the constitution of matter, that these rings were studied. The most important lesson which they were capable of teaching still remained unlearned. It does not appear to have occurred to anyone that they were evidence of a general form of fluid motion, or that the means by which these had been revealed, would reveal other forms of motion.

There was, however, at least one exception, which will not be forgotten in this room: the use of smoke to show the effect of sound upon jets of air.

Also, the late Mr Henry Deacon, in 1871, showed that minute vortex rings might be produced in water by projecting a drop of coloured water from a small tube. And his experiments, in spite of their small scale, excited considerable interest.

Four years ago, being engaged in investigating the action of the screw propeller, and being very much struck by the difference between some of the results he obtained and what he had been led to expect, the author made use of colour to try and explain the anomalies, when he found that the vortex played a part in fluid motion which he had never dreamt of; that, in fact, it was the key to almost all the problems of internal fluid motion. That these results were equally new to those who had considered the subject much more deeply than he had, did not occur to him until after some conversation with Mr Froude and Sir William Thomson.

Having noticed that the action of the screw propeller was greatly affected when air was allowed to descend to the blades, he was trying what influence air would have on the action of a simple oblique vane, when a very singular phenomenon presented itself. The air, instead of rising in bubbles to the surface, ranged itself in two long horizontal columns behind the vane. There was evidence of rotational motion about these air lines. It was evident, in fact, that they were the central lines of two systematic eddies.

That there should be eddies was not surprising, but eddies had always been looked upon as a necessary evil which besets fluid motion as sources of disturbance, whereas here they appeared to be the very means of systematic motion.

Here then was the explanation of the nature of the motion caused by the oblique vane, a cylindrical band of vortices continually produced at the front of the plate, and falling away behind it in an oblique direction.

The recognition of the vortex action caused behind the oblique vane, suggested that there might be similar vortices behind a disc moving flatwise through the water, such as are the eddies caused by a teaspoon.

There was one consideration, however, which at first seemed to render this improbable. It was obvious that the resistance of the oblique vane was caused in producing the vortices at its forward part; so that if a vortex were formed behind a flat plate, as this vortex would remain permanently behind, and not have to be continually elongated, the resistance should diminish after the plate was once set in motion; whereas experience appeared to show that this was by no means the case. It appeared probable, therefore, that from some disturbing cause the vortex would not form, or would only form imperfectly, behind the plate.

This view was strengthened when, on trying the resistance of a flat plate, it did not appear to diminish after the plate had been started.

Accidentally, however, it was found that if the float to which the plate was attached was started suddenly and then released, the float and plate would move on apparently without any resistance. And more than this, for if the float were suddenly arrested and released, it would take up its motion again, showing that it was the water behind that was carrying it on.

There was evidence therefore of a vortex behind the disc. In the hope of rendering this motion visible, coloured water was injected in the neighbourhood of the disc, and then a beautiful vortex ring, exactly resembling the smoke ring, was seen to form behind the disc. If the float were released in time, this ring would carry the disc on with it; but if the speed of the disc were maintained uniform, the ring gradually dropped behind and broke up. Here then was another part played by the vortex previously undreamt of.

That the vortex takes a systematic part in almost every form of fluid motion was now evident. Any irregular solid moving through the water must from its angles send off lines of vortices such as those behind the oblique vane. As we move about we must be continually causing vortex rings and vortex bands in the air. Most of these will probably be irregular, and resemble more the curls in a smoke cloud than systematic rings. But from our mouths as we talk we must produce numberless rings.

One way in which rings are produced in perhaps as great numbers as from our mouths is by drops falling into the sea. If we colour the surface of a glass vessel full of water, and then let drops fall into it, rings are produced, which descend sometimes as much as two or three feet.

But the most striking rings are those produced in water, in a manner similar to that in which the smoke rings are produced, using coloured water instead of smoky air.

These rings are much more definite than smoke rings, and although they cannot move with higher velocities, since that of the smoke ring is unlimited, the speed at which they move is much more surprising.

In the air we are accustomed to see objects in rapid motion, and so far as our own notions are concerned, we are unaware of any resistance; but this is quite otherwise in water. Every swimmer knows what resistance water offers to his motions, so that when we see these rings flash through the water we cannot but be surprised. Yet a still more striking spectacle may be shown, if, instead of coloured water, a few bubbles of air be injected into the box from which the puff is sent; a beautiful ring of air is seen to shoot along through the water, showing, like the lines of air behind the oblique vane, little or no tendency to rise to the surface.

Such is the ease with which these vortex rings in water move, and so slight is the disturbance which they cause in the water behind them, as to lead to the conclusion that they experience no resistance whatever, except perhaps a little caused by slight irregularities in their construction. Their velocity gradually diminishes; but this would appear to be accounted for by their growth in size, for they are thus continually taking up fresh water into their constitution, with which they have to share their velocity. Careful experiments have confirmed this view. It is found that the force of the blow they will strike is nearly independent of the distance of the object struck from the orifice.

The discovery of the ring behind the disc afforded the opportunity of observing the characteristics of these rings much better than was afforded by the smoke rings; and also suggested facts which had previously been overlooked. The manner of motion of the water which formed the ring, and of the surrounding water, was very clearly seen. It was at once seen that the visible ring, whether of coloured water or air, was merely the central line of the vortex; that it was surrounded by a mass of coloured water, bearing something like the same proportion to the visible ring, as a ball made by wrapping string (in and out) round a curtain ring until the aperture was entirely filled up. The disc, when it was there, formed the front of this ball or spheroid of water, but the rest of the surface of the ball had nothing to separate it from the surrounding water but its own integrity. Yet when the motion was very steady the surface of the ball was definite, and the entire moving mass might be rendered visible by colour. The water within the ball was everywhere gyrating round the central ring, as if the coils of string were each spinning round the curtain ring as an axis, the water moving forwards through the interior of the ring and backwards round the outside, the velocity of gyration gradually diminishing as the distance from the central ring increased.

The way in which the water moves to let the ball pass can also be seen, either by streaking the water with colour or suspending small balls in it. In moving to get out of the way and let the ball of water pass, the surrounding water partakes as it were of the gyrating motion of the water within the *ball*, the particles moving in a horse-shoe fashion, so that at the actual surface of the *ball* the motion of the water outside is identical with that within, and there was no rubbing at the surface, and consequently no friction.

The maintenance of the shape of the moving mass of water against the unequal pressure of the surrounding water, as it is pushed out of the way, is what renders the internal gyratory motion essential to a mass of fluid moving through a fluid. The centrifugal force of this gyratory motion is what balances the excess of pressure of the surrounding water in the front and rear of the ball, compared with what it is at the sides.

It is impossible to have a ring in which the gyratory motion is great, and the velocity of progression slow. As the one motion dies out so does the other, and any attempt to accelerate the velocity of the ring by urging forward the disc, invariably destroyed it.

The striking ease with which the vortex ring, or the disc with the vortex ring behind it, moves through the water, naturally raised the question as to why a solid should experience resistance. Could it be that there was something in the particular spheroidal shape of these balls of water which allowed them to move freely. To try this, a solid of the same shape as the fluid ball was constructed and floated after the same manner as the disc. But when this was set in motion, it stopped directly—it would not move at all. What was the cause of this resistance? Here were two objects of the same shape and weight, the one of which moved freely through the water, and the other experienced very great resistance. The only difference was in the nature of the surface. As already explained, there is no friction at the surface of the water, whereas there must be friction between the water and the solid. But it could be easily shown that the resistance of the solid is much greater than what is accounted for by its surface friction or skin resistance. The only other respect in which these two surfaces differ is that the one is flexible, while the other is rigid, and this seems to be the cause of the difference in resistance.

If ribbons be attached to the edge of the disc, these ribbons will envelope the ball of water which follows it, presenting a surface which may be much greater than that of the solid; and yet this, being a flexible surface, the resistance of the disc with the vortex behind it is not very much greater than it would be without the ribbons—nothing to be compared to that of the solid.

Colouring the water behind the solid shows, that instead of passing through the water without disturbing it, there is very great disturbance in its wake. An interesting question is as to whether this disturbance originates with the motion of the solid, or only after the solid is in motion. This is settled by colouring the water immediately in front of the solid before it is started. Then on starting it the colour is seen to spread out in a film entirely over the surface of the solid, at first without the least disturbance, but this follows almost immediately.

Among the most striking features of the vortex rings, is their apparent elasticity. When disturbed they not only recover their shape, but vibrate about their mean position like an elastic solid. So much so, as to lead Sir William Thomson to the idea that the elasticity of solid matter must be due to its being composed of vortex rings.

But apart from such considerations, this vibration is interesting as showing that the only form of ring which can progress steadily is the circular. Two parallel bands, such as those which follow the oblique vane, could progress if they were infinitely long, but if not, they must be continually destroyed from the ends. Those which follow the oblique vane are continually dying out at one end, and being formed again at the other.

If an oval ring be formed behind an oval plate, the more sharply curved parts travel faster than the flatter parts; and hence, unless the plate be removed, the ring breaks up. It is possible, however, to withdraw the plate, so as to leave the oval ring, which proceeds wriggling along, each portion moving in a direction perpendicular to that in which it is curved, and with a velocity proportional to the sharpness of the curvature. So that not only does the ring continually change its shape, but one part is continually falling behind, and then overtaking the other.

These were some of the forms of fluid motion which imagination or reason had failed to show us, but which had been revealed by the simple process of colouring the water.

Now that we can see what we are about, mathematics can be most usefully applied; and it is expected that when these facts come to be considered by those best able to do so, the theory of fluid motion will be placed on the same footing as the other branches of applied mechanics.

26.

ON THE INVESTIGATION OF THE STEERING QUALITIES OF SHIPS.

[*From the* "British Association Report," 1876.]

THE primary object of using steam power in ships is to enable them to pass quickly over long distances. Under normal circumstances rapidity and certainty in manœuvring are matters of secondary importance; but circumstances do arise under which these powers are of vital importance. Experience has taught those who go down to the sea in steam-ships that their greatest danger is that of collision; and fogs are feared much more than storms. That there must always be danger when long ships are driven at full speed through crowded seas in a dense fog cannot be doubted; but this danger is obviously increased manyfold when those in command of the ships are under the impression that a certain motion of the helm will turn the ship in the opposite direction to that in which it does turn.

The uncertainty which at present exists in the manœuvring of large ships is amply proved by the numerous collisions which have occurred between the ships of our own navy while endeavouring to execute ordinary movements under the most favourable circumstances, and with no enemy before them. These accidents may be, and have been, looked upon as indicating imperfections in the ships or the manner in which they were handled; but it must be admitted that the ships are the best and best found in the world, and that they are commanded by the most skilful and highly trained seamen alive. And if peaceable ships fail in their manœuvres when simply trying not to hurt each other, what will be the case of fighting ships when trying to do all they can to destroy each other? If the general impression as to the important part which the ram is to play in the naval combats of the future is ever realized, then certainty in manœuvring must not only be of very great importance (this it has always been in sea fights), but it must occupy the very first place in the fighting qualities of the ship.

Now the results of the investigation of the effect of reversing the propellers on the action of the rudder appear to show that, however capricious the behaviour of ships has hitherto seemed, it is in reality subject to laws; and that by a series of careful trials the commander of a ship may inform himself how his ship will behave under all circumstances.

The experiments of the Committee on large ships have completely established the fact to which it was my principal object last year to direct attention, namely, that the reversing of the screw of a vessel with full way on very much diminishes her steering-power, and reverses what little it leaves; so that where a collision is imminent, to reverse the screw and use the rudder as if the ship would answer to it in the usual manner is a certain way of bringing about the collision. And to judge from the accounts of collisions, this is precisely what is done in nine cases out of ten. In the paper of to-day I find the following (August 22, 1876):—

"*The Fatal Collision off Ailsa Craig.*—The Board of Trade inquiry into the collision between the steamer 'Owl' and the schooner-yacht 'Madcap' was continued at Liverpool yesterday. Two passengers by the 'Owl' were recalled, and spoke to some of the facts of the collision. The night was not misty, though some rain had fallen. They saw the green light of the yacht shining brightly after the collision. William Maher, third officer of the 'Owl,' said it was the chief officer's watch at the time of the collision. There were five able seamen in the watch. Witness and the chief officer were on the bridge. One man was on the look-out from the starboard side of the bridge. His ordinary place was on the forecastle-head, but he was not placed there that night, as there was a heavy head sea, and the vessel was shipping water. His attention was called to a light by the look-out man. It was almost ahead about a mile and a half off. He could not at first distinguish whether it was red or green, as it was dim; but when he made it out to be a green light it bore two to three points on the port bow, and it was only three or four lengths off. He heard no order given to the man at the wheel when the light was first reported; but when witness found that it was a green light he ordered the helm hard aport. If the steamer had starboarded at this time she would have gone right over the yacht. The 'Owl' had been going at the rate of six or seven knots; but when she collided there was no way on her, the engines having been reversed. After the yacht went down the captain ordered a boat to be got out, but subsequently countermanded the order, on the ground that more lives would be lost, as it was not fit to go out. At the close of his examination the witness stated that he would not have gone out in a boat on such a night as that, even if the captain had ordered him—a remark which appeared to greatly astonish the nautical assessors."

He ported his helm to bring his ship round to starboard, but he also reversed his screw; and as he says nothing about having again starboarded his helm, it would appear that from the time of reversing the screw until the collision (time enough to stop the ship), she had moved straight forward or inclined to port. Had he not reversed his screw, but kept on full speed, it is clear the collision could not have happened, for at the time the collision did happen his ship would have been more than her own length away from the spot where the collision occurred. He admitted himself that to have starboarded his helm must have brought about the collision, so he ported his helm and reversed his screw, which, as it had the same effect, did bring about the collision.

From the Committee's report just read, it appears that a ship will turn faster, and for an angle of 30°, in less room when driving full speed ahead, than with her engines reversed, even if the rudder is rightly used. Thus when an obstacle is too near to admit of stopping the ship, then, as was done in the case of the 'Ohio,' mentioned in my paper last year, the only chance is to keep the engines on full speed ahead, and so to give the rudder an opportunity of doing its work.

These general laws are of the greatest importance, but they apply in different degrees to different ships; and each commander should determine for himself how his ship will behave. A ship's ordinary steering-power may soon be learnt in general use, but not so the effect of stopping; there is thought to be a certain risk in suddenly reversing the engines, which anyone in charge of a ship will shrink from, unless he knows it is recognized as part of his duty.

It is also highly important that the effect of the reversal of the screw should be generally recognized, particularly in the law courts; for in the present state of opinion on the subject, there can be no doubt that judgment would go against any commander who had steamed on ahead, knowing that by so doing he had the best chance of avoiding a collision, or who had ported his helm in order to bring his ship's head round to port, with the screw reversed. It seems to me, therefore, that it would be well if steps could be taken by this Association to bring the matter prominently before the Admiralty, the Board of Trade, and those concerned in navigation.

So far as the capabilities of each individual ship are concerned, there is no insuperable difficulty or risk about the experiments, and to have determined these will be a great point. When the officers know exactly what can be done in the way of turning their ships, and how to do it, the chances of accidents must be greatly reduced.

But at all events for fighting ships it is desirable that the officers should have experience beyond the mere turning powers of their own ships. When two ships are manœuvring so as to avoid or bring about a collision, each commander has to take into account the movements of his opponent. To enable him to do this with readiness, it would be necessary to have friendly encounters. A fight between two ships whose captains had never before fought, would be like a tournament between two novice knights who had never practised with pointless spears; and such a contest, although not unequal, must be decided by chance rather than skill.

Unfortunately sham fights or tournaments between ships with blunt rams would be about as dangerous as a real fight; and the chance of an accident would be far too great for such friendly tournaments, however important, ever to become an essential part of the training of a naval officer, as they were of the knights of old. For although, should war arise, the danger from want of experience may be even greater than the danger of an accident in gaining such experience by friendly fights, yet, as the chance of war is always remote, the former risk would be preferred; and this is not all.

As yet there has been no such thing as a ramming fight between steamships; so that not only are our officers without actual experience, but even the rules by which they are instructed to act (the rules of naval tactics) are based entirely on theoretical considerations, and hence are very imperfect.

Now there appears to me to be a means by which experience of the counter-manœuvring powers of ships, as well as the manœuvring powers of single ships, could be ascertained without any of the risk and but little of the cost attending on the trials of large ships, and which, if not equal to an actual fight, would be very useful as a means of training the officers.

If small steam-launches were constructed similar to the ships, so that they represented these ships on a given scale (say one-tenth linear measure), and their engines were so adjusted that they could only steam at what we may call the speed corresponding to that of the larger ships, then two launches would manœuvre in an exactly similar manner to the large ships, turning in one-tenth the room; and the time which the manœuvres with the launches would take would only be about half that occupied by similar manœuvres with full-sized ships. The only points in which it would be necessary that the model should represent the ship would be in its shape under water, and as regards the longitudinal disposition of its weights. The centre of gravity should occupy the same position amidships, and the longitudinal radius of gyration of the model should bear the same proportion to that of the ship as the other linear dimensions. In other respects the model might be made as was most convenient. It might be made of wood, and so strengthened that two models might run into each other with impunity.

There would not be much difficulty in so strengthening the models, as the speed of the models would be very small. For instance, if the speed of the ship were $13\frac{1}{2}$ knots, then that of the model would be $4\frac{1}{2}$ knots.

The study of the qualities of ships from experiments on their models has not until recent years led to any important results. But this in great part was owing to the fact that proper account had not been taken of the effect of the wave caused by the ship and the consequent resistance. It was not known that the waves set up by the model bear the same relation to the size of the model as the waves set up by the ship do to the ship when, and only when, the speed of the model is to the speed of the ship in the ratio of the square root of the ratio of their lengths.

Since this fact has been recognized, most important information has been obtained by experimenting on models. Mr Froude, by recognizing this law, has been able to bring the comparison of ships by means of their models to such a degree of perfection, that he can now predict with certainty the comparative and actual resistance of ships before they are constructed, and the great practical value of his results have been recognized by the Admiralty.

What I propose is virtually to extend these experiments on models so as to make them embrace the steering-powers of ships as well as their resistances. The manner of experimenting would have to be somewhat altered. Steam-launches would have to be substituted for dummy models; but the principle of the experiments would have to remain the same, and the speed of the launches must be regulated by the same law as that of the models.

The turning qualities of such launches might be verified by comparing them with the turning qualities of the ships as found by actual experiment; and then the models might be handed over to the officers of the ships, and they might practise encounters and manœuvres until they knew not only what they could do with their ships, but what it was best to do in order to outmanœuvre each other, and this without any cost or risk.

The behaviour of the models would be in all respects similar to that of the ships, the only difference being that the manœuvres would be on a smaller scale; and the scale of the manœuvres would be the same as that of the models, so that the step from the models to the large ships would be easy; and familiarity with the working of the ships as well as the models under ordinary circumstances would prepare the officers for using the ships in an actual fight as they have been accustomed to use the models in their friendly encounters. The scheme here proposed has its parallel in military schools.

Although "autumn manœuvres" and sham fights afford soldiers a much better opportunity of preparing themselves for battle than anything at present within reach of the sailors, still the war game appears to be growing in favour, and this is nothing more than practising manœuvres in miniature.

Independently of their value as a means of training naval officers, such models would afford a means of studying naval tactics. From them might be learnt the way in which a ship should strive to approach another of nearly equal power and speed, so as to use her ram to the greatest advantage; and of this as yet but very little can be known; and, except on models, it can only be learnt from experiments on the ships.

Important as are the laws which have been verified by the Committee on the steering of screw-steamers, it appears to me that the most important lesson to be learnt from their investigation is, that there is nothing capricious in the behaviour of these ships. To realize the value of this lesson the investigation must be followed up; and it appears that the best way to do this would be by the aid of model launches on the plan thus roughly sketched out.

For continuation see p. 204.

27.

ON THE RATE OF PROGRESSION OF GROUPS OF WAVES AND THE RATE AT WHICH ENERGY IS TRANSMITTED BY WAVES.

[*From* "Nature," *Aug.* 23, 1877.]

(*Read before* Section A of the British Association, 1877.)

WHEN several waves forming a discontinuous group travel over the surface of deep water, the rate of progression of the group is always much less than the rate at which the individual waves which compose the group are propagated.

As the waves approach the front of the group they gradually dwindle down and die out, while fresh waves are continually arising in the rear of the others. This, which is a well-known phenomenon, presents itself to our notice in various ways.

When a stone is thrown on to the surface of a pond, the series of rings which it causes gradually expands so as finally to embrace the entire surface of the water; but if careful notice be taken it is seen that the waves travel outwards at a considerably greater rate than that at which the disturbance spreads.

Or, when viewing a rough sea, if we endeavour to follow with the eye any wave which is larger than its neighbours, we find, after following it in its course for a short distance, that it has lost its extra size, while on looking back we see that this has been acquired by the succeeding wave.

But perhaps the most striking manifestation of the phenomenon is in the waves which spring from the bows of a rapid boat, and attend it on its course. A wave from either bow extends backwards in a slanting direction for some distance and then disappears; but immediately behind it has come into existence another wave parallel to the first, beyond which it extends for some distance when it also dies out, but not before it is followed by a third which extends still farther, and so on, each wave overlapping the others rather more than its predecessor. Although not obvious, very little consideration serves to show that the stepped form of these columns of waves, is a result of the continual dying out of the waves in the front of the group, and the formation of fresh waves behind. For as each wave cuts slantwise through the column formed by the group, one end is on the advancing side or front of the group, and this is continually dying, while the other is in the rear, and is always growing.

So far as I am aware, no general explanation of these phenomena has as yet been given. It has been shown, and I believe first by Prof. Stokes, that if two series of parallel waves of equal magnitude, but differing slightly in length, move simultaneously in the same direction over the same water so as to form a series of groups of waves separated by bands of interference, that these groups will advance with half the velocity of the individual waves. This is doubtless an example of the same phenomenon, and shows that the theory of wave motion is capable of explaining the phenomena; but it appears to leave something to be desired,—for instance, why should the bands of interference only progress with half the velocity of propagation in a deep sea, whereas in sound the corresponding bands of interference which constitute the beats move at the same velocity as the waves?

My object in this paper is to point out a fact in connection with wave transmission which appears to have hitherto passed unnoticed, at all events in connection with the phenomena described above, of which it affords a clear and complete explanation. One of the several functions performed by waves progressing through a medium, is the transmission of energy. Thus the energy which we receive from the sun is brought to us in the waves of light and heat; so in the case of sound the work done by the arm of the drummer is transmitted to our ears by the waves of sound. It is possible, however, to have waves which travel through a medium without conveying energy; such are the waves caused by the wind on a field of corn. This kind of wave may be well understood by suspending a series of small balls by threads, so that the balls all hang in a row, and the threads are all of the same length. If we then run the finger along, so as to set the balls oscillating in succession, the motion will be such as to give the idea of a series of waves propagated from one end to the other; but in reality there is no propagation, each pendulum swings independently of its neighbours, there is no communication of energy, the waves being merely the result of the general arrangement of the motion.

In this case there is no communication of energy, neither is there any propagation of disturbance. Any one ball may be set swinging without in the least disturbing the others; and what is indicated here is a general law that wherever a disturbance is transmitted through a medium by waves, there must always be communication of energy. The rate at which energy is transmitted in different media, or by different systems of waves, is very different. This may be illustrated at once by experiment. If the balls just described are all connected by an elastic thread, then they can no longer swing independently. If one be set in motion, then, by virtue of the connecting thread, it will communicate its motion to its neighbours until they swing with it, so that now waves would be propagated through the balls. The rate at which a ball would impart its motion, *i.e.* its energy, to its neighbours, would clearly depend on the tension of the connecting thread. If this was very slight compared with the weight of the balls, it would stretch, and the ball might accomplish several swings before it had set its neighbours in full motion, so that of the initial energy of disturbance a very small portion is communicated at each swing. But if the tension of the thread be great compared with the weight of the balls, one ball cannot be disturbed without causing a similar disturbance in its neighbours, and then the whole energy will be communicated. This is simply illustrated by laying a rope or chain on the ground, and fastening down one end; if then the loose end be shaken up and down the wriggle caused will travel to the other end, leaving the rope perfectly straight and quiet on the ground behind it, so that in this case it is at once seen that the wave carries forward with it the whole energy of the disturbance.

The straight cord and the pendulous balls represent media in which the waves are at the opposite limits; in one case none of the energy of disturbance is transmitted, and in the other case the whole is transmitted. Between these two limits we may have waves of infinite variety, in which any degree of energy from all to nothing is transmitted. Now the waves of sound belong to the class of the cord in which all the energy is transmitted; but what I want particularly to make clear, is that when the waves on water are between the limits, they are analogous to the waves in the balls suspended when connected by an elastic string. And I have so to show that according to the accepted theory of wave motion the waves on deep water only carry forward half the energy of disturbance.

In regular trochoidal waves the particles move in vertical circles with a constant velocity, and are always subject to the same pressure. Of the energy of disturbance half goes to give motion to the particles, and half to raise them from their initial position to the mean height which they occupy during the passage of the wave.

Now the mean horizontal positions of the particles remain unaltered by the waves, hence, since their velocities are constant, none of their energy of motion is transmitted; nor since the pressure on each particle is constant, can any energy be transmitted by pressure. The whole energy, therefore, which remains to be transmitted, is the energy due to elevation, and that this is transmitted is obvious, since the particles are moving forward when above their mean position, and backwards when below it. This energy constitutes half the energy of the disturbance, and this is, therefore, the amount transmitted.

For a definite mathematical proof that

In waves on deep water the rate at which the energy is carried forward is half the energy of disturbance per unit of length multiplied by the rate of propagation.

Let h_0 be the initial height occupied by a particle supposed to be of unit weight, h_1 the height of the centre of the circle in which it moves as the wave passes, r the radius of the orbit, and θ the angle the radius vector makes with the horizontal diameter, then the height of the particle above its initial position is $h_1 - h_0 + r\sin\theta$; adding to this height due to its velocity, we have the whole energy of disturbance

$$= 2(h_1 - h_0) + r\sin\theta.$$

The velocity of the particle is

$$\sqrt{2g(h_1 - h_0)},$$

and the horizontal component of this is

$$\sqrt{2g(h_1 - h_0)}\sin\theta.$$

Therefore the rate at which energy is being transmitted by the particle is

$$\{2(h_1 - h_0) + r\sin\theta\}\sqrt{2g(h_1 - h_0)}\sin\theta,$$

and the mean of this is

$$\frac{1}{2\pi}\int_0^{2\pi}\{2(h_1 - h_0) + r\sin\theta\}\sqrt{2g(h_1 - h_0)}\sin\theta.d\theta$$
$$= \tfrac{1}{2}r\sqrt{2g(h_1 - h_0)},$$

and if λ be the length of the wave and $n\lambda$ the rate of propagation

$$h_1 - h_0 = \frac{\pi r^2}{\lambda}, \text{ and } \frac{2g}{\lambda} = 4\pi r^2.$$

Therefore the mean rate at which energy is transmitted by this particle

$$= n\lambda(h_1 - h_0),$$

or the rate of propagation multiplied by half the energy of disturbance.

[Q. E. D.

It now remains to come back to the speed of the groups of waves, and to show that: *if the rate at which energy is transmitted is equal to the rate of propagation multiplied by half the energy of disturbance, then the velocity of a group of waves will be half that of the individual waves.*

Let P_1, P_2, P_3, P_4 be points similarly situated in a series of waves which gradually diminish in size and energy of disturbance from P_3 to P_1, in which direction they are moving. Let E be the energy of disturbance between P_1 and P_2 at time t, $E+a$ the energy between P_2 and P_3, $E+2a$ between P_3 and P_4, and so on.

Then at the time $t+n$ after the wave has moved through one wave-length, it follows that the energy between P_1 and P_2 will be

$$\frac{E+E+a}{2}=E+\frac{a}{2},$$

and between P_2 and P_3 will be

$$=\frac{E+a+E+2a}{2}=E+\frac{3a}{2};$$

and again, after another interval n, the energies between P_1 and P_2, P_2 and P_3 will be respectively

$$\frac{E+\frac{a}{2}+E+\frac{3a}{2}}{2}=E+a,$$

and

$$\frac{E+\frac{3a}{2}+E+\frac{5a}{2}}{2}=E+2a.$$

So that after the waves have advanced through two wave-lengths, the distribution of the energy will have advanced one, or the speed of the groups is half that of the waves. [Q. E. D.

Of course this reasoning applies equally to the waves on the suspended balls, when connected by an elastic string, as to water; and in this case the conclusions may be verified, for, as on water, the groups of waves travel at a slower rate than the waves. This experiment tends to throw light on the manner in which the result is brought about. When a ball is disturbed, the disturbance is partly communicated to the adjacent ball by the connecting string, and part retained in the form of pendulous oscillation; that part which is propagated forward, is constantly reduced in imparting oscillations to the successive balls, and soon dies out, while the motion retained by the swinging pendulum, constantly gives rise to succeeding waves until it is all absorbed. If the tightness of the cord be adjusted to the length of the suspending threads, waves may be made to travel along in a manner closely resembling the way in which they travel on water, the speed of the group being half the speed of the individual waves.

Although the progression of a group has hitherto been spoken of as if the form of the group was unaltered, this is by no means the case as a rule.

In the mathematical investigation it was assumed that the motion of the particles is circular; this, however, cannot be the case when the succeeding waves differ in size by a sensible quantity, and hence in this case the form of the group cannot be permanent. And it may be further shown, that as a small group proceeds, the number of waves which compose it will continually increase, until the graduation becomes indefinitely small; and this is exactly what is observed, whether on water or on the strings.

So far as we have considered deep water, when the water is shallow compared with the length of the waves, the results are modified, but in this case the results as observed are strictly in accordance with the theory.

According to this, as waves enter shallow water, the motion of the particles becomes elliptical, the eccentricity depending on the shallowness of the water; and it may be shown that under these circumstances, the rate at which energy is transmitted is increased, until, when the elliptic paths approach to straight lines the whole energy is transmitted, and consequently it follows that the rate of the speed of the groups to the speed of the waves will increase as the water becomes shallower, until they are sensibly the same. In which case only the groups of waves are permanent, and Mr Scott Russell's solitary wave is possible. Besides the explanation thus given of these various phenomena, it appears that we have here a means of making some important verifications of the assumptions on which the wave theory is based; for the relative speed of the groups, and the waves which compose them, affords a criterion as to whether or not the particles move in circles.

28.

ON THE EFFECT OF PROPELLERS ON THE STEERING OF VESSELS.

[*From the* "British Association Report," 1877.]

Report of the Committee, consisting of JAMES R. NAPIER, F.R.S., Sir W. THOMSON, F.R.S., W. FROUDE, F.R.S., J. T. BOTTOMLEY, *and* OSBORNE REYNOLDS, F.R.S. (Secretary), *appointed to investigate the Effect of Propellers on the Steering of Vessels.*

SINCE the meeting of the British Association held in Glasgow last year, the Committee has been able to carry out some further experiments on steering as affected by the reversing of the screw.

The largest vessel experimented upon last year was the barge No. 12, of about 500 tons, and it appeared, on comparing the behaviour of this vessel with the behaviour of those of smaller size, that the larger the ship the more important would the effect of reversing the screw become. This view has been completely borne out by the experiments of this year, made with one vessel of 850 tons and another of 3594 tons.

In May last the 'Melrose,' a new vessel belonging to Messrs Donald Currie & Co., was tried at the instance and under the superintendence of Mr James R. Napier. The 'Melrose' is 228 feet in length by 29 feet in breadth, and 16 feet 3 inches in depth. She is 850 tons gross register; her propeller makes 90 revolutions per minute with the vessel going at a speed of $10\frac{3}{4}$ knots.

The following is Mr Napier's report of the trials:—"These experiments were made on 3rd of May 1877, between Wemyss Bay and Rothsay. There was little or no wind; the sea was glassy smooth. The draft of water was 9 feet 1 inch forward, and 12 feet 5 inches aft; the diameter of the propeller was 11 feet 6 inches, the pitch 14 feet 3 inches, it had 4 blades and was right-handed. The maximum speed at the nautical mile was $10\frac{3}{4}$ knots; but the speed was about 10 knots when the trials were made.

"A trial was made with the rudder said to be amidships, and the ship's head turned to starboard; but it was found afterwards that the pointer on the bridge had been misplaced, and, as it was difficult at the time to ascertain the rudder's position, the result was uncertain.

"*First mock collision trial.*—The vessel was steaming about 10 knots when the telegraph bell warned the engineer to stand by his engines, and shortly after the bell was rung for him to reverse at full speed (no intermediate order to slow or stop being given); in 15 seconds after this order was given the engines began to reverse, and in 2 minutes 15 seconds after the giving of the order to reverse, the forward motion of the ship had entirely stopped.

"At the instant that the engineer below telegraphed to the captain on deck that his engines were reversing, the captain gave the order '*Hard aport,*' which was quickly obeyed by the two men at the wheel. *The vessel's head almost immediately commenced turning to port,* and when the ship's way was stopped, or about 2 minutes after the order to port was given, the vessel's head had turned 26 or 28 degrees to port.

"*Second mock collision trial.*—Everything was done in the same manner as in the first trial, except in this case the order was to *starboard hard.* The vessel's way was lost in about the same time. *The vessel's head commenced to turn to starboard almost immediately after the engines began to reverse,* and when the forward way was lost, her head had gone round 40° to starboard.

"These results were so contrary to the expectation of some of the nautical party on board, that they made a *third mock collision trial* (a second one with the helm *hard aport*); but on this occasion the orders to reverse the engines and to port the helm were given simultaneously. The result was similar to the first trial, the head turning a long way to port; but I was not on the bridge to note the angle through which her head moved before head-way was lost.

"Mr Currie, one of the owners of the ship, most of the nautical men and visitors on board learned, I think, something regarding the steering of screw-steamers, and a cause of some, if not of many, collisions which they did not know before. The Captain of the ship, however, when asked before the trials what would be the result of the sudden reversal of the engines, with the helm aport or starboard, stated the direction in which the ship's head would turn as it actually happened."

The Committee wish to thank Mr Currie for allowing them the use of his ship for the experiments.

It will be seen, from Mr Napier's report, that the 'Melrose' behaved in precisely the same way as did the vessels last year, except that the effect of the reversed screw on the action of the rudder was even more apparent than in the previous trials. This was obviously owing to the greater size of the ship, and the consequently greater time taken by the reversed screw in bringing her to rest, and the result led the Committee to conclude that with still larger ships the result would be yet more pronounced.

This conclusion has been verified in a somewhat unexpected although in a most satisfactory manner; for, after arriving at Plymouth, the Secretary received the following account of trials made in the s.s. 'Hankow,' of London, 3594 tons, by Captain Symmington, the commander, in response to the circular issued by the Committee last year, but otherwise at his own instance.

Capt. Symmington's Report.

"S.s. 'Hankow,' of London,
8th March, 1877.

"Gross tonnage 3594^{12}, net 2331^{75} tons.

"Length 389 feet, breadth 42·1, depth 28·8.

"Some experiments were conducted this forenoon from 9.20 A.M. to 11.20 A.M., in lat. 8° 50′ S., long. 153° 58′ E., in order to determine how the ship's head turned on reversing the engines suddenly when going full speed ahead with the helm amidships, port, and starboard; also the time and diameter of the circles made when going slow and full speed ahead on the port helm.

"Sea smooth or between No. 1 and 2 of the Beaufort scale; ship drawing, on leaving Sydney on the 28th ult., 26 feet forward and 24 feet 3 inches aft; to-day the probable draft will be 24 feet 8 inches forward and 23 feet 8 inches aft, mean 24·2.

"*First Experiment.*

"Ship going ahead full speed, engines were suddenly reversed, helm put hard aport; immediately the engines started, time noted and bearing of ship's head by standard (Admiralty compass) noted, and the bearing of the ship's head also noted at every 15 seconds until the ship came to a dead stop.

Time. A.M.			Interval		Ship's Head by Compass			Head turned to Port	Head turned to Starboard
h.	m.	s.	m.	s.		°		°	°
9	20	7		...	N.	62	W.		
		22		15	„	62½	„	0½	
		37		15	„	66	„	3½	
		52		15	„	69	„	3	
	21	7		15	„	78½	„	4½	
		22		15	„	77	„	3½	
		37		15	„	80	„	3½	
		52		15	„	84½	„	4	
	22	7		15	„	88	„	3½	
		22		15	„	88	„	Stationary	
		37		15	„	87	„	...	1
		52		15	„	85½	„	...	1½
	23	7		15	„	84	„	...	1½
		22		15	„	82½	„	...	1½
		37		15	„	79½	„	...	3
	3	30	3	30				26	8½

"Ship came to a dead stop in 3 min. 30 sec., and turned *to port* 26° in 2 min., then turned to starboard 8½° in 1 min. 30 sec.

"*Second Experiment.*

"Ship going ahead full speed, say 10 knots. The engines were suddenly reversed full speed astern, helm put hard astarboard; bearing of ship's head taken and time. At every 15 seconds the bearing of ship's head was also noted until the ship came to a dead stop.

Time. A.M.			Interval		Ship's Head by Compass			Head turned to Port	Head turned to Starboard
h.	m.	s.	m.	s.		°		°	°
9	45	30		...	N.	39	W.		
	45	45		15	„	41	„	2	
	46	0		15	„	41	„		
	46	15		15	„	39½	„	...	1½
	46	30		15	„	37½	„	...	2
	46	45		15	„	32½	„	...	5
	47	0		15	„	28	„	...	4½
	47	15		15	„	44½	„	...	3½
	47	30		15	„	21½	„	...	3
	47	45		15	„	18	„	...	3½
	48	0		15	„	13	„	...	5
	48	15		15	„	9	„	...	4
	48	30		15	„	5	„	...	4
	48	45		15	„	2½	„	...	2½
	48	53		8	„	2	„	...	0½
	3	23	3	23				2	39

"Ship came to a dead stop in 3 min. 23 sec. Her head payed off to port 2° during the first 15 sec., and afterwards turned to starboard 39° before coming to rest.

"*Third Experiment.*

"Ship going full speed ahead, say 10 knots, the engines were suddenly reversed, full speed astern, the helm put amidships, and the bearing of the ship's head noted by the standard azimuth compass (Admiralty) at every 15 seconds until the ship came to absolute rest. Wind and weather as before. Going full speed ahead 10 knots, reversed full speed astern, helm amidships.

Time. A.M.	Interval	Ship's Head by Compass	Head turned to Port	Head turned to Starboard
h. m. s.	m. s.	°	°	°
10 34 16	...	N. 39½ E.		
34 31	15	„ 29 „	0½	
34 46	15	„ 29½ „	...	0½
35 1	15	„ 30½ „	...	1
35 16	15	„ 32 „	...	1½
35 31	15	„ 36 „	...	4
35 46	15	„ 39 „	...	3
36 1	15	„ 44 „	...	5
36 16	15	„ 46½ „	...	2½
36 31	15	„ 48 „	...	1½
36 46	15	„ 50½ „	...	2½
37 1	15	„ 51½ „	...	1
37 16	15	„ 52 „	...	0½
37 31	15	„ 53½ „	...	1½
37 46	15	„ 54 „	...	0½
38 1	15	„ 54½ „	...	0½
38 16	15	„ 55 „	...	0½
38 31	15	„ 56 „	...	1
4 15	4 15		0½	27

"Ship came to absolute rest in 4 min. 15 sec., her head turned to port ½° and then 27° to starboard before coming to rest.

"*Fourth Experiment.*

"In this case the ship was going full speed astern, say about 9 knots,

Time. A.M.	Interval	Ship's Head by Compass	Head turned to Port	Head turned to Starboard
h. m. s.	m. s.	°	°	°
11 3 11	...	S. 65½ E.		
3 26	15	„ 66 „	0½	
3 41	15	„ 67 „	1	
3 56	15	„ 67½ „	0½	
4 11	15	„ 67½ „		
4 26	15	„ 66½ „	...	1
4 41	15	„ 65½ „	...	1
4 56	15	„ 63½ „	...	2
5 11	15	„ 60½ „	...	3
5 26	15	„ 57½ „	...	3
5 41	15	„ 53½ „	...	4
5 56	15	„ 48 „	...	5½
2 45	2 45		2	19½

when the engines were suddenly reversed to full speed ahead, helm put *hard to port,* time and direction of ship's head noted until the ship came to a dead stop. Sea, wind, and weather as before, viz. most favourable conditions for these trials.

"Ship came to a dead stop in 2 min. 45 sec., and her head turned 2° to port in the first 45 seconds and 19½ to starboard in the next 2 minutes.

"*Fifth Experiment.*

"Making the circle: *hard to port: full speed ahead.* Lat. 8° 50′ S., long. 153° 58′ E.

"Ship started full speed from a position of absolute rest, with the helm hard aport, and at the instant of starting an empty flour barrel was dropped from the stern to mark the point started from. Sea smooth or nearly so, between No. 1 and 2 of the Beaufort Scale. Wind very light, about No. 1 to 2.

Time. A.M.			Interval		Ship's Head by Compass			Arc turned
h.	m.	s.	m.	s.		°		°
9	27	54		...	N.	56½	W.	
	28	24	1	30	,,	54	,,	2½
	28	54		30	,,	49	,,	5
	29	24		30	,,	38	,,	11
	29	54		30	,,	28	,,	10
	30	24		30	,,	18	,,	10
	30	54		30	,,	5	,,	13
	31	24		30	N.	6	E.	11
	31	54		30	,,	19	,,	13
	32	24		30	,,	30	,,	11
	32	54		30	,,	43½	,,	13½
	33	24		30	,,	58	,,	14½
	33	54		30	,,	74	,,	16
	34	24		30	,,	89	,,	15
	34	54		30	S.	75	E.	16
	35	24		30	,,	61	,,	14
	35	54		30	,,	46½	,,	14½
	36	24		30	,,	33	,,	13½
	36	54		30	,,	20	,,	13
	37	24		30	,,	7⅛	,,	12½
	37	54		30	,,	4½	,,	12
	38	24		30	,,	16½	,,	12
	38	54		30	,,	30	,,	13½
	39	24		30	,,	45½	,,	15½
	39	54		30	,,	61	,,	15½
	40	24		30	,,	78½	,,	17½
	40	54		30	,,	84	,,	17½
	41	24		30	,,	67	,,	17
	41	40		16	,,	57	,,	10

"Ship completed the circle in 13 min. 46 sec., and came outside the barrel (point of starting), about 150 feet, when the barrel was abreast of the taffrail. That is, we had the barrel on our starboard side when circle was completed.

(Signed) "W. SYMMINGTON,
"Commander s.s. 'Hankow.'"

These experiments need no comment; they are conclusive as to the truth and importance of the results previously obtained; and the Committee thank Capt. Symmington for his report.

In answer to the request of the Committee, made last year, the Admiralty have caused experiments to be made as to the effect of reversing the screw on the steering of H.M.S. 'Speedy,' 273 tons, with a maximum speed of 5 knots an hour. The perusal of the extract of the report on these trials received by the Committee and appended to this report, shows at once that the conditions under which the experiments were made were such as to preclude the possibility of their throwing much light on the subject. The greatest speed of the vessel was 5 knots, and the effect of the rudder with the screw reversed was so small, that the vessel, in most instances, turned her forward end into the wind.

On the receipt of the report of these trials, a letter was written to the Admiralty, urging them to have experiments made with larger and more powerful ships, but as yet no further communication has been received.

In accordance with the resolution by which they were appointed, the Committee have communicated with the Admiralty, the Board of Trade, the Elder Brethren of the Trinity House, and other Corporations, and copies of the last year's report were forwarded as soon as they could be obtained; no intimation has yet been received of any action being taken by these bodies.

It appears, from an article in the *Nautical Magazine* of December, that the last report of the Committee was discussed at the conference of the Association for the Reform and Codification of the Law of Nations, held last year at the Ancient House, City of Bremen, when the following resolution was agreed to:—

"It is the opinion of the Conference that the existing international rules for preventing collisions at sea are not of a satisfactory character, and that it is desirable that the Governments of the maritime states should take counsel together with a view to amend these rules and to adapt them more carefully to the novel exigencies of steam navigation."

The article in the *Nautical Magazine* was written by Sir Travers Twiss, and in this and in a subsequent article he discusses the facts established by the Committee, and their bearing on the question of the alteration of the rule of the road at sea, pointing out the absolute necessity of modifying Article 15 of the Amended Board of Trade Steering and Sailing Rules, which are likely to become law.

These and other notices which have appeared in English and foreign publications show that the subject has already attracted considerable attention; and it is important to notice that in no way have the conclusions of the Committee been in the smallest degree controverted.

Numerous collisions have occurred during the year, which, to judge from the law reports, might in many instances have been avoided had the effect of reversing the screw been known and acted upon; but it does not appear as if a consideration of this has influenced any of the judgments given.

The collisions have for the most part been with small ships, and so have not attracted much attention; but the loss of the 'Dakota' was a disaster of the first magnitude, and if it was not due to the porting of the helm with the screw reversed it might have been, for as soon as the officers became aware of their extreme danger (the shore being on their port bow) the helm was put hard aport and the screw reversed full speed, after which, according to the evidence of Mr Jones, a pilot on board, the vessel turned to port until she struck. The evidence offered by the Secretary of the Committee was, however, rejected by the Commissioner of Wrecks (Mr Rothery), on the ground that the ship was virtually lost before the screw was reversed. It is to be noted, however, that the orders to reverse the engines and to port the helm were avowedly given in the hope of saving the ship, and that had there been a chance of escape, such action, as shown by all the experiments of the Committee, must most certainly have reduced it.

APPENDIX.

Extract from Report of Captain of Steam Reserve at Portsmouth, dated 24th January, 1877.

Experiments on the Turning of Screw Ships.

I have the honour to report that, as already reported in my letter, dated 30th September, 1876, to the Admiral Superintendent (through whom I received the original copy of experiments required), there have been no opportunities of making experiments on this subject, on account of ships going out on trial having their time fully occupied, and there have been no ships in the First Reserve which could be taken out for the purpose.

Observing, however, from the report in the *Nautical Magazine* referred to, that the largest vessel of which particulars of trial are given is only 80 tons, I took the 'Speedy,' of 273 tons, out and tried the experiments required with her: her speed is only about 5 knots; draught of water 7 feet 10 inches; rig one small mast forward; screw right-handed, Griffith's, two-bladed, diameter 6 feet 1 inch, pitch 6 feet. The results are given in attached sheet.

An opportunity also occurred of getting one trial of No. 6 in the 'Euphrates,' while waiting for tide. While going ahead the screw was stopped and reversed, the helm being kept amidships; the ship's head came steadily round to starboard (windward) 12° till head to wind, then fell off to port, and continued to do so till stern to wind. An experienced pilot (Mr Harding) who was with me told me beforehand that this would be the case.

The experiments with the 'Speedy' were conducted by myself, with the assistance of Staff-Commander Parker, and Mr Riley, chief gunner of 'Asia' for Reserve.

I think it may be taken as nearly certain that in all cases of putting the helm over and reversing the screw at the same time the ship will obey the helm for a limited time, the amount depending on the way the ship has, her rig, and the direction of the wind and sea with reference to her course, and that as she loses her way she will fall off from the wind until she brings it astern or nearly so. Also, that on reversing the engines with the helm kept amidships, she will come up head towards the wind, and then fall off before the wind as she loses her way.

It is going beyond the part of the article marked for my remarks, but I would venture to express an opinion that it would be highly undesirable to remove the obligation now imposed on ships "approaching each other, so as to involve risk of collision," to reverse their engines. If the action of ships with engines reversed is as I have said above, the reversing not only reduces the risk of serious damage, by lessening the way of both ships, but brings them parallel to each other, thereby placing them in a good position to avoid collision.

I would also submit that it is desirable that attention should be called to the power of the steering-gear. I think it probable that in large steamers of great speed, with small crews, and not fitted with steam steering-gear, the number of men usually kept at the wheel would be found quite inadequate to get the helm hard over till the speed of the ship was reduced.

It is worth consideration whether it should not be made obligatory, on steam-ships over a certain size and speed carrying emigrants or passengers, to be fitted with steam steering-gear, which I believe is not the case at present.

I believe a doubt exists with many people whether it is safe and proper to reverse engines when going at full speed ahead at once to full speed astern; this doubt (if it exists) should be removed, and it should be clearly understood that engines are to stand being suddenly reversed from extreme speed one way to the opposite extreme.

H.M.S. 'Speedy,' gunboat, 273 tons, 60 horse-power, Griffith's screw, right-handed, 2-bladed, diameter 6 feet 1 inch, pitch 6 feet. January 24th, 1877.

Trial	Engines	Helm	Wind	Result
1.	Going full speed ahead, suddenly reversed to full speed astern.	Hard aport.	Ahead.	Before headway was lost, head went to starboard 15°, lost headway in 1′ 15″; ship's head still went to starboard with sternway 180° in 8′ 15″.
2.	Going full speed ahead, suddenly reversed to full speed astern.	Hard astarboard.	Ahead.	Before headway was lost, head went to port 20°, lost headway in 50″; with sternway ship's head went to starboard 88° in 3′ 20″.
3.	Going full speed astern, suddenly reversed to full speed ahead.	Hard aport.	4 points on starboard quarter.	Before sternway was lost, head went to port 9°, lost sternway in 25″; then ship's head went to starboard.
4.	Going full speed astern, suddenly reversed to full speed ahead.	Hard astarboard.	4 points on starboard quarter.	Before sternway was lost, head went to port, lost sternway in 1′ 22″; ship's head went off to port immediately helm was put to starboard 101° in 4′.
5.	Full speed ahead and reversed to full speed astern.	Amidships.	Starboard beam.	Ship's head went to starboard; lost headway in 1′ 10″; still going to starboard, 90° in 4′ 20″.
6.	Full speed ahead.	Amidships.	Starboard beam.	Ship's head went to starboard $22\frac{1}{2}$° in 5′, and $67\frac{1}{2}$° in 9′ 32″.
	Full speed ahead. (No cause could be seen for the ship's head going opposite ways in these two trials.)	Amidships.	2 points on starboard quarter.	Ship's head went to port 31° in 3′ 37″, and continued to go to port till wind was astern 51° in 9′ 4″.
	Full speed astern.	Put from hard aport to amidships.	......	Ship's head went fast to port.
	Full speed astern.	Put from hard astarboard to amidships.	......	Ship's head went to starboard 66° in 3′ 55″.

(Signed) Charles J. Waddilove, Captain,
W. A. Parker, Staff-Commander,
W. J. Riley, Chief Gunner,
} H.M.S. 'Asia.'

For continuation see paper 35.

29.

ON THE MANNER IN WHICH RAINDROPS AND HAILSTONES ARE FORMED.

[*From the Sixth Volume of the Third Series of* "Memoirs of the Literary and Philosophical Society of Manchester." *Session* 1876–77.]

(*Read* October 31, 1876.)

WHEN the particles of water or ice which constitute a cloud or fog are all of the same size, and the air in which they are sustained is at rest or is moving uniformly in one direction, then these particles can have no motion relatively to each other. The weight of the particles will cause them to descend through the air with velocities which depend on their diameters; and since they are all of the same size, they will all move with the same velocity.

Under these circumstances, therefore, the particles will not traverse the spaces which separate them, and there can be no aggregation so as to form raindrops or hailstones.

If, however, from circumstances to be presently considered, some of the particles of the cloud or fog attain a larger size than others, these will descend faster than the others, and will consequently overtake those immediately beneath them; with these they may combine so as to form still larger particles, which will move with greater velocity and, more quickly overtaking the particles in front of them, will add to their size at an increasing rate.

Under such circumstances, therefore, the cloud would be converted into rain or hail, according as the particles were water or ice.

The size of the drops from such a cloud would depend simply on the quantity of water suspended in the space swept through by the drop in its descent—that is to say, on the density and thickness of the cloud below the point from which the drop started.

My object in this paper is to suggest that this is the actual way in which raindrops and hailstones are formed. I was first led to this conclusion from observing closely the structure of ordinary hailstones.

Although to the casual observer hailstones may appear to have no particular shape except that of more or less imperfect spheres, on closer inspection they are seen all to partake more or less of a conical form with a rounded base like a sector of a sphere.

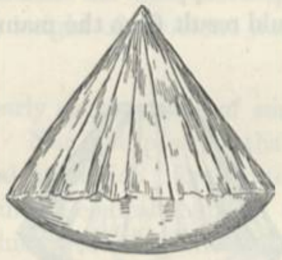


Fig. 1. Perfect Hailstone.

In texture they have the appearance of an aggregation of minute particles of ice fitting closely together, but without any crystallization such as that seen in the snowflake—although the surface of the cone is striated, the striæ radiating from the vertex.

Such a form and texture as this is exactly what would result if the stones were formed in the manner described above. When a particle which ultimately formed the vertex of the cone, started on its downward descent and encountered other particles on its lower face, they would adhere to it, however slightly. The mass, therefore, would grow in thickness downwards; and as some of the particles would strike the face so close to the edge that they would overhang, the lower face would continually grow broader, and a conical form be given to the mass above.

When found on the ground the hailstones are generally imperfect; and besides such bruises as may be ascribed to the fall, many of them appear to have been imperfect before reaching the ground. Such deformities, however, may be easily accounted for.

The larger stones fall faster than those which are smaller, and consequently may overtake them in their descent; and then the smaller stones will stick to the larger and at once deform them. But besides the deformation caused by the presence of the smaller stone, the effect of the impact may be to impart a rotary motion to the stone, so that now it will no longer continue to grow in the same manner as before. Hence we have causes for almost any irregularities of form in the ordinary hailstone.

It appears from the numerous accounts which have been published, that occasionally hailstones are found whose form is altogether different from that described above. These, however, are exceptional; and to whatever causes they may owe their peculiarities, these causes cannot affect the stones to which I am referring.

Again, on careful examination, it is seen that the ordinary hailstones are denser and firmer towards their bases or spherical sides than near the vertex of the cone, which latter often appears to have broken off in the descent. This also is exactly what would result from the manner of formation described above.

Fig. 2. Broken Hailstone.

When the particle first starts, it will be moving slowly, and the force with which the particles impinge upon it will be slight and, consequently, its texture loose; as, however, it grows in size and its velocity increases it will strike the particles it overtakes with greater force, and so drive them into a more compact mass. If the velocity were sufficient, the particles would strike with sufficient force to adhere as solid ice; and this appears to be the case when the stones become large—as large as a walnut, for instance.

An idea of the effect of the suspended particles on being overtaken by the stone, may be formed from the action of the particles of sand in Mr Tilghman's sand-blast, used for cutting glass. The two cases are essentially the same, the only difference being that the hailstone is moving through the air, whereas in the case of the sand-blast the object which corresponds to the stone is fixed, and the sand is blown against it.

By this sand-blast the finest particles of sand are made to indent the hardest material, such as quartz or hard steel; so that the actual intensity of the pressure between the surface of the particles of sand and that of the object they strike must be enormous. And yet the velocity of the blast is not so much greater than that at which a good-sized hailstone descends. It is easy to conceive, therefore, that the force of the impact of the suspended particles of ice, if not much below the temperature of freezing, on a large hailstone, would drive them together so as to form solid ice; for the effect of squeezing two particles of ice together is to cause them to thaw at the surface of contact, and as soon as the pressure is relieved they freeze again; and hence their adhesion.

Nor does there appear to be any other way in which these ordinary hailstones can be formed. They are clearly not raindrops frozen, or they would be somewhat transparent; neither are they aggregations of snow crystals. Nor can they be formed by the condensation and refrigeration of vapour on a nucleus of ice; for there is no way of getting rid of the heat which must be developed by such a process: the heat developed by the condensation of vapour one-seventh of the weight of the stone would be sufficient to thaw the entire stone.

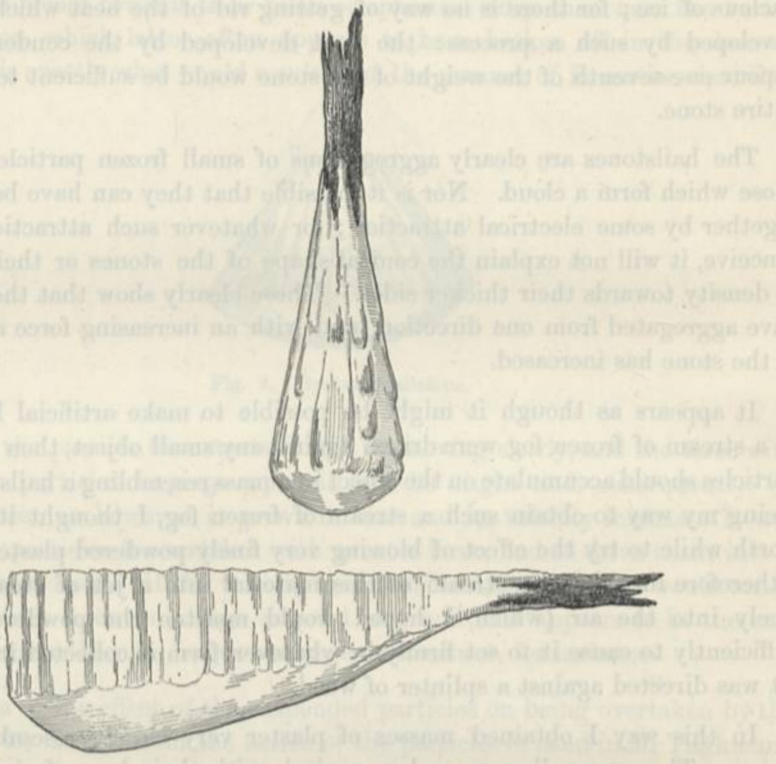
The hailstones are clearly aggregations of small frozen particles such as those which form a cloud. Nor is it possible that they can have been drawn together by some electrical attraction; for whatever such attraction we can conceive, it will not explain the conical shape of the stones or their increase in density towards their thicker sides. These clearly show that the particles have aggregated from one direction, and with an increasing force as the size of the stone has increased.

It appears as though it might be possible to make artificial hailstones. If a stream of frozen fog were driven against any small object, then the frozen particles should accumulate on the object in a mass resembling a hailstone. Not seeing my way to obtain such a stream of frozen fog, I thought it might be worth while to try the effect of blowing very finely powdered plaster of Paris. I therefore introduced a stream of this material into a jet of steam issuing freely into the air (which I hoped would moisten the powdered plaster sufficiently to cause it to set firmly in whatever form it collected into). The jet was directed against a splinter of wood.

In this way I obtained masses of plaster very closely resembling hailstones. They were all more or less conical, with their bases facing the jet. But as might be expected, the angles of the cones were all smaller than those of the hailstones. Two of their figures are shown in the sketches annexed (p. 218).

The striæ were strongly marked, and exactly resembled those of the hailstone. The bases also were rounded. They were somewhat steeper than those of the hailstone; but this was clearly due to the want of sufficient cohesive power on the part of the plaster: it was not sufficiently wet. Owing to this cause also it was not possible to preserve the lumps when they were formed, as the least shake caused them to tumble in pieces.

I also tried a jet of the vapour of naphthaline, which at ordinary temperatures is solid, driven by means of a cross blast of air against a small object; and in this way I obtained masses closely resembling hailstones: but these also were too fragile to bear moving. At ordinary temperatures the powdered naphthaline does not adhere like ice when pressed into a lump. No doubt at very low temperatures ice would behave in the same way; that is to say, the particles would not adhere from the force of impact. Hence it would seem probable that for hailstones to be formed the temperature of the cloud must not be much below freezing-point.



Figs. 3 and 4. Imitations in Plaster of Paris.

That the temperature of the cloud exercises great influence on the character of the hailstones cannot be doubted; and if, as has been suggested by M. L. Dufour, the particles will sometimes remain fluid, even when the temperature is as low as 0° F., it is clear that as they are swept up by a falling stone they may freeze into homogeneous ice, either in a laminated or crystalline form. Upon these questions, however, I do not wish to enter, as they have no bearing on the question as to the manner in which the mass of the stone is accumulated; and I only mention them to show that, if there are unexplained peculiarities, there are also causes the effects of which have not as yet been fully considered.

This view of the manner in which hailstones are formed at once suggests that raindrops may be formed in the same way; nor does there appear, on further consideration, to be any reason to suppose that such is not the case.

Of course a raindrop shows none of the structural peculiarities of the hailstone; and consequently we have not the same evidence of the manner in which raindrops are formed; but the explanation is sufficient, and there is apparently no other.

Raindrops cannot possibly have grown to the size with which they reach the earth by the condensation of the vapour of the air which they pass through, for the same simple reason as that just stated for hailstones, namely that there is no way in which the heat developed by condensation can be got rid of. The fact that the upper regions of the air from which the drops start are colder than those through which they descend, might, as has been supposed, cause the drop to grow by condensing vapour in the air through which it passes—but, as was shown by Mr Baxendell*, only to a very small extent, and one the limit of which may be easily estimated.

Suppose the drop to start having a weight w_1 and a temperature t_1, and on reaching the earth to have a temperature t_2. Then the increase in the quantity of heat in the drop would be $(t_2 - t_1)\, w_1$ nearly. This heat would be developed by the condensation of a weight of water $(t_2 - t_1)\dfrac{w_1}{1000}$ nearly; so that, even supposing $t_2 - t_1 = 100^\circ$ F., which it could not possibly be, the increase in the weight of the drop could not be one-tenth.

It is obvious also that the drop would not have parted with its heat to the air it passes through; for it is assumed to be colder than this air. Therefore the only way in which it could have parted with its heat would have been by radiation. Some heat might be lost in this way, but only a very small amount, and one of which an approximate estimate may be made. For after the drop had acquired a considerable size, say one-hundredth of a foot in diameter, the time occupied in its descent would be very small. Assume this to be one minute; and assume that during this time the drop is 100 degrees hotter than the surrounding objects, although this is of course far beyond what could possibly be. According to the most accurate data the amount of heat it would then lose would not be sufficient to condense $\frac{1}{200}$ of a grain of water†—an altogether inappreciable amount when compared with the weight of the drop, which would be nearly the quarter of a grain.

* *Memoirs of the Lit. and Phil. Soc. of Manchester*, Vol. I., 3rd Series, p. 399.

† The surface of a drop whose diameter is ·05 ft. is ·000314 sq. ft. Now if the temperature of the surrounding objects be zero Centigrade, and the temperature of the drop be 60° Centigrade, then, assuming the radiation from the surface of the drop to be the same as the radiation from the surface of glass, we have (see Balfour Stewart on *Heat*, p. 228):—

$$R = 10\,(16{\cdot}00770 - 1)\,A,$$

It appears clear, therefore, that the only way in which a falling drop can grow is by the aggregation to itself of the particles of moisture in the air; and the only way in which it can encounter these is by its downward motion through this air.

Such a means of growth is amply sufficient to account for the size of raindrops or of hailstones.

If we suppose all the vapour which a body of saturated air at 60° F. would contain, over and above what it would contain at 30°, to be changed into a fog or cloud, then, if a particle, after commencing to descend, aggregated to itself all the water suspended in the volume of air through which it swept, the diameter of the drop after passing through 2000 feet* would be more than an eighth of an inch, and after passing through 4000 feet a quarter of an inch, and so on; so that in passing through 8000 feet of such cloud it would acquire a diameter of half an inch. Now, as clouds must often contain more water than what is here supposed, there is no difficulty in explaining the size of the drops. The difficulty is rather the other way, in explaining why the drops are not sometimes larger than they are.

There are, however, two reasons why raindrops do not acquire the full size which might be expected on the above assumptions.

where R is the heat radiated from the surface A in one minute, the unit being the heat required to raise 1000 grains of water 1° C. This gives

$$R=6A,$$

or

$$R=\cdot 002 \text{ nearly}.$$

Now if R' be the same quantity of heat, the unit being the heat required to raise one grain 1° Fahr.,

$$R'=1800R,$$
$$=3\cdot 6.$$

This is equivalent to the latent heat of condensation of ·0036 grain of water.

Again, let w be the weight of the drop; then

$$w=7000\times 62\cdot 5\times \tfrac{4}{3}\pi r^3$$
$$=\cdot 21 \text{ or nearly } \tfrac{1}{5} \text{ grain}.$$

* If x be the diameter of the drop after descending a distance h, and ρ the volume of water suspended in a unit volume of air, then the increase of volume of the drop in descending a distance dh is given by

$$\frac{\pi}{2}x^2dx=\rho\frac{\pi}{4}x^2dh;$$

$$\therefore\ dx=\frac{\rho}{2}dh;$$

or

$$x=\frac{\rho}{2}h.$$

Hence, if $\rho=\cdot 00001,\quad x=\cdot 000005h;$

and if $x=\cdot 01,\quad h=2000 \text{ feet}.$

In the first place, the drop will not aggregate to itself all the particles in front of it. Some of these will be swept away sideways by the diverging current of air; and the smaller the particles are the more will this be the case. This is, of course, true for hail as well as for rain.

The second reason applies only to rain, and explains why it is that hailstones sometimes acquire magnitudes never approached by raindrops.

A drop retains its form simply by the surface-tension of the water; and as this is the same whatever may be the size of the drop, its power to hold the drop together diminishes as the size of the drop increases, whereas the velocity and consequent tendency of the air to disturb the shape of the drop increase with its size. Hence it must eventually arrive at such a size that it can no longer hold together, but will be blown to pieces by the rush of air past it. This action may be seen in a waterfall or a fountain, where, in passing through the air, a solid column of water is separated into drops not larger than large raindrops.

The same reasoning does not hold for hailstones, which are held together by the adhesion of the particles throughout their entire mass, and whose compactness and strength increase with their size. It is, however, the case that the smaller end of the stone, where the texture is looser, appears to be blown off in its subsequent descent, especially when the stones acquire a larger size.

It seems, therefore, that, so far as the growth of a drop or a stone is concerned, the particles it overtakes in its downward path are a necessary and sufficient cause; but the origin of the drops and stones requires further explanation. Why should some of the particles in a cloud be larger than the others, as it is necessary for them to be in order that they may commence a more rapid descent?

A cloud does not always rain; and hence it would seem that in their normal condition the particles of a cloud are all of the same size and have no internal motion, and that the variation of size is due to some irregularity or disturbance in the cloud.

Such irregularity would result when a cloud is cooling by radiation from its upper surface. The particles on the top of the cloud being more exposed would radiate faster than those below them; and hence they would condense more vapour and grow more rapidly in size. They would therefore descend and leave other particles to form the top of the cloud. In this way we should have in embryo a continuous succession of drops.

Eddies in the cloud also form another possible cause of the origin of drops and stones. Whenever the direction of motion of a portion of the cloud is not straight, the suspended particles will have more or less motion through the air. And if, as in an eddy, the motion of the cloud varies from point to point both in direction and magnitude, then the motion of the particles through the air will also vary, and they may overtake one another and, combining, form larger particles or drops in embryo.

Whatever may be the cause of the variation in the size of the particles which form the cloud, we may know from observations on fogs that such variations do exist. In fogs we have particles of all sizes, from those which are too fine to be seen even by the aid of a microscope, and which will remain suspended for hours without any appreciable descent, up to such a size that they can be easily detected with the naked eye, and descend with a very appreciable velocity so as to form a drizzle. When a coarse mist, such as this, is superimposed over a fine mist, then rain must ensue if the particles are water, and hail if they are ice.

Although, as has been shown, a raindrop cannot add considerably to its volume by condensing the vapour from the air through which it passes; the reverse of this is not the case. The raindrop may be diminished by evaporation. Whenever a raindrop falls through dry air (that is, air of which the dew-point is below the temperature), evaporation might, and would, go on to almost any extent, and the size of the drops be diminished until they entirely vanished, the heat for evaporation being supplied from the air, which would be warmer than the drop.

The case of snow differs from that of hail. The snow crystals are clearly formed by the condensation of vapour, and not by the mere aggregation of particles of ice. In this case the latent heat developed in condensation is probably dissipated by radiation, the shape and smallness of the crystals causing them to descend very slowly, and so affording time for the radiation to produce an effect.

But even in snow we see the effect of aggregation. The individual crystals never acquire a large size. But in their descent, the larger ones overtaking the smaller, they form into flakes. In this case the aggregation may be seen taking place. If when large flakes of snow are falling fast without wind, the eye be fixed on a large flake as high as it can at first be perceived, and follow this flake in its subsequent descent, it may sometimes be seen to overtake another flake and combine with it, the two descending together.

For continuation see p. 223.

30.

ON THE FORMATION OF HAILSTONES, RAINDROPS, AND SNOWFLAKES.

[*From the Sixth Volume of the Third Series of* "Memoirs of the Literary and Philosophical Society of Manchester." *Session* 1877–78.]

(*Read* October 30, 1877.)

THIS communication forms a continuation of the paper I read before this Society on the 31st of October, 1876, "On the Manner in which Raindrops and Hailstones are formed*."

To the contents of this paper I shall have to refer continually; hence, in order to render what I have to say intelligible, it may be well for me to recapitulate some of the leading points in my former paper. The chief purpose of the paper was to explain the manner in which the minute cloud-particles aggregated so as to form raindrops and hailstones.

Aggregation resulting from the more rapid Descent of the larger Particles.

I commenced by pointing out that, as the suspended particles of water or ice which constitute a cloud are all descending with velocities which increase with their size, the larger particles will descend faster than the others, and will consequently overtake those immediately beneath them; with these they will combine so as to form still larger particles, which will move with greater velocity and, more quickly overtaking the particles in front of them, will add to their size at an increasing rate. And I then proceeded to consider how far this was a sufficient as well as a necessary cause of the phenomena of hail and rain. One of the most important points on which my arguments were based was

* See p. 214.

The Shape and Structure of ordinary Hailstones.

On close observation I had found, what had previously been noticed by other observers, that the shape of an ordinary hailstone is not what it at first sight appears to be. They are not spheres more or less imperfect, but more or less imperfect cones or pyramids with rounded bases, like the sectors of spheres—the conical surface being striated, the striæ radiating from the vertex of the cone.

In texture the hailstones have the appearance of being an aggregation of minute particles of ice fitting closely together, but without any crystallization such as that seen in the snowflake; while, on careful observation, it is seen that they are denser and firmer towards their bases or spherical sides than near the vertex of the cone, which latter often appears to have been broken off in their descent.

As I explained, it seemed to me that this form and structure was exactly such as would result from the manner of aggregation which I had supposed. When a particle which ultimately forms the vertex of the cone starts on its downward career and encounters other particles, these adhere to its lower face. The mass, therefore, grows in thickness downwards; and as some of the particles strike the face so close to the edge that they overhang, the lower face continually grows broader, and a conical form is given to the mass above.

When a particle first starts, it moves slowly, and the force with which it meets the other particles is slight, and consequently its texture is loose; but as it increases in size and velocity, it strikes the particles which it overtakes with greater force, and so drives them into a more compact mass.

Assuming that the temperature at which hailstones are formed is not greatly below 32°, the particles must actually freeze together. For the effect of squeezing two pieces of ice together at or near the temperature of 32° is to cause them to thaw at those points where the pressure is greatest, at which points they freeze again as soon as the pressure is removed.

In illustration of the force with which the particles strike the face of the hailstone, I instanced the action of the particles of sand in Mr Tilghman's sand-blast used for cutting glass and other hard materials.

I also reverted to the possibility of making

Artificial Hailstones,

by blowing a stream of frozen fog against a small object, making, as it were, the cloud to rise up and meet the stone instead of the stone falling through the cloud.

I had not, however, then overcome the difficulty of obtaining such a stream of frozen fog; but I gave two sketches of plaster stones, which, as far as their shape and the striated appearance of their surface were concerned, closely resembled hailstones, and which plaster stones had been obtained by blowing some finely-divided plaster of Paris against small splinters of wood by means of a jet of steam.

In the discussion which followed my paper Dr Crompton suggested

The Ether Spray,

such as is used in surgery, as a means of obtaining a frozen fog. And shortly after the Meeting I tried this ether spray, using an instrument such as surgeons use. But although I found that the spray would freeze anything such as a small tube of water, I could get no deposit of ice particles on the outside of any object. I varied the form of the apparatus, but with no better success; and for the time I abandoned the attempt.

What the cause of this failure was I do not precisely know; but I attribute it to some excess of alcohol in the ether then used, which was not methylated ether. That this might have been the cause occurred to me about two months ago. I then determined to try again, and combine a spray of water with that of ether. I now obtained the lightest ether which Messrs Mottershead & Co. could supply. The specific gravity of this was ·717; and it was made from methylated spirit.

With this, somewhat to my surprise, I at once obtained a deposit of ice even without the water spray, and with the same apparatus I had previously used; it was not, however, until I used the combined spray of water and ether that I obtained anything resembling a hailstone in appearance. But the first time I used this combination I obtained a small but well-shaped hailstone on the end of a match which I held pointed towards the spray.

The next time I tried, however, on another day, I did not succeed so well with the water as without it: when using the water spray the deposit of ice was wet or half melted, while without the water I obtained a hailstone in much the same manner as I had obtained before with the water.

This difference in the results on the two occasions was at once explained by the different states of the air; for on the first occasion it had been cold and dry, whereas on the second it was warm and saturated. With the dry air the ether spray reduced the temperature so far below 32° that the particles of ice did not freeze together; the force of impact was not sufficient to cause them to thaw in the first instance; and hence the water spray was necessary to keep this temperature from falling too low; whereas with the warm saturated air the ether did not reduce the temperature of the air and the vapour it contained much below 32°, and consequently, when the water spray was added, the water was only partially frozen.

I subsequently improved the apparatus so as to be able to regulate the supply of water and ether to the condition of the air.

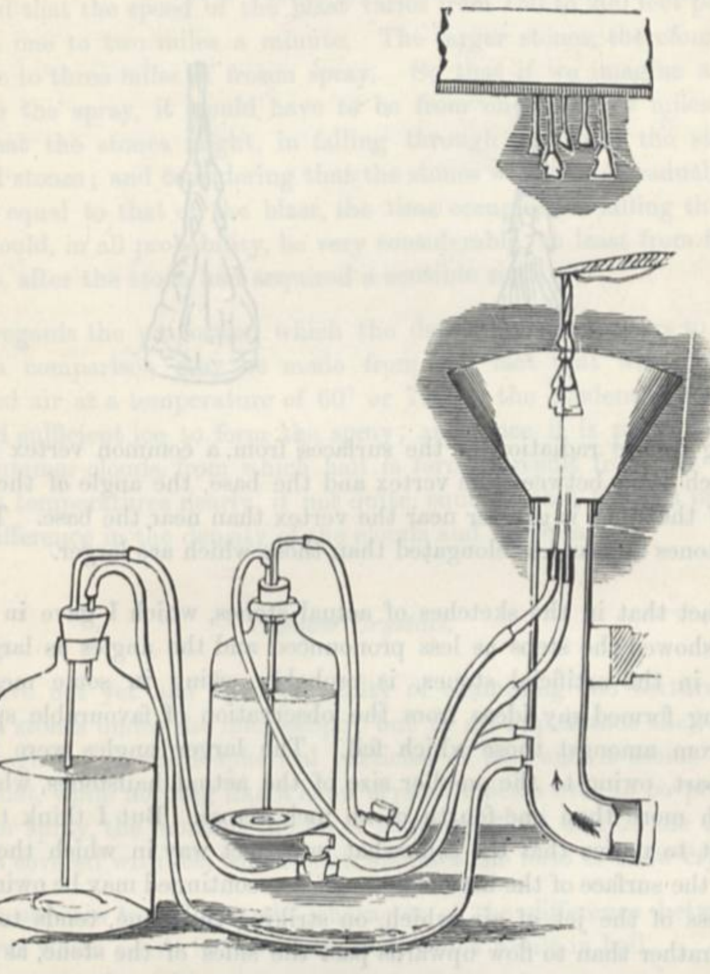
The Apparatus.

This is shown in the accompanying sketch. It consists of a brass tube half an inch in diameter, one end of which is connected with bellows capable of maintaining a constant pressure of about eighteen inches of water; on the other end of the tube is a cap, over the end of which is a flat plate or diaphragm having a central opening an eighth of an inch in diameter, which forms the aperture for the blast. Entering through the sides of the main brass tube are two small brass tubes which reach to within half an inch of the plate, and into the ends of which are sealed fine glass capillary tubes, the glass being very thin; these protrude just through the middle of the aperture, the one about one-sixteenth of an inch and the other one thirty-second. Through these tubes the water and ether are separately introduced into the blast to form the spray; and it is mainly on the adjustment of these tubes that the efficiency of the apparatus depends. It is essential that the ether-tube should be slightly the longest; otherwise the ends become stopped with ice; and I find it better that the ether-tube should be somewhat larger than the water-tube. The bore of the tubes must be very small: but this is not sufficient; for unless the glass is very thin the spray will not be finely divided. Both the ether and water are forced through the tubes from bottles by connecting the interiors of these bottles with the bellows; and the quantities of ether and water are regulated either by raising or lowering the bottles or by means of the cocks in the pipes.

The tube is fixed in an ordinary retort-stand so that the blast is vertical. If, then, a small splinter of wood is held downwards pointing into the spray, a lump of ice forms on the end of the splinter; and this lump has all the appearance of the hailstone. It is quite white and opaque; it is conical in form, and has a rounded base and striated surface.

In this way I have formed stones from half to three-quarters of an inch in diameter. When, however, the stones are growing large, it is necessary to move this splinter so as to expose in succession all parts of the face of the stone to the more direct action of the spray.

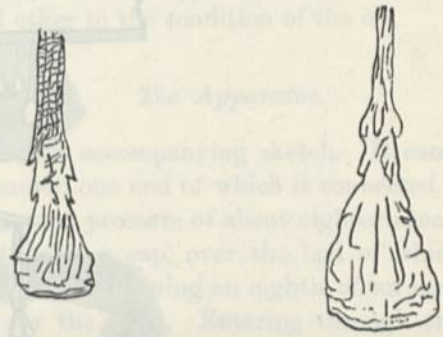
When using this apparatus in a warm room, I have found it best to fix a pad of blotting-paper over the jet at a height of ten or twelve inches. The surface of this pad is cooled by the spray and prevents radiation from the ceiling, which otherwise tends to melt the top of the stone. For a similar reason I have found it well to surround the blast with a wide cylinder or



inverted cone of paper, which keeps off radiation without interfering with the action of the jet.

By sticking several splinters of wood pointing downwards into the pad, a number of stones may be made at once.

In the accompanying sketch (p. 228) are shown a medium-sized stone, as well as one of the largest stones, attached to the splinters of wood. The surface of the cone where continuous is truly conical, or rather pyramidal; but the surface is broken, as it were, by steps; and a very marked fact is that all the continuous surfaces have the same vertex; and hence the different conical surfaces to which they belong have not the same vertical angle, the surface being exactly such as would be acquired by the fragments of a sphere so constituted that the fracture tended to follow radial lines.



Owing to the radiation of the surfaces from a common vertex and the steps which occur between the vertex and the base, the angle of the conical surface of the stone is greater near the vertex than near the base. Thus the smaller stones appear less elongated than those which are larger.

The fact that in the sketches of actual stones, which I gave in my last paper, I showed the steps as less pronounced and the angles as larger than they are in the artificial stones, is probably owing in some measure to my having formed my ideas from the observation of favourable specimens chosen from amongst those which fell. The larger angles were probably also, in part, owing to the smaller size of the actual hailstones, which were not much more than one-fourth of an inch across. But I think that it is important to notice that the somewhat imperfect way in which the outside layers in the surface of the artificial stones are continued may be owing to the narrowness of the jet of air, which, on striking the stone, tends to diverge laterally rather than to flow upwards past the sides of the stone, as it would do if the jet were broader, or as the air must do when the stone is falling through it.

The rate at which stones can be formed depends on the amount of water which can be introduced into the spray, the larger stones taking from one to two minutes. At first sight this may seem to be somewhat slow; but the following estimate tends to show that the artificial are probably formed more quickly than the actual stones.

The speed of the jet of air at the point at which the stones are formed is nearly equal to that at which the larger stones would fall through the air. This is shown by the fact that if a large stone becomes accidentally detached from its splinter of wood it rather falls than rises, but when this happens with smaller stones they are driven up by the force of the blast.

I find that the speed of the blast varies from 150 to 200 feet per second, *i.e.* from one to two miles a minute. The larger stones, therefore, traverse from one to three miles of frozen spray. So that if we imagine a cloud as dense as the spray, it would have to be from one to three miles thick in order that the stones might, in falling through it, attain the size of the artificial stones; and considering that the stones would only gradually acquire a speed equal to that of the blast, the time occupied in falling through the cloud would, in all probability, be very considerable, at least from five to ten minutes, after the stone had acquired a sensible size.

As regards the proportion which the density of spray bears to that of a cloud, a comparison may be made from the fact that when working in saturated air at a temperature of 60° or 70° F., the condensation of vapour supplied sufficient ice to form the spray; and since it is probable that the dense summer clouds, from which hail is formed, result from the cooling of air from temperatures nearly, if not quite, equal to this, there is probably no great difference in the density of the clouds and the spray.

Snow Crystals.

I have not yet had an opportunity of examining the texture of these artificial stones under the microscope; but to all appearance they consist of an aggregation of small spherical particles of ice; and it seems worthy of notice that, while nothing like a snow crystal appears ever to be produced in the ether spray, the moment the blast is stopped the end of the ether-tube becomes covered with ice, which often assumes the form of snow crystals.

This appears to indicate the character of the difference between those conditions which result in snow and those which result in hail.

When the cloud-particles are formed at or above the temperature of 32°, and then freeze, owing to cooling by expansion or otherwise, the particles as they freeze retain their spherical form. This is what happens in the spray.

On the other hand, when saturated air at a temperature below 32° is still further cooled, the deposit of the vapour will be upon ice, and will take the form of snow crystals.

The aggregation of the snow crystals into flakes is, as I have pointed out in my previous paper, accounted for by the larger crystals overtaking the smaller crystals in their descent, and the still more rapid descent of the flakes as they increase in size.

As regards the formation of raindrops, I have nothing to add to what was contained in my last paper. The same explanation obviously applies to both hail and rain; and any doubt which may have been left by the less direct arguments in my former paper will, I venture to think, have been removed by the verification of my predictions in the production of artificial hailstones so closely resembling in all particulars those formed by nature. In conclusion, I would thank Dr Crompton for the suggestion of the means by which I have been able to produce these stones.

31.

ON THE INTERNAL COHESION OF LIQUIDS AND THE SUSPENSION OF A COLUMN OF MERCURY TO A HEIGHT MORE THAN DOUBLE THAT OF THE BAROMETER.

[*From the Sixth Volume of the Third Series of* "Memoirs of the Manchester Literary and Philosophical Society." *Session* 1877–78.]

(*Read* October 30, 1877.)

Introduction.

THE ease with which, under ordinary circumstances, the different portions of liquid may be separated, is a fact of such general observation that the inability of liquids like water to offer any considerable resistance to rupture appears to have been tacitly accepted as an axiom. In no work on Hydrostatics does it appear that the possibility of water existing in a state of tension is so much as considered; and suction is always described as being solely attributable to the pressure of the atmosphere.

The limit, of 32 feet or thereabouts, to the height to which water can be raised by suction in the common pump, and the sinking of the mercury in the barometer-tube (leaving the Torricellian vacuum above) until the column is at most only 31 inches (sufficient to balance the highest pressure of the atmosphere), are phenomena so well known as to be almost household words with us. It is not, therefore, without some fear of encountering simple incredulity that I venture to state

The Object of this Communication.

In the first place my purpose is to show that certain facts, already fully established, afford grounds for believing that almost all liquids, and particularly mercury and water, are capable of offering resistance to rupture commensurate with the resistance offered by solid materials. In the second place, I have to describe certain experimental results which, as far as they go, completely verify these conclusions and subvert the general ideas previously mentioned as to the limits to the height to which mercury can be suspended in a tube, or water raised by suction. And, in conclusion, I shall endeavour to explain the nature of the circumstances which have resulted in the practical limits to these phenomena.

The Separation of Liquids is not caused by Rupture.

Although the smallness of the force generally requisite to separate a mass of liquid into parts leads to the supposition that the parts of the liquid have but little coherence, it may be seen on close examination that this supposition is not altogether legitimate; for such separation of a liquid as we ordinarily observe takes place at the surface of the liquid, is caused by an indentation or running-in of the surface, and not by an internal rupture or simultaneous separation over any considerable area. Thus when we see a stream of liquid break up to drops, the drops separate gradually by the contraction of the necks joining them, as shown in fig. 1, and not suddenly as in fig. 2. And

Fig. 1. Fig. 2.

the ease with which portions of a liquid may be separated by the forcing or drawing in of the surface affords no ground for assuming that the liquid is without coherence, any more than does the ease with which we may cut a piece of string, cloth, or metal with sharp shears, or even tear some of these bodies by beginning at an edge, prove that they are without strength to resist great force when these are applied uniformly so as to call forth the resistance of all the parts of the body simultaneously. It is true that under certain circumstances we observe the internal rupture of liquid—whenever bubbles are formed, as when water is boiled; but under these circumstances we have no means of estimating the forces which cause the internal rupture: they are molecular in their action; and, for all we know, they may be very considerable. Having thus pointed out that the ease of separation of the parts of a mass of liquid does not even imply a want of cohesion on the part of the liquid, I shall now point out that we have in common phenomena.

Evidence of Considerable Cohesion.

These are, for the most part, what are considered minor phenomena; they are confined to the surface of the liquid and are included under what is called "capillarity," or "surface-tension."

The phenomena of capillarity or surface-tension have recently attracted a great deal of attention; and many important facts concerning them have been clearly elucidated, some of which bear directly on my present subject.

Of the phenomena I may instance the suspension of drops of water, the rising of water up small tubes, the tendency of bubbles to contract, and the spherical form assumed by small fragments of mercury.

These phenomena and others are found to be explained by the fact that the surface of these liquids is always under a slight but constant tension, as if enclosed in a thin elastic membrane.

No satisfactory explanation as to the cause of this surface-tension has, I believe, been as yet found; but the fact itself is proved beyond all question. It is a molecular phenomenon; and in order to offer any explanation as to its cause, it would be necessary to adopt some hypothesis respecting the molecular constitution of the liquid. Such an explanation making the surface-tension to arise from the cohesion of the molecules of the liquid is, I believe, possible; but this is beside my present purpose, which will be completely served by showing that

The Surface-tension proves the existence of Cohesion.

To prove this requires no molecular hypothesis; but, before proceeding, it may be well to define clearly the term cohesion.

Cohesion in a liquid is here to be understood as a property which enables the fluid to resist any tendency to cause internal separation of its parts—any tendency to draw it asunder; or, more definitely, it is the property which enables a liquid to resist a tension or negative pressure.

Let us suppose a mass of liquid without internal cohesion. Then any external action tending to enlarge the capacity within the bounding surface of the liquid would at once cause the interior of the liquid to open, and a hollow would be formed within the liquid without any resistance on the part of the liquid. Such a condition is inconsistent with surface-tension; for the tension of the surface of the internal hollow would tend to contract the hollow; and since the interior of the hollow is supposed to be empty, there could be no resistance to the tendency of the surface to contract, such as that offered by the pressure of the gas within an ordinary bubble. Hence any force that might, under the circumstances, balance the surface-tension and keep open the hollow must be supplied by the suction or cohesion of the liquid outside.—Q. E. D.

Again, *the intensity of the cohesion is determined by the intensity of the surface-tension and the smallness of the least possible opening over the surface on which tension exists.*

So far as has yet been determined by experiment, it has been found that the surface-tension is independent of the curvature of the surface—is constant for the same liquid. Assuming that this is the case, it follows that the intensity of the force necessary to keep a spherical bubble or opening from contracting (whether this force arises from the pressure of the gas within the bubble or the cohesive traction of the liquid within the opening) is equal to twice the intensity of the surface-tension divided by the radius of the sphere. *Hence the cohesive tension must be equal to twice the surface-tension of the liquid divided by the diameter of the smallest opening for which the surface-tension exists.*—Q. E. D.

It immediately follows from the foregoing proposition, that no matter how small the surface-tension may be, if it is finite even when the opening is infinitely small, then the cohesion of the liquid must be infinitely great. For, if the liquid were continuous, in its origin the opening must always be infinitely small; and hence to cause such an opening would require infinite tension.

That the cohesion is infinitely great is not probable, to say the least. Hence it is improbable that the surface-tension remains finite when the opening becomes infinitely small. As has already been stated, it has been found that the surface-tension is constant, or nearly so, under ordinary circumstances; but it has never been measured for bubbles of very small diameter, and there appears to be every probability that, when the size of the bubble comes to be of the same order of small quantity as the dimensions of a molecule, the surface-tension must diminish rapidly with the size of the bubble.

If this is the case, then we have a limit to the cohesion, although it is probably very great for most liquids, something like the cohesion of solid matter of the same kind. That is to say, it is probable that it would require nearly as great intensity of stress to rupture fluid as it would to rupture solid mercury, or as great tension to rupture water as to rupture ice.

The Effect of Vapour.

Nothing has yet been said about the effect of the pressure of vapour within the bubbles in balancing the surface-tension. It may, however, be shown that this can be of no moment. Even supposing that the tension of the vapour within the opening of the liquid were equal to the tension due to the temperature under ordinary circumstances, this would be inappreciable. So that, unless the tension of vapour within small openings were much greater than that in larger openings for the same temperature, its effect might be neglected; and so far from this being the case, Sir William Thomson has shown that the pressure of the vapour within a bubble at any particular temperature diminishes with the size of the opening. Hence it is clear that this vapour can have no effect on the result—a conclusion verified by the now well-known fact that water may be raised to a temperature high above 212° without passing into steam.

Experimental Verification necessary.

This line of reasoning has been apparent to me now for several years. I find notes on some of the principal points which I made in 1873; and for several years I have pointed out the conclusions arrived at as regards the probable cohesion of water to the students in the engineering class at Owens College. I have, however, hitherto refrained from publishing my views, because I had no definite experimental results to appeal to in confirmation of them. Experimental indications of such a cohesive force were not wanting, but they were not definite. And although methods of making definite experiments have often occupied my thoughts, certain difficulties, which turn out to have been somewhat imaginary, kept me from trying the experiments.

It had always appeared to me that, in order to subject the interior of a liquid mass to tension, it would be necessary to, as it were, hold the surface of the liquid at all points to prevent its contracting. To accomplish this, it was necessary to have the liquid in a vessel, to the surface of which the liquid would adhere as water adheres to glass. The experiment which I had conceived would have been equivalent to a vertical glass tube more than 34 feet long, closed at the upper end and open at the lower, so that when the tube was full of water the column would be higher than the pressure of the atmosphere would maintain, and hence could only be maintained by the cohesion of the water. The difficulty of such an experiment, however, appeared to be great. It was clear that if mercury could be substituted for water this difficulty would be much reduced; but then mercury does not readily adhere to glass, and the ordinary method of making barometers seemed to disprove the possibility of making it adhere.

It was only on the 2nd of this month that an accidental phenomenon at once afforded me the experimental proof for which I had been looking.

First Experiments.

The phenomenon was observed in a mercurial vacuum-gauge (a siphon gauge which admitted a column of mercury 31 inches long). Before the mercury was introduced the tube had been wetted with sulphuric acid, a few drops of which covered the mercury on both ends of the column.

The gauge had been in constant use as a vacuum-gauge for three weeks; and, probably owing to the action of the acid on the mercury, a little gas had been generated between the mercury and the closed end of the tube, sufficient to cause the column to sink to $27\frac{1}{2}$ when the barometer stood at 29. To get rid of this air, the tube was removed from its situation and placed in such a position that the bubble of air passed along the tube and escaped, the open end of the tube being entirely free. Before the tube was tilted in this way, the unbalanced column was $27\frac{1}{2}$ inches long. When tilted, the mercury ran back right up to the end of the tube as the bubble of air passed out. On erecting the tube again, the mercury remained up to the end of the tube, except about one-eighth of an inch, which was filled with sulphuric acid. The unbalanced column of mercury was therefore 31 inches long. At first the full significance of this phenomenon was not recognized; but in order to ascertain that the tube was cleared of air, it was moved gently up and down to see if the mercury clicked, as it usually does when the tube is free from air, but the mercury did not move in the tube. The rapidity of the oscillation was thereupon increased until it became a violent shake, and, as the mercury still remained firm, it was clear that some very powerful force was holding it in its place. The tube being in a vertical position, was then left in order that the barometer might be consulted. This was standing at 29 inches. After a few seconds, when the gauge was again examined, the column no longer reached the end of the tube, but stood at 29 inches. As it was singular that the mercury should have quietly settled down after having resisted such violent shaking, the tube was again inclined until the mercury and acid came, apparently, up to the end of the tube; but this time on the erection of the tube the mercury at once settled down. That is to say, it settled down gradually as the tube was erected. At first what appeared to be a very small bubble opened in the sulphuric acid; and this enlarged as the top of the tube was raised. On again inclining the tube until it was horizontal, and examining it closely, a minute bubble could be seen in the acid, and it was this bubble which expanded as the tube was erected, and so allowed the mercury to descend. To get rid of this bubble, the tube was turned down so as to allow the bubble to pass along the tube; but, owing to its small size, it did not pass many inches along the tube before it became fixed between the mercury and the glass. When the bubble came to a standstill at about six inches from the end of the tube, the gauge was again erected; the bubble immediately began to move back, but so slowly that it was some seconds before it entered the region of no pressure. During this interval the mercury remained up to the end of the tube; but the bubble, as soon as it neared the top of the tube, expanded and rapidly rose to the top of the tube, leaving the column at 29 inches. This operation having been repeated several times, it became quite evident that it was this small bubble which, either by rising up the tube or being generated at the top, had caused the mercury in the first instance to sink. As the bubble would not pass out by itself, the tube was tilted so as to allow a larger bubble of air to enter; and having been left standing for about twelve hours to allow the small bubble to unite with the larger one, it was again tilted so as to allow the air to pass out. When this was done the mercury again remained firmly against the end of the tube and did not descend when violently shaken. The open end of the tube was then connected with an air-pump and exhausted until the pressure within it fell to about four inches of mercury. This operation occupied some seconds; but all this time the mercury did not move from the end of the tube; but eventually the column opened near the bottom of the tube and a large bubble appeared, which rose up the tube, the mercury falling past the opening. That the breaking of the column so near the bottom of the tube was owing to the presence at that point of a small bubble of air was almost proved by the fact that, on readmitting the air to the open end of the tube and inclining the tube to see if it was free from air, there was found a minute bubble which played exactly the same part as the small bubble which had been previously examined.

At the instant previous to the rupture of the column at the bottom of the tube, there must at the top of the tube have been an unbalanced tension or negative pressure equal to 27 inches of mercury; and this tension did not break the continuity of the column. Hence I had a proof that the cohesion within the mercury and the sulphuric acid as well as the adhesion of the sulphuric acid to the mercury and the glass is sufficient to resist this very considerable tension.

Further Experiments.

In the hope of improving the experiments, another gauge was constructed, the tube being $\frac{3}{16}$ of an inch in internal diameter and 35 inches high. Into this tube mercury and sulphuric acid were introduced, as in the first tube.

But on trying to get rid of the small bubbles of air, it was found impossible to do so, as bubbles were continually generated. Hence it appeared that the three weeks during which the mercury and sulphuric acid in the first tube had remained in contact had had an important influence on the result. Failing in this attempt, it occurred to me to try if water would answer the purpose as well as sulphuric acid. Having in my possession an old vacuum-gauge with a column three inches long, which had originally been wetted with sulphuric acid, but into which a considerable quantity of water had accidentally been introduced, I carefully allowed all the air to escape, and then applied a mercurial air-pump to the open end of the gauge, and exhausted as far as the pump would draw. The mercury did not descend. As I could apply no further tension, I shook the gauge up and down; but still the mercury remained unmoved. I then tapped the gauge smartly on the side; the mercury then fell three inches, until it was level. Having succeeded so far, I extracted the mercury and sulphuric acid from the 35-inch gauge and introduced some water without washing the tube, and, having boiled the water in the tube, again introduced the mercury.

Having extracted all the air, I found no difficulty in making the gauge to stand up to the 35 inches without any immediate tendency to fall. On applying the air-pump to the open end the mercury several times remained up until the exhaustion had proceeded so far that when it fell from 22 to 28 inches, and when the rupture took place it was accompanied by a loud click. I could not on that occasion get the mercury to withstand complete exhaustion; but after leaving the gauge with the mercury suspended for 24 hours at 35 inches, I was able to exhaust the open end of the tube as far as the pump would draw, without bringing the mercury down; so that I had a column of 35 inches of mercury suspended by the cohesion of the liquids.

There was no reason to suppose that this was the limit or anywhere near the limit. It was clearly possible to suspend a longer column; but as the length of the column increased so would the difficulty of getting rid of the disturbing causes, and I determined to rest satisfied with the 35 inches; but in order to see if this could be maintained, I obtained a gauge 60 inches long, which would leave 30 inches above the pressure of the atmosphere.

The difficulty of getting rid of the air in this tube sufficiently to allow of the mercury standing 60 inches was very considerable. Before filling the tube it was rinsed out with concentrated sulphuric acid, then twice washed with distilled water, and then water put in and boiled in the tube. Then sufficient mercury was introduced to fill the long leg and the bend, so that the column, when complete, was 59 inches long, the barometer being at 29·5.

After the tube had been tilted several times so as to allow the air to pass out, the mercury would be suspended as the tube was slowly re-erected, until it had attained an elevation of 40, 50, or sometimes the full height of 60 inches (as shown in fig. 3), but only for a few seconds. When the mercury fell, if the column broke anywhere near the top of the tube, it gave way with a loud click. But this was by no means always the case. The mercury would sometimes separate nearly 30 inches down the tube; and then the

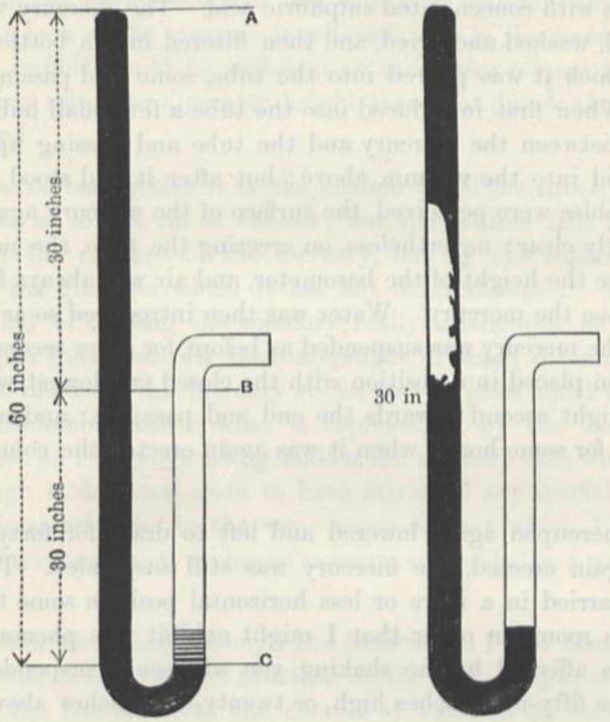


Fig. 3. Fig. 4.

appearance of the upper portion falling was very singular: the upper portion of the column remained intact; and a stream of mercury fell from its under surface, as shown in fig. 4, breaking up into globules as it came into contact with the lower portion, with a loud rattling noise. I was unable to get the column in the tube thus filled to maintain itself for more than twenty or thirty seconds, which failure was clearly due to the presence of air; for after the mercury had fallen a small quantity of air was always found to collect above it. Sometimes, when on inclining the tube the liquid again reached the top, the bubble which remained was so small as to be scarcely visible, although subject to no pressure other than the surface-tension; but its presence always became apparent instantly on erecting the tube. In no case was it possible, after the mercury had once fallen, to get it to remain up to any considerable height above that due to the pressure of the atmosphere until the bubble of air collected had been allowed to pass out.

The tube was then again emptied, washed, and filled with glycerine. This behaved much in the same manner as the water; but the difficulty of getting rid of the air was greater.

Similar results were obtained when very dilute ammonia-liquid was tried.

The tube was then again carefully washed, first with water, and then several times with concentrated sulphuric acid. The mercury was subjected to nitric acid, washed and dried, and then filtered into a bottle of sulphuric acid, from which it was poured into the tube, some acid passing in with the mercury. When first introduced into the tube a few small bubbles could be seen rising between the mercury and the tube and passing up through the sulphuric acid into the vacuum above; but after it had stood for five or six hours no bubbles were perceived, the surface of the mercury against the tube being perfectly clear; nevertheless, on erecting the tube, the mercury would not rise above the height of the barometer, and air was always found to have collected above the mercury. Water was then introduced so as to dilute the acid; then the mercury was suspended as before, for a few seconds only. The tube was then placed in a position with the closed end lowest, so that the air and water might ascend towards the end and pass out; and after being in this position for some hours, when it was again erected the column remained intact.

It was thereupon again lowered and left to drain for forty-eight hours. On being again erected, the mercury was still suspended. The tube has since been carried in a more or less horizontal position some three miles to the Society's rooms in order that I might exhibit this phenomenon. If it has not been affected by the shaking, you will see a suspended column of mercury some fifty-nine inches high, or twenty-nine inches above the height due to the atmosphere*.

Conclusion.

The difficulty of obtaining a column of mercury thirty inches above the pressure of the atmosphere does not, I think, prove that the limit of the cohesive power of the liquid has been arrived at, or even the limit of the adhesive power of the water for glass and mercury, but simply shows that, although imperceptible, there are still bubbles of air in the liquid between the mercury and the glass which will not readily pass out.

It seems to me to be probable that, with sufficient care, or by using apparatus more suitable to the purpose, much greater heights might be attained. But however this may be, we have proof that mercury and water

* At the Meeting not only did the mercury remain suspended when the tube was erect, but on the pressure of the atmosphere being removed with an air-pump it still remained suspended, although the tension at the top of the tube was nearly equal to two atmospheres.

will, by their cohesion, resist a tension of at least one atmosphere, or that the common pump would, if the water were free from air, raise water by suction to a height of more than sixty feet. At first sight it cannot but appear remarkable that such a fact should for so long have escaped notice; but a little consideration removes the difficulty.

Water is almost always more or less saturated with air, which separates into bubbles as soon as the pressure is relieved; and in the common pump a single minute bubble would be sufficient to cause the column to break and prevent it being raised to a greater height than that due to the pressure of the atmosphere.

In the case of barometers it is the custom to fill the tubes full and boil the mercury, so as to get rid of the air; but the column falls to the usual height not by the rupture of the mercury, but by the separation of the mercury from the glass, for which it has but little adhesion. Whether the ordinary method of boiling the mercury really disengages all the air is, I think, an open question. In vacuum-gauges of small diameter it is not uncommonly found that the mercury sticks to the glass until the pressure has fallen considerably below what is represented by the height of the mercury, so that on the gauge being shaken the mercury falls with a sudden drop. Although it does not seem to have attracted any special notice, this phenomenon is clearly due to the same cause as that which I have found capable of maintaining thirty inches of mercury suspended in a comparatively large tube.

It would seem then that, although the facts which I now bring before the Society have little bearing on the practical limits to the height of the column of mercury in the barometer or the column of water in the common pump, they show that these limits are owing to the presence of air or some other minor disturbing cause, and are not, as seems to have been hitherto supposed, owing to the want of cohesion of the liquid. And it seems to me that the cohesion now found to exist occupies an important as well as an interesting place in the properties of liquids.

APPENDIX (26th April).—*Previous Notices of the Cohesion of Liquids.*

Besides the hanging of mercury in small gauges, another phenomenon, which has long been known, shows a small degree of cohesion in water; that is, that water will rise up small tubes by capillary attraction as well in the receiver of an air-pump as in air at the ordinary pressure. This fact was shown before the Royal Society by Robert Hooke.

Prof. Maxwell, in his *Treatise on the Theory of Heat*, p. 259, after commenting on the fact that water has been raised to a temperature of 356° F., without boiling, remarks:—"Hence the cohesion of water must be able to support 132 lbs. weight on the square inch," from which it would appear that he recognizes cohesion as a property of water, and considers that the possibility of raising the temperature above the boiling-point is evidence of such cohesion; but I am not aware that he has anywhere given his reasons for such a conclusion.

I am indebted to Dr Bottomley for reference to a paper in the *Ann. de Chim. et de Phys.* (3) XVI. 167, by M. F. Donny, in which M. Donny gives an account of experiments in which he found that columns of sulphuric acid could be suspended *in vacuo* to a height of 1·3 mètre (about 50 inches), showing a tension of about 7 inches of mercury, care having been taken first to remove all the air from the acid. M. Donny further describes experiments made with water in exhausted tubes, in which he showed the effect of cohesion by shaking the tube. M. Donny does not, however, appear to have thought of the plan which I adopted of making mercury adhere to the tubes by wetting them with sulphuric acid or water. Not being able to use mercury, the tensions which he obtained were comparatively small; and although he seems to have considered that greater tensions might be obtained, he mentions one or two atmospheres as probably possible. It would therefore appear that he had not conceived the possibility of the cohesion of liquids being comparable with that of solids.

M. Donny appears to have been influenced in adopting this limit to his idea of cohesion by a passage from Laplace, *Mécanique Céleste*, Supplément au Xe livre, p. 3, which he quotes.

Laplace, who was the first to investigate systematically the phenomena of capillary attraction, proceeded on the hypothesis that the molecules of a liquid exercise attraction for each other at insensible distances only; and from this assumed attraction he deduces the surface-phenomena. The entire passage quoted by M. Donny is too long to introduce here; but the gist of it is comprised in the following extract:—

"*Son expression analitique est composée de deux termes: le premier, beaucoup plus grand que le second, exprime l'action de la masse terminée par une surface plane; et je pense que de ce terme dependent la suspension du mercure dans un tube du baromètre à une hauteur deux ou trois fois plus grande que celle qui est due à la pression de l'atmosphère, le pouvoir réfringent de corps diaphanes, la cohésion, et généralement les affinités chimiques.*"

Laplace here speaks of the suspension of mercury to 60 or 90 inches as if it were a well-known phenomenon; but I cannot find any reference to experiments, or, indeed, any further mention of the phenomenon in his memoir.

I did not refer to Laplace in the first instance, although I knew well that it is to him we are indebted for the theory of surface-tension almost in the form now accepted, because I wished to avoid all reference to molecular hypothesis, and particularly the molecular attractions assumed by Laplace, lest it might in any way appear as if the conclusion that continuous liquids are as capable of resisting tension as solids (at which I arrived simply from considering the phenomena of surface-tension) were based on such assumptions. I was not aware, however, that Laplace had at all inferred or attempted to apply his theory to prove the ability of liquids to resist great tensions; nor do I find, on again reading his memoir, that he anywhere, with the exception of the almost casual reference quoted above, treats of such a property of liquids. His purpose appears to have been solely to explain the phenomena of capillarity. It appears obvious, moreover, that his line of reasoning must have forced upon his notice the conclusion that, according to his hypothesis, liquids ought to possess the property of very great cohesion; so that from the extremely slight notice which he has accorded to this property, one can only infer that he was not completely convinced of its existence.

32.

ON THE STEERING OF SCREW STEAMERS.

Report of the Committee, consisting of JAMES R. NAPIER, F.R.S., Sir W. THOMSON, F.R.S., W. FROUDE, F.R.S., J. T. BOTTOMLEY, *and* OSBORNE REYNOLDS, F.R.S. (Secretary), *appointed to investigate the effect of Propellers on the Steering of Vessels.*

[*From the* "Report" *of the* "British Association," 1878.]

SINCE the Meeting of the British Association held in Plymouth last year, the Committee have had the satisfaction of receiving reports of the trials of various English and foreign steamers, made by the owners and officers of the steamers, without any further instigation from the Committee than that contained in their circulars. These reports all show that those by whom the trials were made have become convinced of the importance of the facts which they have observed. And, indeed, the mere fact of the trials having been undertaken shows that the importance of the effect of the reversed screw on the steering while the ship is stopping herself is beginning to be recognised. This is further shown by the fact that one of the trials was undertaken at the instance of the Court of Mr Stipendiary Yorke, in order to ascertain if the captain of the s.s. 'Tabor' had been justified in starboarding his helm in order to bring his vessel round to starboard after his screw was reversed.

All these trials, without a single exception, confirm the results obtained in the previous trials made by the Committee. But this is not the most important purpose which this year's trials serve. For, as regards the general effect of the reversed screw on the action of the rudder, the trials already reported, particularly those of the 'Hankow' (see last year's *Report*, p. 201), are conclusive, and leave nothing to be desired. But the previous trials were all made with fast vessels at their full draught, their screws being well covered, and the conditions of the weather being most favourable. The trials this year, on the other hand, appear, for the most part, to have been made with vessels in light trim; and in two instances the wind was blowing with considerable force. The result of these circumstances on the behaviour of the vessels is very decided, and coincides remarkably with the effects deduced by Professor Reynolds from his experiments on models (see *Report*, 1875, I. p. 145), viz., that when the screw is not deeply immersed and froths the water, it exerts, when reversed, considerable influence to turn the vessel independently of the rudder; the vessel turning to starboard or port, according as the screw is right or left handed, which effect (and this seems to be the point most generally unknown) nearly disappears when the screw is so deeply immersed that it does not churn air with the water.

Neither the Admiralty, the Board of Trade, nor the Elder Brethren of Trinity House have taken any further notice of the results communicated to them by the Committee.

The Marine Board of South Shields has, however, taken considerable interest in the question, and has invited captains to make trials, and Mr J. Gillie, the Secretary, was present at the trial of the 'Tabor' ordered by the Court, and reported the results to the Committee.

There have been numerous collisions during the year. In almost all cases the practice of reversing the screw has been adhered to. In many, if not in all instances where this has been done, the evidence goes to show that the vessel in which the screw was reversed did not turn in the direction in which those in charge of her were endeavouring to turn her. In two important cases this fact was fully apparent even to those in charge of the vessel. And in one instance the owners and captain of the vessel attributed the failure to steer to its true cause, namely, the reversal of the screw; although in both cases those immediately in charge of the vessels contended that the rudder was not handled according to their directions.

The first case was that of the 'Menelaus' and the 'Pilot' schooner on the Mersey. The 'Menelaus' was in charge of a first-class pilot, and this steamer, in broad daylight, ran into and sank the 'Pilot' schooner, which was dropping up the river with the tide. The pilot in charge contended that, owing to the wheel chains having got jammed, his orders were not attended to. The jamming of the chains was denied by the owners, and the fact that they subpœnaed the Secretary of the Committee to give evidence at the trial may be taken to indicate the cause to which they attributed the collision. The case, however, was only in part heard, for after the evidence for the plaintiffs a compromise was effected, and the pilot withdrew all assertion that the wheel chains had been jammed, thus admitting that the failure to steer had been brought about by the reversal of the screw.

The other case is the well-known accident to the 'Kürfurst.' In this it is admitted that the order was to starboard the helm and reverse the screw of the 'König Wilhelm,' and this order was avowedly given with the view of bringing the vessel round to port. All the experiments of this Committee, however, go to prove that with a reversed screw and a starboard helm such a vessel as the 'König Wilhelm' would have turned to starboard rather than port. This was what, according to all the evidence, did actually happen, and was the final cause of the catastrophe. But it appears that those in charge of the 'König Wilhelm' arrived at the conclusion that the men at the wheel (and these would be many), although they all aver that they heard the order and obeyed it, in reality turned the wheel the wrong way. Considering, therefore, that it was not one man but a number of men at the wheel, and that the vessel behaved exactly as she would have behaved had the order been obeyed, as the men say it was, the conclusion of the Court seems to be most improbable, and, for the sake of future steering, most unfortunate.

The Committee are now of opinion that the work for which they were originally brought together has been fully accomplished. The importance of the effect of the reversed screw on the action of the rudder has been fully established, as well as the nature of its effect completely ascertained. Also for two years the Committee have urged the results of their work upon the attention of the Admiralty, and the various marine boards, and although they regret that as yet they have failed to obtain the general recognition of the facts brought to light which their vital importance demands, they consider that this will surely follow, and that as a Committee they can do no more than publish the reports of the trials, and the conclusions to which they have been led.

Full accounts of the experiments made previously to this year have been given in the two previous Reports, and those which the Committee have received this year are given at length at the end of this Report. The following is a summary of the conclusions which have been established; and it is interesting to notice that the conclusions drawn by Professor Reynolds from experiments on models have been fully confirmed by the experiments on full-sized ships:—

Summary of the Results of the Trials of the Effect of the Reversed Screw on the Steering during the time a vessel is stopping herself.

It appears both from the experiments made by the Committee, and from other evidence, that the distance required by a screw steamer to bring herself to rest from full speed by the reversal of her screw is independent, or nearly so, of the power of the engines, but depends on the size and build of the ship, and generally lies between four and six times the ship's length. It is to be borne in mind that it is to the behaviour of the ship during this interval that the following remarks apply.

The main point the Committee have had in view has been to ascertain how far the reversing of the screw in order to stop a ship did or did not interfere with the action of the rudder during the interval of stopping; and it is as regards this point that the most important light has been thrown on the question of handling ships. It is found an invariable rule that, during the interval in which a ship is stopping herself by the reversal of her screw, the rudder produces none of its usual effect to turn the ship, but that, under these circumstances, the effect of the rudder, such as it is, is to turn the ship in the opposite direction from that in which she would turn if the screw were going ahead. The magnitude of this reverse effect of the rudder is always feeble, and is different for different ships, and even for the same ship under different conditions of loading.

It also appears from the trials that, owing to the feeble influence of the rudder over the ship during the interval in which she is stopping, she is then at the mercy of any other influences that may act upon her. Thus the wind, which always exerts an influence to turn the stem (or forward end) of the ship into the wind, but which influence is usually well under control of the rudder, may, when the screw is reversed, become paramount and cause the ship to turn in a direction the very opposite of that which is desired. Also the reversed screw will exercise an influence, which increases as the ship's way is diminished, to turn the ship to starboard or port according as it is right or left handed; this being particularly the case when the ships are in light draught.

These several influences—the reversed effect of the rudder, the effort of the wind, and the action of the screw—will determine the course the ship takes during the interval of stopping. They may balance, in which case the ship will go straight on, or any one of the three may predominate and so determine the course of the ship.

The utmost effect of these influences, when they all act in conjunction, as when the screw is right-handed, the helm starboarded, and the wind on the starboard side, is small as compared with the influence of the rudder as it acts when the ship is steaming ahead. In no instance has a ship tried by the Committee been able to turn with the screw reversed on a circle of less than double the radius of that on which she would turn when steaming ahead. So that, even if those in charge could govern the direction in which the ship will turn while stopping, she turns but slowly; whereas in point of fact those in charge have little or no control over this direction, and unless they are exceptionally well acquainted with the ship, they will be unable even to predict the direction.

It is easy to see, therefore, that if on approaching danger the screw be reversed, all idea of turning the ship out of the way of the danger must be abandoned. She may turn a little, and those in charge may know in which direction she will turn, or may even by using the rudder in an inverse manner be able to influence this direction, but the amount of turning must be small, and the direction very uncertain.

The question, therefore, as to the advisability of reversing the screw is simply a question as to whether the danger may be better avoided by stopping or by turning; a ship cannot do both with any certainty.

Which of these two courses it is better to follow, must depend on the particular circumstances of each particular case, but the following considerations would appear to show that when the helm is under sufficient command there can seldom be any doubt.

A screw steam-ship when at full speed requires five lengths, more or less, in which to stop herself; whereas by using her rudder and steaming on at full speed ahead, she should be able to turn herself through a quadrant, without having advanced five lengths in her original direction. That is to say, a ship can turn a circle of not greater radius than four lengths more or less (see 'Hankow,' 'Valetta,' 'Barge'); so that, even if running at full speed directly on to a straight coast, she should be able to save herself by steaming on ahead and using her rudder after she is too near to save herself by stopping; and any obliquity in the direction of approach, or any limit to the breadth of the object ahead, is all to the advantage of turning, but not at all so to stopping.

There is one consideration, however, with regard to the question of stopping or turning which must, according to the present custom, often have weight, although there can be but one opinion as to the viciousness of the custom. This consideration is the utter inability of the officers in charge to make any rapid use of the rudder so long as their engines are kept on ahead. It is no uncommon thing for the largest ships to be steered by as few as two men. And the mere fact of the wheel being so arranged that two men have command of the rudder, renders so many turns of the wheel necessary to bring the rudder over that, even where ready help is at hand, it takes a long time to turn the wheel round and round so as to put a large angle on the rudder.

The result is that it is often one or two minutes after the order is heard before there is any large angle on the rudder, and of course under these circumstances it is absurd to talk of making use of the turning qualities of a ship in case of emergency. The power available to turn the rudder should be proportional to the tonnage of the vessel, and there is no mechanical reason why the rudder of the largest vessel should not be brought hard over in less than fifteen seconds from the time the order is given. Had those in charge of steam-ships sufficient control over the rudder, it is probable that much less would be heard of the reversing of the engines in cases of imminent danger.

Reports of the Trials of this Year.

"S.s. 'North-Western,'
February 7, 1878.

"Right-handed screw. Speed of ship 13 knots. Signalled to engine-room 'Stop.' 'Full speed astern' 20 seconds after first order. Engines moving astern. Helm put hard a-starboard. Head commenced moving to starboard and went from N. 20 E. to N. 50 E. in $1\frac{1}{2}$ minute. The vessel had by this time stopped going through the water. We then got up full speed ahead, stopped, put the helm hard a-port, and reversed full speed. The vessel had stopped going ahead in $1\frac{1}{2}$ minute, and the head had gone to starboard from N. 30 E. to N. 50 E. At $2\frac{1}{4}$ minutes the head stopped going to starboard, and at $2\frac{1}{2}$ minutes the ship's head was going to port. The vessel was going astern through the water before her head stopped going to starboard.

"The draught of water was 9 feet 2 inches and 12 feet 10 inches. The centre of propeller is 7 feet 1 inch above bottom of keel, and the propeller is 13 feet in diameter, so that the top of the blade was 9 inches out of the water.

"W. Bottomley, Jun."

Remarks by the Committee.

The screw of this vessel being right-handed, its tendency when reversed would be to bring the vessel's head to starboard, and, owing to the screw being partially out of water, this tendency would be considerable. Accordingly we find that the direct effect of the screw prevailed over the influence of the rudder, and when the screw was reversed the vessel turned to starboard for all positions of the helm. The reversed effect of the rudder was, however, very apparent, for the vessel went to starboard while stopping much faster with the helm starboarded than with the helm ported.

The same phenomena exactly will be seen in the trials of the next four vessels.

Kongl. Gieenska Norsk General Consulatet i Stettin.

"Stettin, *May* 11, 1878.

"Sir,—Being a subscriber to the *Navy* I perused an article in No. 124, vol. v., of that journal (Oct. 7, 1876) regarding experiments on the turning of screw steamers.

"The same inspired me with great interest in the matter it treats of, and caused me to instruct the captains of my three steamers, 'Martha,' 'Marietta,' 'Susanne' (of which I subjoin the necessary particulars at foot), to make the experiments in question. This has been done, and the results obtained communicated to the Nautical Associations here and at other German ports. Being indebted to you, as the promoter of these experiments, for the idea, I consider it my duty to acquaint you with the results of the experiments made by my captains, and venture to enclose a translation of the report on same. I need not state that any comments you might favour me with, or a few lines stating whether the conclusions arrived at correspond to your own, would be most highly esteemed.

"I am, Sir, your most obedient,

"T. IVERS."

"Professor Reynolds, Manchester."

"*On the Steering of Steamships with Right-handed Screws, when the vessel is going ahead, but her engines reversed.*

"The experiments made by Professor Reynolds, of Manchester, in reference to the correct steering of screw steamships, when going ahead with the propeller working astern, and the results of the trials made with the steamer 'Melrose,' which have been published in the *Glasgow News*, have induced us, the undersigned, to try the three chief manœuvres in question with the steamers which we command. We subjoin a statement of the results obtained, accompanied by sketch and explanation.

"As screw steamers differ from each other in respect of model, construction, and size of propeller and helm, draught of water, &c., there is naturally a difference in the degree in which they deviate from a straight course when making these movements. We would, therefore, recommend every master to make experiments with his ship, with a view to ascertaining in what way the helm should be handled in all conceivable emergencies.

"I. Ship going ahead, propeller working astern, rudder amidships.

"*Result.* The stern turns to the right.

"*Explanation.* The rotation of the screw to the left presses the stern to the left, and consequently the stem to the right; the helm, which is amidships, is thus neutralised, and must be regarded merely as a prolongation of the ship.

"II. Ship going ahead, the screw working backwards and the helm hard a-starboard.

"*Result.* The stern turns very decidedly to the right.

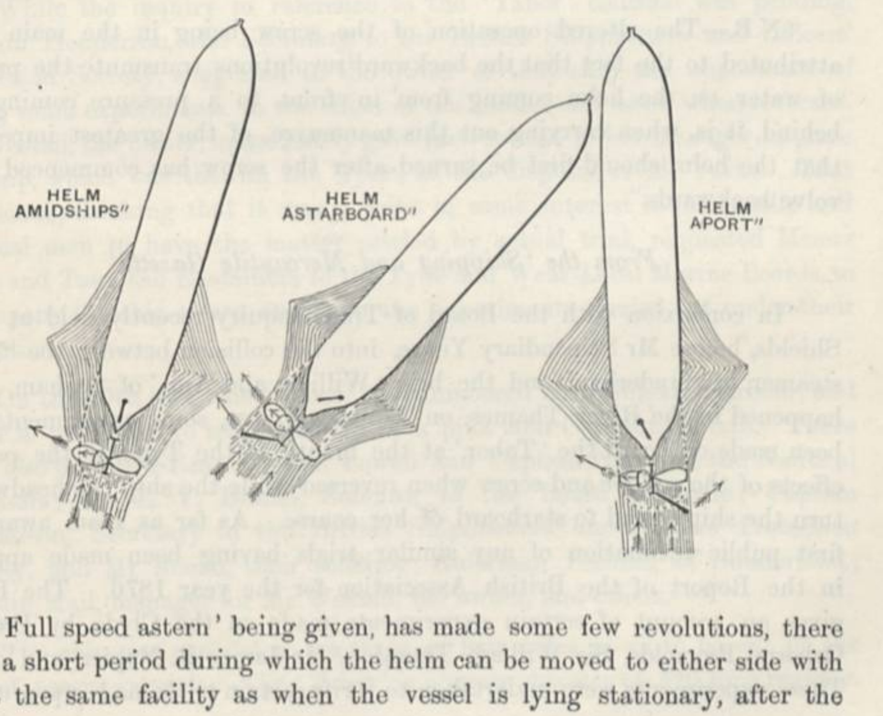
"*Explanation.* The rotation of the screw to the left presses the stern to the left, and consequently the stem to the right; the starboarded helm is subjected to a pressure of water from behind, so that the after part of the ship is doubly impelled to the left, the fore part being also with correspondingly greater force pressed to the right.

"III. The ship going ahead, propeller working astern, helm hard a-port.

"*Result.* The stern inclines slightly to the left.

"*Explanation.* The rotation of the screw to the left presses the after part of the ship to the left; on the other hand, the ported helm is under a pressure of water from behind which impels the stern to the right. The operation of these two forces being of opposite nature, the vessel only deviates slightly from the straight course, and then mostly with the stern to the left.

"*But as soon as headway is lost* the force of the screw asserts itself so much more that, in spite of the helm being ported, the stern turns to the left and the bow to the right; they turn in the same directions in a stronger degree when the helm is amidships, and strongest of all when the helm is starboarded. From the moment the ship begins to go astern the screw must be regarded as the fore end or bow of the ship. After the propeller, on the



order 'Full speed astern' being given, has made some few revolutions, there comes a short period during which the helm can be moved to either side with almost the same facility as when the vessel is lying stationary, after the expiration of which it almost immediately becomes necessary to use considerable force to work the helm. This is due to the circumstance that immediately after the screw has made the first backward revolutions the pressure of water in front on the helm ceases, causing so-called deadwater at the helm; whereas an increasing pressure of water on the helm from behind results as soon as the backward revolutions of the screw begin to gain in rapidity. The pressure on the rudder from behind, in spite of the vessel's headway, is presumably to be attributed to the fact that the masses of water thrown forward by the screw must immediately be replaced, causing a correspondingly powerful suction, and consequently a current of water acting on the rudder. The conflict between the contending masses of water is clearly visible also on the surface on both sides of the ship (particularly on the right side).

"We beg to hand these results of our observations to the Nautical Association of this town for distribution amongst other masters, and trust that they will likewise communicate the result of their experiments with their respective ships, in order that the data thus collected may be of service to the seafaring community.

"(Signed)	J. SCHÜTZ,	Master of the	s.s. 'Susanne.'
„	F. WILKE,	„	s.s. 'Marietta.'
„	C. STREECK,	„	s.s. 'Martha.'

"N.B.—The altered operation of the screw being in the main to be attributed to the fact that the backward revolutions transmute the pressure of water on the helm coming from in front to a pressure coming from behind, it is, when carrying out this manœuvre, of the greatest importance that the helm should first be turned after the screw has commenced to revolve backwards."

From the 'Shipping and Mercantile Gazette.'

"In connexion with the Board of Trade inquiry recently held at South Shields, before Mr Stipendiary Yorke, into the collision between tbe 'Tabor' steamer, of Sunderland, and the brig 'William and Ann,' of Seaham, which happened in the River Thames, on January 25 last, some experiments have been made on board the 'Tabor,' at the mouth of the Tyne, on the peculiar effects of the rudder and screw when reversed while the ship has headway, to turn the ship's head to starboard of her course. As far as I am aware, the first public intimation of any similar trials having been made appeared in the Report of the British Association for the year 1876. The Report gives an account of certain experiments made on the Clyde by Professor Osborne Reynolds, Sir William Thomson, Mr James R. Napier, and others. These experiments were undertaken to verify certain trials made upon models by Professor Reynolds, which he had brought under the notice of the Association the previous year at Bristol. An able article on the subject, from the pen of Sir Travers Twiss, appeared in the *Nautical Magazine*, last year; so that nautical men should now be aware that they cannot always depend upon the action of the rudder and screw when reversed while the ship has headway.

"Almost all experienced officers of the navy and merchant service are doubtless well acquainted with the manœuvring of screw vessels under steam, but it is thought that junior officers and others might not have had opportunities of acquiring information of this kind. There is a marked difference in the results observed on the Clyde and those noticed at the mouth of the Tyne, inasmuch as in none of the trials detailed below did the ship's head swing to port of her course, but invariably to starboard. I would have preferred to wait until these differences had been either accounted for or explained by further experiments before placing the facts before your readers; but an imperfect account of the trials having appeared in the local newspapers, it was thought desirable to publish the facts just as they were obtained, and to reserve for a future letter further remarks, and a description of a series of similar experiments made by direction of the Local Marine Board on board the 'Cervin' steamer, off the Tyne in August last year.

"While the inquiry in reference to the 'Tabor' collision was pending, Captain Henderson, the Secretary to the British Shipmasters' and Officers' Protection Society, suggested to the owner of that ship the importance of trying some experiments on the effect of the rudder and screw when reversed. Mr Westall, the owner, immediately gave instructions to his manager to place the ship, which was then in the Tyne, at the disposal of Mr Yorke. That gentleman, thinking that it was a point of some interest to mercantile and nautical men to have the matter settled by actual trial, requested Messrs Gillie and Tate, the Examiners to the Tyne and Wear Local Marine Boards, to accompany the ship to sea, and have the experiments carried out under their inspection.

"On the 19th inst., the 'Tabor' was unmoored from Shields Harbour, and at 2 P.M. proceeded to sea in charge of a pilot and Captain Mankin. There were also on board Rear-Admiral Powell and Captain Nicholas, the Nautical Assessors; Mr L. V. Hamel, Solicitor to the Board of Trade; Captain Henderson, Secretary to the British Shipmasters' and Officers' Protection Society, and Mr Roche, their solicitor; Alderman Peckett, of Sunderland; Captain Mail, manager for Mr Westall, the owner, and others.

"The ship was run out to sea three or four miles so as to be out of the way of passing vessels. The 'Tabor' is a screw steamer of 520 tons register.

Her length between perpendiculars is 208 feet; breadth, 27·8 feet; depth, 14·8 feet. She is propelled by two engines of 90-horse power combined, and at the time of the trial had in about 200 tons of water ballast. Her draught of water forward was 6 feet 6 inches, and aft 10 feet 4 inches, she being nearly in the same trim as she was at the time the collision happened. Her screw is right-handed, and four blades; diameter, 12 feet; pitch, 17 feet. The top of the blade was about 2 feet out of the water when the blade was parallel with the sternpost. The direction of the wind was S. by W. ½ W.; force 4 to 5. The sea was perfectly smooth. The weather being a little hazy, and the marks upon the land indistinct, a dumb card could not be used to measure the angles made by the ship's head, but the bridge compass, being in excellent condition and not sluggish, was used for this purpose. Mr Tate and Mr Hamel noted the time, the change in the ship's head was observed by Mr Gillie, who also took down the notes, and Mr Roche noted the time it took to stop and reverse the engines. The ship's head previous to commencing the whole of the trials was steadied at W.S.W., and the engines were kept going full speed ahead, the ship making 8 knots an hour as shown by the patent log. The fore and main trysails were set during trials 1 and 2; there was no canvas set during trials 3, 4, and 5.

"Trial No. 1 (helm hard a-starboard).—At 3.43 P.M., while the ship was going full speed ahead, the order was given to stop and reverse the engines to full speed astern, at the same time the helm was put hard to starboard, both operations being done simultaneously, and completed in 12 seconds from the time of the order being given. In 40 seconds ship's head fell off to starboard of W.S.W. 10 degrees; in 1 minute 30 seconds ship's head fell off to starboard of W.S.W. 34 degrees; in 2 minutes 45 seconds ship's head fell off to starboard of W.S.W. 67 degrees, and the ship's way through the water ahead was completely stopped.

"Trial No. 2 (with helm hard a-port), the ship's head being brought to W.S.W., going full speed, all other conditions as in trial 1.—In 40 seconds ship's head fell off to starboard of W.S.W. 12 degrees; in 1 minute 30 seconds ship's head fell off to starboard of W.S.W. 23 degrees; in 2 minutes 45 seconds ship's head fell off to starboard of W.S.W. 42 degrees, and the ship's way through the water ahead completely stopped.

"Trial No. 3 (with helm amidships), fore and main trysails taken in, all other conditions as before.—In 40 seconds ship's head fell off to starboard of W.S.W. 15 degrees; in 1 minute 30 seconds ship's head fell off to starboard of W.S.W. 50 degrees; in 2 minutes 45 seconds ship's head fell off to starboard of W.S.W. 79 degrees, and way stopped.

"Trial No. 4 (with helm hard a-starboard), being trial No. 1 repeated with no canvas set, all other conditions as before.—In 40 seconds ship's head fell off to starboard of W.S.W. 7 degrees; in 1 minute 30 seconds ship's head fell off to starboard of W.S.W. 45 degrees; in 2 minutes 45 seconds ship's head fell off to starboard of W.S.W. 78 degrees, and way stopped.

"Trial No. 5 (with helm hard a-port), being trial No. 2 repeated with no canvas set.—In 40 seconds ship's head fell off to starboard of W.S.W. 16 degrees; in 1 minute 30 seconds ship's head fell off to starboard of W.S.W. 34 degrees; in 2 minutes 25 seconds ship's head fell off to starboard of W.S.W. 45 degrees. At this point the engines were stopped by mistake, but the ship's head appeared to be fixed at W.N.W., and she had very little, if any, way through the water ahead.

"The practical results of these trials, as far as the 'Tabor' is concerned, is to show that when she is going full speed ahead in ballast trim, if her engines are stopped and reversed, her head will go to starboard of the course she is steering. The helm seems to have very little effect, the results obtained with the helm hard a-starboard and when it was amidships being very much alike. With the helm hard a-port the ship's head still went to starboard, but the angle described was much smaller than that made when the helm was amidships or a-starboard. I will not at present trespass any further on your space, but perhaps you will allow me to add that the experiments made with the 'Cervin' steamer, with a draught of 22 feet, go a long way to show that, in ships of her class, similar results to those detailed above will follow under similar circumstances.

"John Gillie."

"Local Marine Board, South Shields,
"February 22, 1878."

Remarks by the Committee.

It will be seen that the results in this case are very similar to those obtained in the case of 'North-Western.' The right-handed screw only partially immersed gave the vessel a strong bias to starboard. But in this case, in addition to the direct effect of the screw, the effect of the wind, which was of force 4 or 5, was to bring the vessel round to windward, which happened in all cases to be to starboard.

In the next vessel reported, the 'Cervin,' it will be seen that the screw was well immersed, and hence would probably exert no great influence when reversed to turn the vessel to starboard. At the commencement of all the trials, however, the wind was blowing with force 5 on the starboard side of the vessel, and the effect of this would be to cause the vessel, as long as she had way on, to turn to windward, and this, it will be seen, is what happened; in every case the vessel's head turned to windward. Here also the reverse influence of the rudder was apparent, for the vessel turned faster to starboard with the helm starboarded than with it ported.

"*August* 25, 1877.

"S.s. 'Cervin,' of South Shields, length, 287 feet; breadth, 34 feet; depth, 24 feet; tonnage, 1913. Propelled by two engines of 180 h.p., combined; screw right-handed, 4 blades; diameter, 14 feet 9 inches; pitch, 17 feet; draught of water, forward 21 feet 4 inches, aft 21 feet 9 inches; top of blade of screw immersed in water, about 5 feet; wind, E.N.E.; force, 5; sea smooth.

"Trial No. 1 (helm hard a-port).—Ship's head N. by W., going full speed ahead, 9½ knots, the engines were stopped and reversed, and helm put hard to port; ship's head came up to N. by E. in 2 minutes, and remained steady on that point; way through the water ahead stopped in 3 minutes 40 seconds.

"Trial No. 2 (helm hard a-starboard).—Ship's head N. by E., it came up to N.E. by E., or 45 degrees, in 4 minutes, and way stopped.

"Trial No. 3 (going fast astern, screw started to drive her ahead, helm a-port).—Head N. by W., fell off to N.N.W. in 1 minute 30 seconds.

"Trial No. 4 (with helm a-starboard).—Head at N.E., fell off to N.N.E. in 2 minutes.

"Trial No. 5 (full speed ahead, helm amidships).—Head N. by W., went slightly towards west, then back to north, in 3 minutes.

"J. Gillie."

For continuation, see paper 37.

33.

ON CERTAIN DIMENSIONAL PROPERTIES OF MATTER IN THE GASEOUS STATE.

[*From the* "Philosophical Transactions of the Royal Society." Part II. 1879.]

(*Read* February 6, 1879.)

Part I. *Experimental Researches on Thermal Transpiration of Gases through Porous Plates and on the Laws of Transpiration and Impulsion, including an Experimental Proof that Gas is not a continuous Plenum.*

Part II. *On an Extension of the Dynamical Theory of Gas, which includes the Stresses, Tangential and Normal, caused by a Varying Condition of Gas, and affords an Explanation of the Phenomena of Transpiration and Impulsion.*

PART I. (EXPERIMENTAL).

SECTION I. INTRODUCTION.

1. THE motion of gases through minute channels, such as capillary tubes, porous plugs, and apertures in thin plates, has been the subject of much attention during the last fifty years. The experimental study of these motions, principally by Graham*, resulted in the discovery of several important properties of gases. And it is largely, if not mainly, as affording an explanation of these properties that the molecular theory has obtained such general credence.

It does not appear, however, that either the experimental investigations of these motions or the theoretical explanations of the properties revealed have hitherto been in any sense complete.

There exists a whole class of very marked phenomena which have escaped the notice of Graham and subsequent observers; while several of the most

* *Edin. Phil. Trans.*, 1831; *Phil. Trans.*, 1846 and 1863.

marked and important facts discovered by Graham have hitherto remained unconnected by any theory.

2. Amongst the best known of the phenomena is the difference in the rates at which different gases transpire through minute channels, and the consequent difference of pressure which ensues when two different gases initially at the same pressure are separated by a porous plate. It does not appear, however, that hitherto any attempt has been made to ascertain the existence of what may be considered a closely analogous phenomenon—that a difference of temperature on the two sides of the plate might cause gas, without any initial difference of pressure or any difference in chemical constitution, to pass through the plate—nor am I aware that such a result from a difference of temperature has been in any way surmised (see Appendix, note 1).

I have, however, now ascertained, by experiments which will be described at length, that a difference of temperature may be a very potent cause of transpiration through porous plates. So much so that with hydrogen on both sides of a porous plate, the pressure on one side being that of the atmosphere, a difference of 160° F. (from 52° to 212°) in the temperature on the two sides of the plate secured a permanent difference in the pressure on the two sides equal to an inch of mercury; the higher pressure being on the hotter side. With different gases and different plates various results were obtained, which are however, as will be seen, connected by definite laws.

I propose to call the motion of the gas caused by a difference of temperature *Thermal Transpiration* (see Appendix, note 2).

3. Again, although Graham found that he obtained not only very different results but also very different laws of motion with plates of different coarseness or with plates and capillary tubes*, neither he nor any subsequent observer appears to have followed up this lead. As regards Graham this appears to me to be somewhat surprising. For although he may have considered the mere difference in the results to have been analogous to the difference found by Poiseuille for liquids, it would seem as though the difference in the laws of motion which he obtained should have excited his curiosity; and then, as he was avowedly of opinion that gas is molecular, he could hardly have failed to observe that so long as the distance separating the molecules in the gas bore a fixed relation to the breadth of the openings in his plates, he should have had the same laws of motion. This view, however, appears to have escaped him as well as all subsequent observers. Otherwise it would have been seen that with a simple gas such as hydrogen, similar results must be obtained so long as the density of the gas is inversely proportional to the lateral dimensions of the passages through the plates.

* *Phil. Trans.*, 1863.

By experiments, to be described, I have now fully established this law. I find that with different plates similar results are obtained when the densities of the gas with the different plates bear a fixed ratio; and this is the case whatever may be the cause of the transpiration, *i.e.*, a difference of temperature or a difference of pressure (a difference of gas I have not investigated, as it was obviously unnecessary to do so). Thus with two plates, one of stucco and the other of meerschaum, similar results of transpiration caused by pressure were obtained when the densities with the plates were respectively as 1 to 5·6, both with hydrogen and air and at pressures ranging from 30 to 2 inches of mercury. Also with the same two plates similar results of thermal transpiration were obtained when the densities were respectively as 1 to 6·5 both for air and hydrogen, and through a range of pressures from 30 to ·25 inches of mercury. The discrepancies of 5·6 and 6·5 were in all probability owing to a slightly altered condition of the plates (see Appendix, note 4).

This correspondence of the results at corresponding densities holds, although the law of motion changes. Thus with air at 30 inches the law was the same as that obtained by Graham for stucco plates, while at the smallest pressures (·25 inch) it was nearly the same as he found for graphite plates or apertures in thin plates.

4. Having established this law of corresponding results at corresponding densities, it became apparent that the results obtained with plates of different coarseness, and with the same plates but different densities of gas, also followed a definite law. This law, which admits of symbolical expression, shows that there exists a definite relation between the results obtained, the lateral dimensions of the passages, and the density of the gas.

This law is important as reconciling results which have hitherto appeared to be discordant, such as Graham's results with plates of different coarseness, and as tending to complete the experimental investigation; but it has another and a more general importance.

It may not appear at first sight, but on consideration it will be seen that this law amounts to nothing less than an absolute experimental demonstration that gas possesses a heterogeneous structure—that it is not a continuous plenum of which each part into which it may be divided has the same properties as the whole.

It would appear that Graham must have had this proof, so to speak, under his eyes, and it is strange that both he and subsequent observers have overlooked it. It seems possible, however, that they were not alive to the importance of such a demonstration. It is now so generally assumed that gas does possess molecular structure that the weakness of the evidence on which the assumption is based and the importance of further proof are points that are apt to escape notice.

The importance of an experimental demonstration that gas possesses molecular structure.

5. The idea of molecular gas does not appear to have originated from the recognition of properties of gas which were inconsistent with the idea of a continuous plenum, but from a wish to reconcile the properties of gas with the properties of other substances, or more strictly with some general property of matter. And the general conviction which may be said to prevail at the present time is owing to the simplicity of the assumptions on which the molecular hypothesis is based, and the completeness with which many of the properties of gases have been shown to follow from the molecular hypothesis.

But it will be readily seen that however simple may be the assumptions of the kinetic theory, and however completely the properties of gases may be shown to follow from these assumptions, this is no disproof of the possibility that gas may be a continuous substance, each elementary portion of which is endowed with all the properties of the whole, and unless this is disproved there may exist doubt as to the necessity for the kinetic theory.

Any direct proof, therefore, that gas is not ultimately continuous altogether alters the position of the molecular hypothesis.

The sufficiency of the demonstration that gas is not structureless.

6. In order to prove that gas is not continuous it is not necessary that we should be able to perceive the actual structure; we have only to find some property of a certain quantity of gas which can be shown not to be possessed by all the parts—some property which is altered by a re-arrangement of the parts.

Hitherto I believe that no such property has been recognised, or at all events the conclusions to be drawn from such a property have not been recognised. The phenomena of transpiration as well as those of the radiometer depend on such properties, but these properties have not been sufficiently understood to bring out the conclusion. This conclusion however follows directly from the law indicated in Art. 4, viz.: that the results of transpiration and impulsion depend on the relation between the size of the internal objects and the density of the gas.

The force of this reasoning will be better seen after the results of the experiments have been described, but it is introduced here to show the importance which attaches to what otherwise might be considered secondary properties of gases.

To these properties I must now return, not having yet indicated how I was led to make the experiments, and besides those already mentioned there remains an important class of phenomena to be noticed.

The results deduced from theory.

7. Although the existence of the phenomena of thermal transpiration, and the existence of the law of corresponding results at corresponding densities have been verified by experiments, they were not so discovered.

They followed from what appeared to me to be a successful attempt to complete the explanation I had previously given* of the forces which must result when heat is communicated from a surface to a gas, and the phenomena of the radiometer.

Having found, what I had not at first perceived, that according to the kinetic theory the force resulting from the communication of heat to a gas must depend on the surface from which it is communicated being of limited extent, and must follow a law depending on some relation between the mean path of a molecule and the size of the surface, it appeared that by using vanes of comparatively small size the force should be perceived at comparatively greater pressures of gas (see Appendix, note 3).

On considering how this might be experimentally tested, it appeared that to obtain any result at measurable pressures the vanes would have to be very small indeed; too small almost to admit of experiment. And it was while thinking of some means to obviate this difficulty that I came to perceive that if the vanes were fixed, then instead of the movement of the vanes we should have the gas moving past the vanes—a sort of inverse phenomenon—and then instead of having small vanes, small spaces might be allowed for the gas to pass. Whence it was at once obvious that in porous plugs I should have the means of verifying these conclusions. I followed up the idea, and by a method which I devised of taking into account the forces, tangential and normal, arising from a varying condition of molecular gas, I was able to deduce what appears to me to be a complete theory of transpiration.

This theory appears to include all the results established by Graham, as well as the known phenomena of the radiometer, which for the sake of shortness I shall call the *phenomena of impulsion*. I was also able definitely to deduce the results to be expected, both as regards thermal transpiration, and the law of corresponding densities, both for transpiration and impulsion.

Having made these deductions, I then commenced the experiments on transpiration, which so completely verified my theoretical deductions that I have been able to produce the theory in its original form, with some additions, but without any important modification.

Moreover, having succeeded (not without some trouble) in rendering apparent the effect of differences of temperature in causing gas to move through fine apertures, I recurred to the original problem, and by suspending fibres of silk and spider lines to act as vanes, I have now succeeded in directly

* *Proc. Roy. Soc.*, 1874, p. 402 (paper 11).

verifying the conclusion that the pressure of gas at which the force in the radiometer becomes apparent varies inversely as the size of the vanes. With the fibre of silk I obtained repulsion at pressures of half an atmosphere.

The arrangement of the paper.

8. My object is to describe the reasoning by which I was led to undertake the experiments as well as the experiments themselves; but as the theory will be better understood after acquaintance with the facts, I have inverted the natural order and given the experiments first. And in order that the reader may not be at a disadvantage in reading the accounts of the experiments, I include here a somewhat fuller account of the results to be expected as deduced from the theory which is to follow.

The Laws established by the experiments.

9. Law I. When gas exists at equal pressures on either side of a porous plate across which the temperature varies, the gas will transpire through the plate from the colder to the hotter side, with velocities depending on the absolute temperature and chemical nature of the gas, the relation between the density of the gas and the fineness of the pores, the thinness of the plug, and the difference of temperature on the two sides of the plate.

Law II. In order to prevent transpiration through the plate, the pressure on the hotter side must be greater than the pressure on the colder side. This difference of pressure will depend on the chemical nature of the gas, the mean pressure of the gas, the absolute temperature, the relation between the size of the pores and the density of the gas, and the difference of temperature on the two sides of the plate, but not on the thickness of the plate.

Law III. For the same plate and the same difference of temperature, when the gas is sufficiently dense, the difference of pressure is approximately proportional to the inverse density, but as rarefaction proceeds this law gradually changes, the increase in the difference of pressure becomes less and less until that difference reaches a maximum and begins to diminish, then on further rarefaction this diminution increases until the difference of pressure becomes approximately proportional to the density of the gas.

Law IV. After the rarefaction has reached that point at which the difference in pressure is nearly proportional to the density, then the difference in pressure will bear to the greatest pressure the ratio which the difference in the square roots of the absolute temperature bears to the square root of the greatest absolute temperature, or if A and B indicate the two sides of the plate,

$$\frac{P_A - P_B}{P_A} = \frac{\sqrt{\tau_A} - \sqrt{\tau_B}}{\sqrt{\tau_A}},$$

where P and τ represent respectively the pressure and the absolute temperature in the gas.

Respecting the results depending on the relation between the density of the gas and the fineness of the pores.

Law V. Both in the case of thermal transpiration and of transpiration under pressure, similar results will respectively be obtained when the density of the gas bears a fixed relation to the diameters of the apertures in the plates.

Respecting the rate of transpiration arising from a difference of pressure on the two sides of the plate.

Law VI. When gas exists at different pressures on the two sides of a plate, and the difference of pressure bears a fixed ratio to the pressure on either side; then for a certain plate and a certain gas the time of transpiration of equal volumes will, when the gas is sufficiently dense, be inversely proportional to the density; but as the rarefaction increases, the increase in the time of transpiration becomes less and less, until the time becomes constant.

Law VII. When the rarefaction is so great that the time of transpiration of equal volumes of the same gas is constant, the times of transpiration of equal volumes of different gases will be proportional to the square root of the atomic weights of the gases.

Respecting the results of impulsion, and the connection between these results and the relation between the density of the gas and the size of the vanes.

Law VIII. When the gas is sufficiently dense, then the impulsive force will be inversely proportional to the densities of the gas; but as the rarefaction proceeds the increase in the force becomes less and less until the rarefaction has reached a point depending on the size of the vanes (the larger the vanes the higher must be the rarefaction), after which the force begins to diminish, and ultimately diminishes with the density.

These laws were reduced to the form in which they have been stated in order to adapt them for experimental verification. Thus they do not represent the simplest nor yet the fullest form in which the properties of the gas can be expressed. This may be seen by reference to Sections X. and XII. which treat of the theory of these properties. There definite expressions will be found for the relations indefinitely indicated in Laws I. and II. These definite expressions are not introduced here, because they have not been definitely verified by experiment.

The definite relations expressed in Laws III., IV., V., VI., VII., and VIII., although derived from theoretical considerations, have all been to a greater or less extent verified by experiment—as far as the possible range of densities would admit—and in all cases the experimental results within the limits of error corresponded well with the theoretical deductions.

SECTION II. EXPERIMENTS RELATING TO THERMAL TRANSPIRATION.

10. In commencing these experiments it was impossible to form any estimate whatever of the magnitude of the results to be expected. The laws just stated only showed what would be the comparative value of the results under different circumstances; so that until a result had been found it was impossible to predict whether, with any particular plate, the result would be appreciable or not.

Thus it happened that although the experiments commenced on Jan. 15, 1878, it was not until March that any definite results were obtained. This delay was chiefly owing to several very subtle sources of disturbance, the effect of which could only be distinguished from true results after a series of tests extending in each case over several days.

The material first used for the plates was Wedgewood biscuit-ware, $\frac{3}{16}$ths inch thick; and it was with this material after a long series of trials that connected results were first obtained. These results were very minute. With air at the pressure of the atmosphere, the greatest difference of pressure was ·1 of an inch (2·5 millims. of mercury).

Having, however, once obtained this result, it was seen to follow from Law V., Art. 9, that greater results could be obtained with a finer plate. My idea was to try graphite, such as that used by Graham; but in the meantime it occurred to me to try meerschaum, which proved to be a most convenient material, as it could be obtained in any sizes and readily cut into plates of any thickness.

With this material, first used on March 7, the later results were very striking; the difference of pressure amounting to ·25 of an inch with air at the pressure of the atmosphere, and to nearly an inch with hydrogen at the same pressure.

The description of the details of the earlier experiments, together with the various difficulties which were met with and the means employed to overcome them, would take too much room to admit of their being given at length. I shall, therefore, proceed at once to the description of the apparatus in its final form, and shall confine myself to noticing only such results as are important to the subject.

Description of the apparatus.

11. This consisted principally of an instrument which may be called a thermo-diffusiometer.

This instrument, as shown in Fig. 1, consists essentially of two chambers separated by a plate of porous material, means being provided for keep-

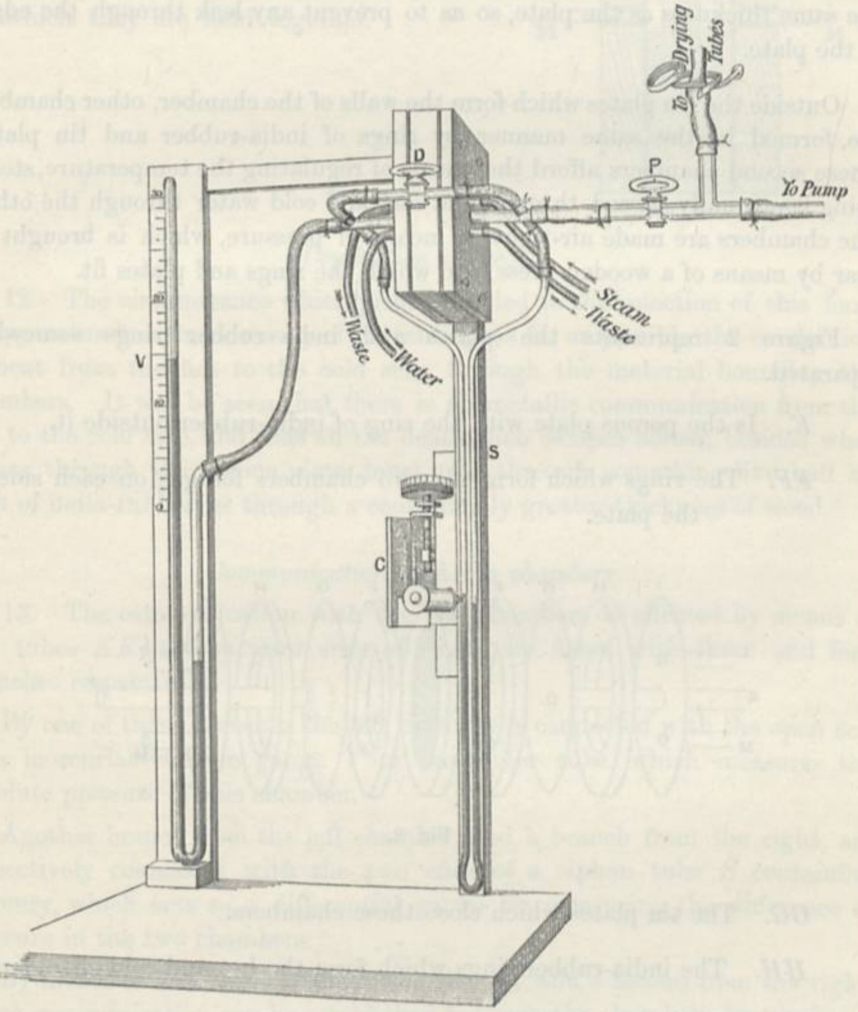


Fig. 1.

ing the chambers at constant but different temperatures for many hours at a time; also for measuring the pressure of gas in the chambers, for exhausting the chambers, and for bringing the chambers into direct communication when desired.

The chambers are formed by tin plates separated by rings of india-rubber, between which is held the porous plate. The external diameter of the rings is about $3\frac{1}{2}$ inches; and the internal diameter, the diameter of the chambers, is $1\frac{1}{2}$ inches. The thickness of the rings, the depth of the chamber, is about $\frac{3}{16}$ths of an inch. The porous plates are 2 inches in diameter, so that the edges are well covered by the rings of india-rubber which bound the chambers; and outside the plate is fitted another ring of india-rubber of the same thickness as the plate, so as to prevent any leak through the edges of the plate.

Outside the tin plates which form the walls of the chamber, other chambers are formed in the same manner by rings of india-rubber and tin plates. These second chambers afford the means of regulating the temperature, steam being continually passed through the one and cold water through the other. The chambers are made air-tight by means of pressure, which is brought to bear by means of a wooden press into which the rings and plates fit.

Figure 2 represents the plates and india-rubber rings somewhat separated.

E. Is the porous plate with the ring of india-rubber outside it.

FF. The rings which form the two chambers for gas on each side of the plate.

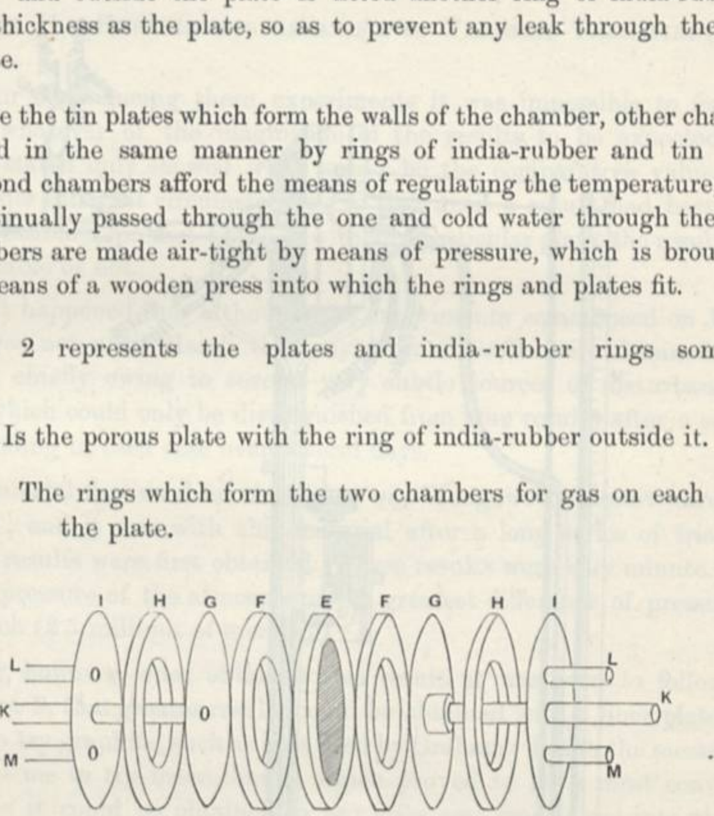


Fig. 2.

GG. The tin plates which close these chambers.

HH. The india-rubber rings which form the hot and cold chambers.

II. The tin plates which close these chambers.

KK. Tubes soldered to the tin plates *GG* to communicate with the chambers *FF*, and

LM, LM. Are tubes soldered to the tin plates *II*, to allow of the streams of steam or water through the chambers *HH*.

Figure 3 shows a section taken along the axis of the rings and plates, showing them in position, also the wooden press by which they are held together.

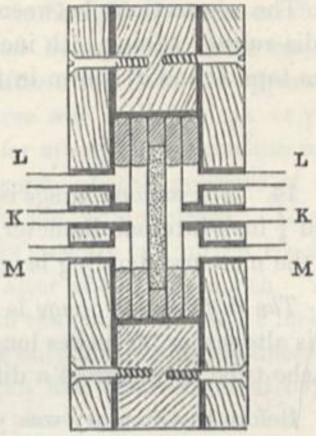


Fig. 3.

Conduction of heat.

12. The circumstance which principally led to the selection of this form of apparatus was the necessity of preventing, as far as possible, the conduction of heat from the hot to the cold side, through the material bounding the chambers. It will be seen that there is no metallic communication from the hot to the cold side, and that all the heat which escapes across, besides what passes through the porous plate, must pass through something like half an inch of india-rubber, or through a considerably greater thickness of wood.

Communication with the chambers.

13. The communication with the gas chambers is effected by means of the tubes *KK*, the outward ends of which are fitted with three and four branches respectively.

By one of these branches the left chamber is connected with the open end of a mercurial vacuum gauge *V* or barometer tube, which measures the absolute pressure of this chamber.

Another branch from the left chamber, and a branch from the right, are respectively connected with the two ends of a siphon tube *S* containing mercury, which acts as a differential gauge for measuring the difference of pressure in the two chambers.

By means of the third branch from the left, and a second from the right, direct communication can be established between the chambers by turning a tap *D*.

The third and fourth branches on the right are used to establish communication with a mercurial pump and to admit dry gas.

These various connections are shown in Fig. 1, page 265, which also shows the general arrangement of the apparatus.

The connections between the metal and glass tubes are made with thick india-rubber tubing, $\frac{1}{8}$th inch bore and $\frac{5}{8}$th inch external diameter; and the two taps D and P shown in the sketch are both of glass.

The gauges.

14. *The vacuum gauge* is an ordinary barometer tube about 32 inches long and $\frac{1}{4}$ inch internal diameter, having its second limb sufficiently long to allow of the mercury standing level when the chambers were exhausted.

The differential gauge is of glass tube about $\frac{1}{8}$th inch internal diameter, it is altogether 30 inches long, so as to prevent the mercury being driven out of the tube by too great a difference of pressure.

Before the mercury was put into this tube it was wetted with sulphuric acid. A small quantity of this remained and covered the mercury on either side, by means of which sulphuric acid the free motion of the mercury was secured, so that differences of pressure as small as $\frac{1}{10000}$th of an inch of mercury caused it to move without the necessity of shaking.

Reading the gauges.

15. As far as the vacuum gauge was concerned, there was no point to be gained by extreme accuracy in reading the absolute pressure of gas in both chambers, so that a scale attached to the gauge was found to answer all purposes.

On the other hand the range of the experiments depended on the accuracy with which the differential gauge could be read. A special means of reading this gauge was devised. This consisted of a species of cathetometer almost close to the gauge, in which, instead of a telescope, a microscope with an inch object-glass and a semi-disc in the focus of the eye-piece was used, the screw which moved the microscope had 50 threads to an inch, and the head had 200 divisions, so that one division corresponded to the $\frac{1}{10000}$th part of an inch. Owing to the high magnifying powers, the effect of a motion of one division was visible, and several readings taken from the same position of the mercury agreed to within one division.

Testing the apparatus.

16. The complicated character of the apparatus and the number of joints rendered it extremely difficult to make it perfectly tight. When working at the pressure of the atmosphere this was of no great moment, but when working with rarefied gas it was necessary that it should be so tight that the leak might cause no appreciable disturbance.

At first india-rubber varnish was used to make the joints tight; but this did not answer, as the vapour from the varnish produced very considerable disturbance. After this the surfaces of the india-rubber were carefully washed, and then considerable pressure applied by wrapping wire on the tubes and screwing up the press. In this way, after a few days, the apparatus became what may be called perfectly tight. There was a slight leak or probably slight diffusion through the india-rubber, for after the experiments were concluded the apparatus was left full of hydrogen at the pressure of the atmosphere, and the tap communicating with the pump closed. It was then found that the pressure within the chambers steadily fell until it reached 9 inches of mercury. This point was reached after about one month. The pressure then began to rise, and in another month the gauge showed 12 inches. The entire volume of the chambers and tubes is only about 6 fluid ounces, so that it might well be imagined that the hydrogen had been absorbed by, or condensed on the india-rubber or the porous plate, but the fact that the pressure again rose seemed to imply that the hydrogen had escaped; but whether through the india-rubber or not it is impossible to say.

Such a leak, however, was entirely without effect on the results. In fact, a leak which admitted air at the rate of 1 inch of mercury in an hour into one chamber did not cause any appreciable alteration in the differential gauge.

Drying the gas.

17. The presence of vapour in the gas was at first a source of great trouble. The tendency of porous plates to absorb moisture is so great, and the presence of vapour in the gas produces such a great disturbance even when the pressure of vapour is a long way below that at which it would condense on the cold surface, that for some time this threatened to prevent any satisfactory result being obtained. At last, however, by having steam on both sides, and repeatedly exhausting and refilling with air that had been passed slowly through drying tubes, 40 inches long, containing first sulphuric and then anhydrous phosphoric acid—for which I am indebted to the kindness of Dr Roscoe—the effect of vapour was all but eliminated, and consistent results were obtained over several trials, even when *the sides of the steam and water were reversed.*

The differences of temperature.

18. The steam used for heating the apparatus was obtained by boiling water in a glass flask which held about a gallon, enough to last for twelve hours at a time. The glass was fitted with a water safety-valve; so that the pressure of steam could not exceed about 8 inches of water. The flask was placed about 6 feet from the instrument, so that the heat from the gas flame did not produce any material disturbance or materially affect the mercury in the gauges.

The cold water was direct from the main, and was found to be very constant in temperature, not varying throughout the experiments more than 23°—from 47° F. in February to 70° F. in July.

In this way the tin plates (*GG*, Fig. 2) which bound the gas chambers were respectively maintained at temperatures differing by less than 1° F. from the temperature of the steam (212°) and that of the water.

The sides of the porous plate would not acquire the same temperatures as the steam and water, because the conduction through the porous plates would tend to equalise the temperature. Nor was there any means of ascertaining the exact temperatures other than by comparing the results obtained. But from these it appeared that there was considerable difference between the temperature of the surfaces of the porous plate and that of the opposite tin plate. A method of eliminating this difference has been found, and this will be explained with the results themselves.

The porous plates.

19. These, whether of biscuit-ware, meerschaum, or stucco, were circular discs 2 inches (53·0 millims.) in diameter. The rings *FF* which formed the chamber had a diameter of $1\frac{1}{2}$ inches (38 millims.), and these limited the portion of the plate exposed to the passage of gas. The plates were of different thicknesses, the thinnest being $\frac{1}{16}$th inch (1·5 millims.) and the thickest ·44 inch (14·2 millims.).

The results with air through porcelain plate No. 3 *and meerschaum Nos.* 1 *and* 2.

20. After numerous experiments, commencing on January 23, with plates Nos. 1, 2, and 3 of biscuit-ware, the results of which, although there appeared to be a residual difference of pressure, were very much disturbed, the first definite and consistent results were obtained with a porcelain plate, No. 3, $\frac{1}{10}$th inch (2·5 millims.) thick, on February 22.

TABLE I. Thermal transpiration of air by biscuit-ware plate No. 3 (·1 inch or 2·5 millims. thick). Temperature of steam, 212° F. or 100° C.; temperature of water, 47° F. or 8° C.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge, February 22		Ratio of mean pressure to difference of pressure
inches	millims.	inch	millims.	
30	762	·1	2·54	300

This result was found to remain constant over a period of 8 hours, during which the steam and water were kept constantly flowing. It was also found to be the same whichever side of the diffusiometer was heated. During the experiment the tap bringing the hot and cold chambers into direct communication was frequently opened, and the differential gauge then indicated equal pressures. After each of these openings on the tap being again closed the same difference was re-established in a few seconds.

The next experiments were made with a somewhat thinner plate of meerschaum No. 1.

TABLE II. Thermal transpiration of air by meerschaum plate No. 1 (·06 inch or 1·5 millims.). Temperature of steam, 212° F. or 100° C.; temperature of water, 47° F. or 8° C.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge, March 12		Ratio of mean pressure to difference of pressure
inches	millims.	inch	millims.	
30	762	·08	2·03	350

As it seemed highly probable that the meerschaum plate was of finer texture than the porcelain plate previously tried, the fact that the difference of pressure with the meerschaum was not larger than with the porcelain was a matter of some surprise. There appeared, however, to be a possible cause for this in the thinness of the meerschaum. It was possible that there was some flaw in the plate, or more probably that the thinness of the plate allowed a considerable equalisation of temperature by the conduction of heat. It was, therefore, resolved to try a thicker plate of meerschaum, and a plate ·25 inch (6·3 millims.) was introduced in place of that previously tried.

TABLE III. Thermal transpiration of air by meerschaum plate No. 2 (·25 inch or 6·3 millims. thick). Temperature of steam, 212° F. or 100° C.; temperature of water, 47° F. or 8° C.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge, March 15		Ratio of mean pressure to difference of pressure
inches	millims.	inch	millims.	
30·2	764·5	·25	6·096	121
12·9	327·6	·20	5·080	64
8·53	216·7	·17	4·318	50
3·70	94·0	·12	3·048	31
2·0	50·8	·08	2·032	25
0·88	12·35	·045	1·143	20
0·5	12·7	·035	0·889	13·6

Whether the fact that the thicker plate of meerschaum gave nearly three times the difference of either of the previous plates was due to the thicker plate maintaining a greater difference of temperature, or to some difference of texture in the thin plate, such as a flaw, has not been clearly determined, but it now appears probable that it was largely due to the first of these causes.

With this plate lower pressures were for the first time tried, and Table III. shows these differences falling with the pressure.

The ratio of the difference of pressure to the mean pressure, however, as is shown in the last column, increases as the pressure falls, and apparently is approximating to a constant value at lower pressures. This is according to Law III., Art. 9.

From Law IV., Art. 9, it appears that this ratio should, as the pressure fell, have approximated to the ratio which the difference of the square roots of the absolute temperature on the two sides of the plate bears to the square root of the temperature on the side at which the pressure was measured. Assuming $1 \div 13$ to be this ratio, it would appear that there must have been considerable differences of temperature between the surfaces of the meerschaum and the side of the plate; but it also appeared probable that with still lower pressures the ratio might have been considerably lower.

It would have been desirable to have carried the experiments to lower pressures, but at that time this was impossible as there was then no special means of reading the differential gauge; so that this had to be deferred until such a means was provided.

Hydrogen.

21. In the meantime other gases were tried. Owing to its lightness it was thought probable that hydrogen would at the higher pressure give a somewhat higher result than air. How much this might be the theory gave no certain indication, for it depended on qualities of the gas which had not been determined. But at the lower pressure, according to Law IV., the difference of pressure should approximate towards the same value relatively to the absolute pressure.

This was clearly a point which might be tested even though no very close approximation should be reached. Hydrogen was accordingly tried.

Table IV. shows that at the pressure of the atmosphere the difference with hydrogen was four times as great as it had been with air, and reached the very considerable figure of ·92 of an inch of mercury. This was much more than had been anticipated, although there was nothing in the theory to show that it should not exist. This great difference at the higher pressures only serves to bring out more forcibly the convergence, according to Law IV.,

TABLE IV. Thermal transpiration of hydrogen by meerschaum plate No. 2 (·25 inch or 6·3 millims. thick). Temperature of steam, 212° F. or 100° C.; temperature of water, 47° F. or 8° C.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge		Ratio of mean pressure to difference of pressure
inches	millims.	inch	millims.	
30·2	767	·88	23·37	32·4
13·0	330	·60	15·24	21
7·5	190·5	·44	11·18	17
4·25	107·9	·28	7·11	15
2·0	50·8	·15	3·81	13·3
1·0	25·4	·08	2·03	12·5
0·5	12·7	·036	0·91	13·7

as the pressure falls. At pressures of 1 inch it will be seen that the differences for air and hydrogen are as 12·5 to 20, while if the results at ·5 inch could be trusted, the ratio is 13·7 to 13·6.

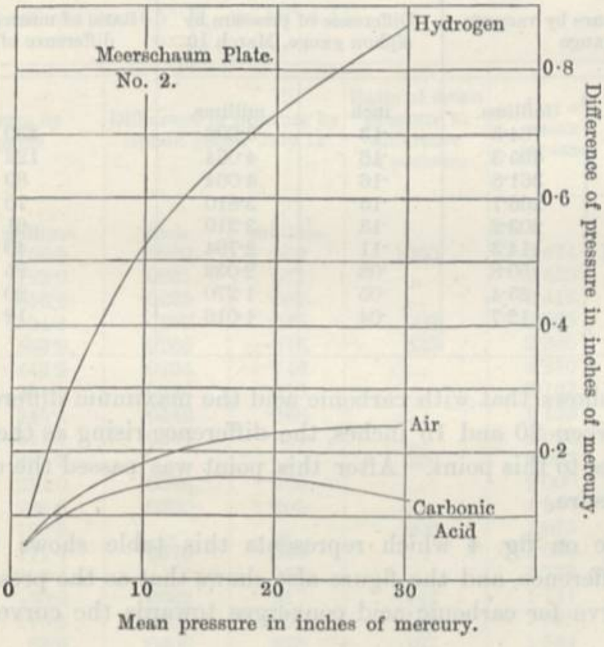


Fig. 4.

The convergence of these results is best seen in the accompanying diagram. The curves are drawn through points of which the pressures are abscissæ, and the differences of pressure (on a different scale) are the ordinates.

The maximum difference of pressure (carbonic acid).

22. The curves, fig. 4, show that the differences both for hydrogen and air appear to be tending, as the pressure rises, to a maximum value. This was exactly what was expected from Law III., Art. 9, and had the apparatus been capable of withstanding considerable pressures it would have been desirable to have raised the pressure until the maximum was passed. But it appeared that the same end might be more readily accomplished in other ways.

Owing to the great density and low coefficient of diffusion of carbonic acid, it seemed to be probable that with this gas the difference of pressure would reach a maximum at considerably lower pressures than either hydrogen or air. Carbonic acid was therefore tried.

TABLE V. Thermal transpiration of carbonic acid by meerschaum plate No. 2 (·25 inch or 6·3 millims. thick). Temperature of steam, 212° F. or 100° C.; temperature of water, 47° F. or 8° C.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge, March 10		Ratio of mean pressure to difference of pressure
inches	millims.	inch	millims.	
30·1	764·5	·13	3·302	230
19·5	495·3	·16	4·064	122
14·25	361·8	·16	4·064	89
10·5	266·7	·15	3·810	70
8·0	203·2	·13	3·310	61
4·5	114·3	·11	2·794	40
2·0	50·8	·08	2·032	25
1·0	25·4	·05	1·270	20
0·5	12·7	·04	1·016	12

Table V. shows that with carbonic acid the maximum difference was at a pressure between 20 and 15 inches, the difference rising as the pressure fell from 30 inches to this point. After this point was passed the difference fell with the pressure.

The curve on fig. 4 which represents this table shows the point of maximum difference, and the figure also shows that as the pressures became small the curve for carbonic acid converges towards the curves for air and hydrogen.

These results for carbonic acid are perhaps sufficient to verify Law III. respecting the existence of a maximum. But they were obtained with considerable trouble, as the india-rubber tubing absorbed the carbonic acid very rapidly, and so caused considerable disturbance. For this reason carbonic acid was not again used.

Stucco plate No. 1.

23. As it appeared from Law V., Art. 9, that any increase in the coarseness of the plate should reduce the pressure at which the difference should be a maximum for each gas, a plate of stucco was tried with this object.

It was clear that the differences would be much smaller with the stucco than with the meerschaum. Therefore this plate was not tried until the differential gauge had been furnished with the cathetometer to read to $\frac{1}{10000}$th of an inch ($\frac{1}{400}$th of a millim.).

Final experiments.

A series of experiments, commencing with stucco plate No. 1, but continued with meerschaum plate No. 3 and stucco No. 2, were commenced on May 6,

TABLE VI. Thermal transpiration of air by stucco plate No. 1 (·25 inch or 6·3 millims. thick). Temperature of steam, 212° F. or 100° C.; temperature of water, 65° F. or 18°·4 C.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge, July 11		Ratio of mean pressure to difference of pressure	Log of mean pressure	Log of difference of pressure
inches	millims.	inch	millims.			
29·80	756·9	·0220	·559	1360	2·474 − 1	2·342 − 4
28·50	723·9	·0225	·571	„	2·455	2·352
25·85	656·6	·0235	·597	...	2·412	2·371
23·40	594·4	·0250	·635	903	2·369	2·397
22·20	563·9	·0266	·675	835	2·346	2·424
17·40	443·9	·0294	·746	...	2·240	2·468
15·40	391·2	·0336	·813	...	2·187	2·526
13·60	345·4	·0342	·868	...	2·133	2·534
12·25	311·1	·0348	·884	...	2·066	2·541
11·35	288·3	„	„	326	2·053	2·541
10·00	254·0	·0366	·929	...	2·000	2·563
9·00	228·6	·0380	·965	...	1·954	2·579
7·50	190·5	„	„	200	1·875	2·579
6·75	171·4	·0376	·955	...	1·630	2·575
6·00	152·4	„	·955	...	1·778	2·575
5·15	130·8	·0362	·917	142	1·711	2·559
4·35	110·5	·0354	·899	120	1·638	2·549
3·50	88·9	·0306	·828	107	1·544	2·513
2·90	73·7	·0314	·797	92	1·462	2·496
2·35	59·7	·0290	·736	81	1·370	2·462
2·25	57·15	·0284	·721	79	1·350	2·453
1·25	31·75	·0230	·584	54	1·097	2·361
0·60	15·24	·0149	·378	42	1·778	2·258
0·25	6·35	·0080	·203	31	1·398	1·903
0·15	2·66	·0066	·167	23	1·176	1·819

and repeated in July. To give all the observations made in this series of experiments would occupy too much space, therefore a selection has been made, those results being chosen which appeared to be least subject to disturbance. However, the results all agree so well that there was but little choice, and it was clearly unnecessary to resort to the usual method of taking

TABLE VII. Thermal transpiration of hydrogen by stucco plate No. 1 (·25 inch or 6·3 millims. thick). Temperature of steam, 212° F. or 100° C.; temperature of water, 63° F. or 17° C.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge			Ratio of mean pressure to difference of pressure	Log of mean pressure	Log of difference of pressure
		May 6	July 11				
inches	millims.	inch	inch	millims.			
33·00	858·0	...	·1340	3·404	252	2·518 − 1	3·127 − 4
31·00	787·4	...	·1366	3·470	227	2·491	3·135
29·00	736·6	...	·1396	3·546	207	2·462	3·145
28·50	723·9	·1408	...	3·576	203	2·454	3·149
,,	,,	...	·1400	3·556	,,	,,	3·147
27·00	685·8	...	·1436	3·647	188	2·431	3·157
25·30	642·6	...	·1446	3·672	174	2·403	3·160
23·75	603·2	...	·1460	3·708	162	2·375	3·164
22·00	558·8	...	·1490	3·784	147	2·342	3·173
20·00	508·0	...	·1530	3·886	130	2·301	3·185
19·00	482·6	...	·1540	3·912	123	2·279	3·187
18·00	457·2	...	,,	3·912	116	2·255	3·187
16·70	424·1	...	·1542	3·917	109	2·222	3·188
16·00	406·4	·1532	...	3·891	104	2·204	3·185
15·80	401·3	...	·1532	,,	103	2·199	3·185
14·90	378·4	...	·1538	3·906	94	2·178	3·187
13·35	339·0	...	·1536	3·901	87	2·125	3·186
12·50	317·5	...	·1534	3·896	81	2·096	3·186
11·55	393·4	...	·1512	3·840	76	2·062	3·179
9·80	248·9	...	·1506	3·825	65	1·991	3·178
9·50	239·7	·1512	...	3·840	62·5	1·977	3·170
9·00	228·6	...	·1480	3·759	60·8	1·954	3·175
8·00	203·2	...	·1470	3·734	55	1·903	3·167
6·00	152·4	...	·1320	3·353	45	1·778	3·120
3·25	82·5	...	·1046	2·637	31	1·511	3·019
3·2	81·3	...	·1020	2·590	,,	1·505	3·008
2·0	50·8	...	·0760	1·930	25	1·301	2·880
1·8	45·72	·0760	...	,,	23·5	1·255	2·880
1·15	29·21	...	·0500	1·270	23	1·176	2·698
0·7	17·78	...	·0330	·838	21	0·845	2·518
0·6	15·24	·0280	...	·711	,,	0·778	2·447
0·4	10·16	...	·0190	·482	,,	0·602	2·278
0·3	7·62	·0158	...	·401	19	0·477	2·198

mean values. Such differences as do exist are sufficiently accounted for by the small differences in the temperature of the water, which was several degrees higher in July than in May.

With the stucco plate the greatest differences of pressure, both in the case of air and that of hydrogen, are small, something less than one-fourth the differences previously found in the case of the meerschaum plate No. 2; but then with the stucco the points of maximum difference are well below the pressure of the atmosphere.

The difference of pressure between the observations is so small, and the agreement of the observations so great, that by merely joining the points plotted to represent the observations, very fair curves are formed.

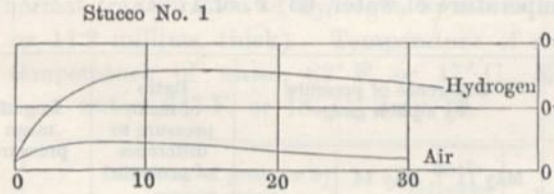


Fig. 5.

These curves bring out in a marked manner the agreement of the results with Law III., Art. 9.

With air the difference rises from ·02 of an inch (·508 millim.), at a pressure of 30 inches, to ·0380 of an inch or (·965 millim.), at a pressure of 7·5 inches, which is the maximum.

With hydrogen the difference also rises as the pressure falls from 30, but the rise is not so great and the maximum is reached at 16 inches.

After passing the maximum the curves both fall, and in falling obviously converge.

This is all exactly in accordance with what was expected.

Corresponding pressures (stucco 1, *meerschaum* 2).

24. Law V. shows that there should be correspondence between certain portions of the curve for stucco and those for meerschaum, although the corresponding points would not be at the same pressures.

Assuming the temperatures to be the same, the corresponding points would be those for which the ratio of the mean pressure to the difference of pressure were the same. Which points may at once be found by comparing the figures in the columns showing this ratio in Tables III. and IV., with the same columns in Tables VI. and VII. respectively.

Before making such a comparison, however, it is necessary to introduce certain small corrections for the difference in the temperature of the water in the two experiments; this, as will be subsequently explained, will be equivalent to diminishing the difference in the Tables III. and IV. in the ratio 7 to 8.

Then we find that the pressures at which the ratios are the same in Tables III. and VI. are approximately as 6 to 1, taking only the higher pressures, while the Tables IV. and VII. give the ratio 6·7 to 1.

TABLE VIII. Thermal transpiration of air by meerschaum plate No. 3 (·44 inch or 11·2 millims. thick). Temperature of steam, 212° F. or 100° C.; temperature of water, 63° F. or 17° C.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge			Ratio of mean pressure to difference of pressure	Log of mean pressure	Log of difference of pressure
		May 11	May 14				
inches	millims.	inch	inch	millims.			
31·00	787·4	·2200	...	5·588	141	2·49 − 1	2·342 − 3
29·50	749·3	...	·2140	5·436	138	2·47	2·330
28·50	723·9	·2160	...	5·486	132	2·45	2·334
27·50	698·5	...	·2126	5·400	129	2·44	2·327
24·50	622·3	...	·2100	5·334	116	2·37	2·322
23·00	584·2	·2130	...	5·410	108	2·36	2·328
21·50	546·1	...	·2054	5·217	104	2·33	2·312
20·00	508·0	·2120	...	5·385	94	2·30	2·326
19·50	495·3	...	·1970	5·003	99	2·29	2·294
18·00	457·2	·2100	...	5·334	85	2·25	2·322
17·00	431·8	...	·1890	4·800	90	2·23	2·276
12·50	317·5	·1730	...	4·394	72	2·09	2·238
11·50	292·1	...	·1630	4·140	70	2·06	2·212
8·25	209·5	·1446	...	3·672	57	1·92	2·160
7·80	198·1	...	·1336	3·393	59	1·89	2·105
5·20	133·3	·1184	...	3·007	44	1·72	2·073
4·70	118·4	...	·1050	2·667	"	1·672	2·021
3·40	86·4	·0904	...	2·290	37	1·531	1·954
3·10	78·7	...	·0806	2·047	38	1·491	1·906
2·10	53·3	·0710	...	1·803	29	1·322	1·851
2·00	50·8	...	·0630	1·604	32	1·301	1·799
1·40	35·6	·0510	...	1·294	27	1·146	1·707
1·32	33·5	...	·0486	1·234	"	1·120	1·687
1·10	28·0	·0394	...	1·000	28	1·041	1·595
0·83	21·06	...	·0380	0·965	22	0·919	1·580
0·65	16·51	·0304	...	0·762	21	0·812	1·482
0·52	13·21	...	·0290	0·736	18	0·716	1·462
0·40	10·16	·0250	...	0·635	16	0·544	1·392
0·39	9·91	...	·0220	0·559	17	0·531	1·342
0·28	7·11	·0192	...	0·488	14	0·361	1·283
"	"	...	·0180	0·457	14	0·361	1·255
0·20	5·08	·0168	...	0·427	12	0·301	1·225
0·19	4·82	...	·0146	0·371	12	0·278	1·164
0·15	3·81	·0154	...	0·391	10	0·176	1·187

The results of this comparison, although not strictly consistent, indicate that there is a correspondence, the points on the curves for meerschaum corresponding with points on the curves for stucco, for which the pressures are about $\frac{1}{6}$ for air, and $\frac{1}{6{\cdot}7}$ for hydrogen.

It was clear, however, that the number of observations with the meerschaum plate was not sufficient to allow of a very close comparison with the curve for stucco, for the accuracy with which the differences had been read without the cathetometer was not sufficient to allow of any use being made of the lower pressures.

TABLE IX. Thermal transpiration of hydrogen by meerschaum plate No. 3 (·44 inch or 11·2 millims. thick). Temperature of steam, 212° F. or 100° C.; temperature of water, 63° F. or 17° C., May 15 and 18; temperature of water, 65° F. or 18° C., July 10.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge				Ratio of mean pressure to difference of pressure	Log of mean pressure	Log of difference of pressure
		May 15	May 18	July 10				
inches	millims.	inch	inch	inch	millims.			
35·00	889·0	...	...	·7940	20·17	44	2·544 − 1	2·900 − 3
34·00	863·6	...	·7930	...	20·14	43	2·531	2·899
32·00	812·8	·7930	...	...	20·14	40	2·505	2·899
30·00	762·0	...	·7670	...	19·48	39	2·477	2·885
29·50	749·3	·7760	...	...	19·71	38	2·470	2·889
,,	,,	...	...	·7776	19·75	37	,,	2·890
27·50	698·5	·7600	...	...	19·30	36	2·439	2·880
22·00	558·8	·6914	...	...	17·56	32	2·342	2·840
18·50	469·9	...	·6250	...	15·87	29·5	2·267	2·795
18·00	457·2	·5710	...	...	14·50	31	2·255	2·757
,,	,,	...	...	·5960	15·14	30	2·255.	2·775
12·00	304·8	...	...	·4800	12·19	25	2·070	2·681
11·40	289·6	...	·4626	...	11·75	24·6	2·056	2·664
10·50	266·7	·4156	...	...	10·56	25	2·021	2·618
7·70	195·6	...	...	·3594	9·13	21	1·886	2·555
7·60	193·0	...	·3169	...	8·03	24	1·880	2·500
6·95	176·5	·3046	...	...	7·74	23	1·842	2·484
4·75	120·6	...	·2206	...	5·60	23	1·676	2·343
,,	,,	...	...	·2584	6·57	18	,,	2·412
4·50	114·3	·2120	...	...	5·38	21	1·653	2·326
3·00	76·2	...	·1568	...	3·98	19	1·477	2·200
,,	,,	...	...	·1784	4·53	17	,,	2·251
2·60	66·0	·1420	...	...	3·60	18	1·414	2·152
1·95	49·6	...	...	·1204	3·06	16	1·290	2·080
1·70	43·2	·1063	...	...	2·70	16	1·230	2·026
1·25	31·8	...	...	·0784	1·991	16	1·096	1·894
1·10	27·95	...	·0630	...	1·600	17	1·041	1·799
1·00	25·40	·0660	...	...	1·676	15	1·000	1·819
0·70	17·78	...	...	·0380	0·965	18	0·845	1·580
0·65	16·51	...	·0325	...	0·825	20	0·813	1·511
0·60	15·26	·0380	...	...	0·965	15	0·778	1·500
0·35	8·88	...	·0250	...	0·635	14	0·544	1·297
0·32	8·13	·0200	...	...	0·580	16	0·505	1·301
0·175	...	·0150	...	...	0·381	12	0·243	1·176

Meerschaum plate No. 3.

25. A fresh meerschaum plate, ·44 inch thick, was therefore tried, another diffusiometer, exactly similar to the original one, being constructed for the purpose.

Although this plate was so much thicker than meerschaum plate No. 2, the results were no greater. They appear rather less, but this was owing to the somewhat higher temperature of the water, which would reduce the results in Table IV. in the ratio 8 to 9, and when this correction is applied the agreement is very close.

Effect of the thickness of the plate.

26. It had been expected, however, that the extra thickness of the plate No. 3 would have caused it to give somewhat higher results, and its not doing so seemed to imply that the plates were so thick that the conduction of heat through the plate produced no appreciable effect on the temperature of the surfaces of the meerschaum. It appeared, however, from subsequent experiments that in all probability there was a small difference in the two instruments. The original instrument, that in which the experiments on plate No. 2 were made, had been used a great deal, and the surfaces of the tin plates which were opposite to the meerschaum had lost all their polish and become black, while in the second instrument the plates were new and bright. It might, therefore, be expected that the old plates would radiate more heat than the bright plates, and so better maintain the difference of temperature, and besides this the india-rubber rings in the new instrument were somewhat thicker than those in the old one, and so the space between the plates and the surface of the meerschaum was greater than in the old instrument. It appears, therefore, that these causes may have neutralised the increase in the difference of temperature that would otherwise have resulted from the extra thickness of the plate. And it will be seen that this conclusion was confirmed when on introducing a new stucco plate into the old instrument new tin plates and thicker rings were also introduced.

Infusion of air.

The curves, fig. 6, show the degree of regularity attained in these experiments. Such discrepancies as there are, are apparently owing to the absorption and exhalation of the gas by the india-rubber and possibly by the plate itself, for these discrepancies only occur at the lower pressures.

In the case of hydrogen the greatest care was taken to get the gas pure; but it is not to be supposed that as the gas was pumped out the residual gas would maintain a high degree of purity, for the gases given off by the india-rubber and the air which diffused through it would gradually replace the hydrogen.

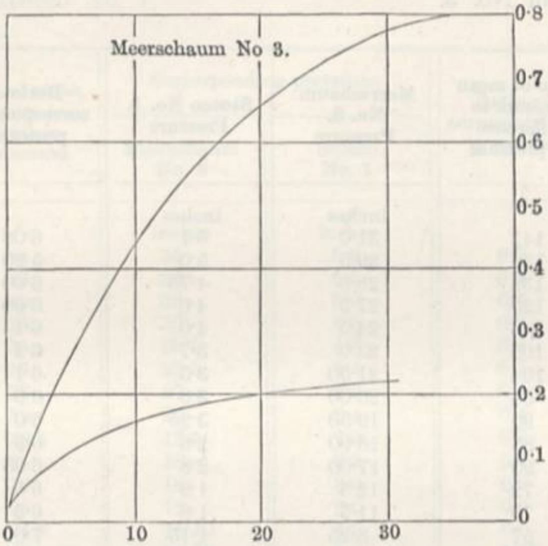


Fig. 6.

Corresponding pressures with stucco No. 1 and meerschaum No. 3.

27. Comparing the ratio columns in Tables VIII. and IX. with the corresponding columns in Tables VI. and VII. respectively, the corresponding pressures are found to be as shown in Tables X. and XI.

In Tables X. and XI. the first columns are the ratios taken direct from Tables VIII. and IX., the second columns are the pressures also taken direct from Tables VIII. and IX.

In order to find the pressures with the stucco plate which would yield exactly the same ratios (difference of pressure to mean pressure) as those in the table, the numbers in the ratio columns of Tables VI. and VII. were plotted, the mean pressures being taken as abscissæ. The points were joined so as to form curves, and then finding points on the curve whose ordinates corresponded to a particular number in the first column, the abscissæ gave the numbers required for the third column in Tables X. and XI. In this way the numbers in the third column are rather more uniform than they would have been had they been the results of actual observation.

Tables X. and XI. show that within the limits of accuracy of the experiments the pressures in the stucco correspond with pressures in the meerschaum six times as great. This is exactly according to Law V., Art. 7, from which it appears that the numerical relation between the corresponding

TABLE X. Showing the pressures of air for which the ratio of the difference of pressure to the mean pressure is the same for stucco No. 1 and meerschaum No. 3.

Ratio of mean pressure to difference of pressure	Meerschaum No. 3. Pressure	Stucco No. 1. Pressure	Ratio of corresponding pressures
	inches	inches	
141	31·0	5·1	6·08
138	29·5	5·0	5·90
132	28·5	4·75	6·00
129	27·5	4·6	6·00
116	24·5	4·0	6·1
108	23·0	3·7	6·2
104	21·50	3·5	6·1
94	20·00	3·0	6·6
99	19·50	3·25	6·0
85	18·00	2·6	6·9
90	17·00	2·8	6·05
72	12·5	1·9	6·5
70	11·5	1·8	6·3
57	8·25	1·15	7·0
59	7·8	1·25	6·2
44	5·25	0·5	10·5
44	4·70	0·5	9·0
37	3·40	0·35	9·9
38	3·10	0·35	9·0
29	2·10	0·24	8·5
32	2·00	0·301	6·6
27	1·40	0·20	7·0
27	1·32	0·20	6·0

pressures is the relation between the diameters of the interstices of the meerschaum and stucco plates. This fact also is confirmed, for not only does it appear that the ratio is independent of the mean density of the gas, but it is the same for hydrogen as it is for air, showing that the relation depends only on the nature of the plates.

Logarithmic homologues of the curves in figs. 5 and 6.

28. It appeared, however, that as a method of obtaining the corresponding pressures the comparison of the ratios was not entirely satisfactory, for it involved the assumption that the ratio of corresponding differences of pressure should be exactly the same as the ratio of corresponding mean pressures; whereas this would only be the case if the differences of temperature were exactly the same for both plates. It seemed desirable therefore to find a means of comparing the curves for the two plates on the assumption that the corresponding abscissæ might bear one ratio and the corresponding ordinates another, or if 1 and 2 are corresponding points, $x_2 = ax_1$ while $y_2 = by_1$.

TABLE XI. Showing the pressures of hydrogen at which the ratio of the difference of pressure to the mean pressure is the same for meerschaum No. 3 and stucco No. 1.

Ratio of mean pressure to difference of pressure	Corresponding pressures		Ratio of corresponding pressures
	Meerschaum No. 3	Stucco No. 1	
	inches	inches	
44	35	5·8	6·0
43	34	5·5	6·2
40	32	5·0	6·4
39	30	4·8	6·2
38	29·5	4·6	6·4
37	29·5	4·4	6·7
36	27	4·2	6·4
32	22	3·4	6·4
29·5	18·5	2·9	6·3
31	18	3·2	5·6
30	18	3·0	6·0
25	12	2·0	6·0
24·6	11·40	1·9	6·0
25	11·50	2·0	5·25
21	7·70	8·0	9·7
24	7·60	1·7	4·5

A graphic method of doing this simply and perfectly was found by comparing not the curves themselves, but what may be called their *logarithmic homologues*.

Instead of plotting, as in figs. 4 and 5, the mean pressures and differences of pressure as the abscissæ and ordinates of the points on the curve, the logarithms of these quantities are plotted. Thus, $x_1' = \log x_1$, $y_1' = \log y_1$, where $x_1 y_1$ may be taken to be a point on any one of the curves already plotted, and $x_1' y_1'$ the corresponding point on the logarithmic homologue. It is thus seen that if for two curves (1) and (2), $x_2 = ax_1$ and $y_2 = by_1$, then $x_2' = x_1' + \log a$ and $y_2' = y_1' + \log b$; or the logarithmic homologues will all be similar curves but differently placed with regard to the axes, such that the one curve may be brought into coincidence with the other by a shift of which the co-ordinates are $\log a$, and $\log b$.

Fig. 7 shows the logarithmic homologues of the curves for stucco No. 1 and meerschaum No. 3, both for hydrogen and air. By tracing the log curves for stucco No. 1, together with the axes, on a piece of tracing paper, and then moving the tracing (so that the axes remain parallel to their original direction) until the traced curves fit on to the curves for meerschaum No. 3, it is found that the fit is perfect, a portion of the traced curve $e'f'$ (stucco) coinciding with a portion of ab, while at the same time a portion of the traced

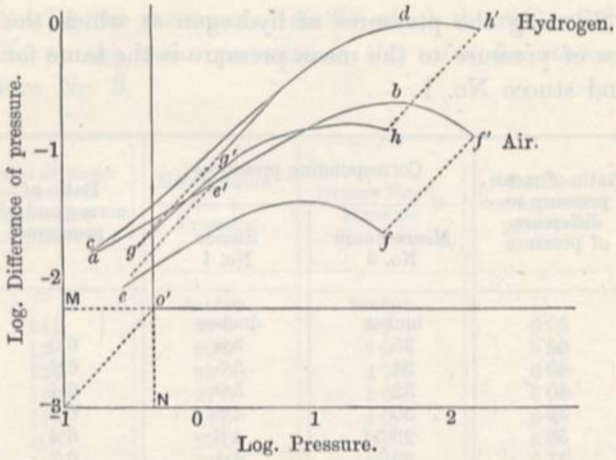


Fig. 7.

curve $g'h'$ coincides with a portion of cd. The effect of the superposition is shown in the figure, $e'b$ and $g'd$ being the portions of the curves which overlap. O' is the new position of O.

It will at once be seen that $O'M$ is the logarithm of the ratio of corresponding abscissæ, while $O'N$ is the logarithm of the corresponding ordinates.

In this particular case

$$O'N = \cdot 7 = \log 5 \text{ and } O'M = \cdot 77 = \log 5 \cdot 9.$$

These numbers differ somewhat from those given by Tables X. and XI., and the difference is very suggestive. The absolute agreement of the curves shows that the difference is not owing to experimental inaccuracy, and it will be seen on comparing the results next given that the difference (5 and 5·9) is owing to a difference in the temperature in the two instruments. If the temperatures had been the same we should have had the same ratio for the corresponding ordinates as for the abscissæ; but a difference in the temperature would alter all the ordinates in a certain ratio without affecting the abscissæ.

The difference $O'N - O'M = \cdot 07 = \log 1 \cdot 175$ gives the ratio in which the differences of pressure are affected by a difference in temperature. This, according to the law that the results are proportional to the square roots of the differences of temperature, would be equivalent to a difference of 21° in the temperature of the water. This difference did not exist, hence there must have been a difference, owing to the greater thickness or to the different nature of the meerschaum plate.

The size of the woodcut does not admit the points indicating the actual experiments being shown, but these are shown in figures 8 and 9, pages 286 A and 286 B.

TABLE XII. Thermal transpiration of air by stucco plate No. 2 (·25 inch or 6·35 millims. thick). Temperature of steam, 212° F. or 100° C.; temperature of water, 70° F. or 21° C.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge			Ratio of mean pressure to difference of pressure	Log of mean pressure	Log of difference of pressure
		July 17	July 18				
inches	millims.	inch	inch	millim.			
30·25	768·3	·0160	...	·406	1892	2·480 − 1	1·204 − 3
30·05	763·3	...	·0162	·411	1855	2·477	1·209
28·05	712·4	·0166	...	·422	1710	2·448	1·220
27·25	692·1	...	·0170	·432	1600	2·435	1·230
25·85	656·6	·0176	...	·447	1470	2·412	1·245
24·90	632·4	...	·0180	·457	1383	2·396	1·255
23·15	588	·0196	...	·498	1181	2·364	1·292
22·05	560	...	·0194	·492	1137	2·343	1·287
20·30	515·6	·0208	...	·528	976	2·307	1·318
19·20	487·4	...	·0204	·518	946	2·283	1·309
18·00	457·2	·0230	...	·584	784	2·255	1·361
16·10	408·94	·0240	...	·610	670	2·207	1·380
15·8	401·32	...	·0230	·584	680	2·199	1·361
14·0	355·6	·0254	...	·645	551	2·146	1·404
13·80	350·52	...	·0244	·620	565	2·140	1·387
12·45	316·2	·0266	...	·676	453	2·095	1·425
11·85	301·00	...	·0256	·650	462	2·073	1·408
10·82	274·8	·0276	...	·701	391	2·034	1·440
10·05	255·2	...	·0262	·660	383	2·002	1·418
9·80	248·9	·0282	...	·716	348	1·991	1·450
9·10	231·1	...	·0272	·691	334	1·959	1·434
8·75	222·1	·0284	...	·721	308	1·942	1·453
8·10	205·7	...	·0280	·711	290	1·908	1·447
7·65	194·3	·0290	...	·736	264	1·883	1·462
7·15	181·6	...	·0284	·721	252	1·853	1·453
6·72	170·7	·0294	...	·746	229	1·817	1·468
6·20	157·5	...	·0288	·731	215	1·791	1·459
5·50	139·7	·0290	...	·746	190	1·740	1·462
5·25	133·3	...	·0286	·726	183	1·720	1·456
4·40	111·7	·0280	...	·711	157	1·643	1·447
4·30	109·2	...	·0276	·701	156	1·633	1·440
3·40	86·4	·0266	...	·676	128	1·531	1·422
3·35	85·1	...	·0264	·671	127	1·525	1·421
2·70	68·6	·0242	...	·615	112	1·431	1·381
2·40	60·96	...	·0226	·574	106	1·380	1·354
2·00	50·8	·0214	...	·543	93	1·301	1·330
1·45	36·8	...	·0182	·462	80	1·161	1·266
1·22	31·00	·0176	...	·447	70	1·086	1·245
·80	20·82	·0138	...	·350	58	0·903	1·139
·50	12·70	...	·0108	·274	48	0·699	1·033
·38	9·65	...	·0088	·223	42	0·580	0·944
·225	5·71	·0050	...	·127	45	0·352	0·699

TABLE XIII. Thermal transpiration of hydrogen by stucco plate No. 2 (·25 inch or 6·35 millims. thick). Temperature of steam, 212° F. or 100° C.; temperature of water, 70° F. or 21° C.

Mean pressure by vacuum gauge		Difference of pressure by siphon gauge			Ratio of mean pressure to difference of pressure	Log of mean pressure	Log of difference of pressure
		July 19	July 20				
inches	millims.	inch	inch	millims.			
31·00	787·40	...	·1072	2·723	290	2·491 − 1	2·030 − 3
30·55	775·50	·1080	...	2·743	283	2·484	2·033
29·60	751·60	...	·1084	2·753	273	2·456	2·035
28·10	713·70	...	·1102	2·799	255	2·449	2·042
26·70	677·90	...	·1122	2·850	237	2·426	2·050
25·50	647·50	...	·1132	2·875	225	2·406	2·054
25·25	641·00	·1130	...	2·870	223	2·402	2·053
24·15	613·40	...	·1152	2·916	209	2·383	2·061
22·40	568·90	·1180	...	2·997	190	2·350	2·072
22·05	560·00	...	·1176	2·977	188	2·343	2·070
21·10	535·90	...	·1182	3·002	177	2·324	2·072
20·15	510·50	...	·1186	3·012	170	2·304	2·074
20·00	508·00	·1192	...	3·027	168	2·301	2·076
19·20	487·60	...	·1190	3·023	160	2·283	2·075
17·15	435·60	...	·1204	3·058	142	2·234	2·080
16·20	411·40	...	·1208	3·068	134	2·209	2·082
16·00	406·40	·1214	...	3·083	130	2·204	2·084
15·30	388·60	...	·1214	„	126	2·185	2·084
14·60	370·80	...	·1220	3·098	119	2·164	2·086
14·55	369·50	·1220	...	„	„	2·163	2·086
13·95	354·30	...	·1220	„	114	2·144	2·086
13·20	335·20	·1212	...	3·078	108	2·120	2·083
12·35	317·70	...	·1216	3·088	101	2·091	2·085
11·95	281·80	·1200	·1200	3·048	100	2·077	2·070
10·70	271·80	·1198	...	3·043	89	2·029	2·087
9·60	243·80	·1176	...	2·987	80	1·982	2·070
8·65	219·70	·1146	...	2·910	75	1·937	2·059
7·75	196·80	·1120	...	2·844	69	1·889	2·049
6·30	160·00	·1064	...	2·702	60	1·799	2·027
5·75	146·00	...	·1000	2·540	56	1·759	2·000
5·10	129·50	·0976	...	2·479	52	1·700	1·989
3·65	92·70	...	·0854	2·169	42	1·562	1·931
3·40	86·30	...	·0860	2·184	40	1·531	1·934
2·50	63·50	·0704	...	1·788	35	1·398	1·847
1·60	40·00	...	·0524	1·331	30	1·204	1·719
1·10	27·90	...	·0420	1·066	26	1·041	1·623
·35	8·88	·0170	...	·431	20	0·544	1·230

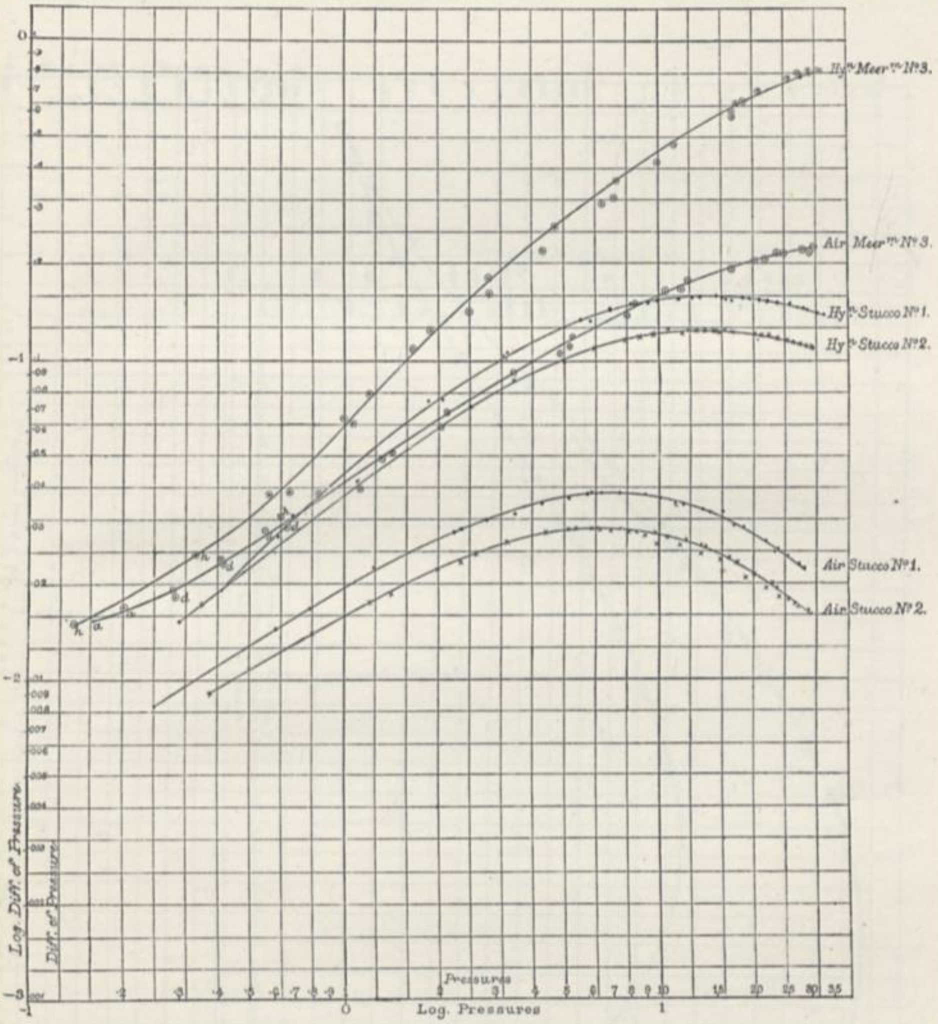

Hyᵈ Meerᵐ Nº 3.
Air Meerᵐ Nº 3.
Hyᵈ Stucco Nº 1.
Hyᵈ Stucco Nº 2.
Air Stucco Nº 1.
Air Stucco Nº 2.
Log. Diffᶜ of Pressure
Diffᶜ of Pressure
Pressures
Log. Pressures

Fig. 8.

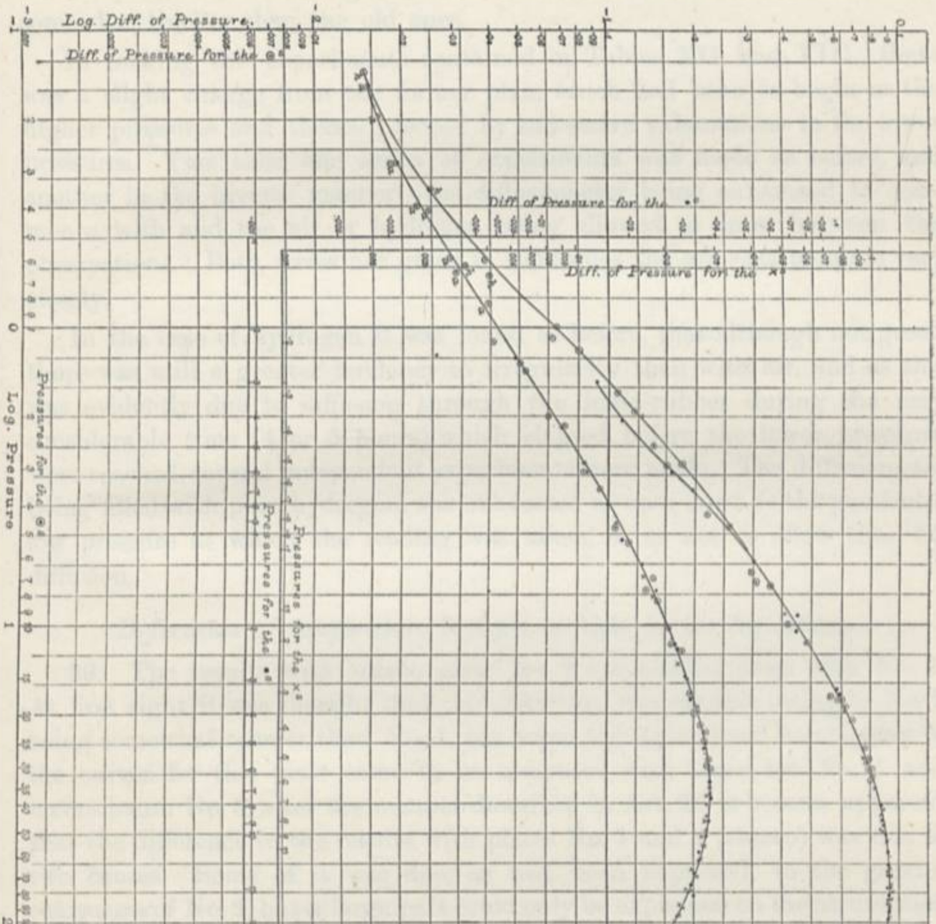

Log. Diff. of Pressure.
Diff. of Pressure for the ⊕s
Diff. of Pressure for the •s
Diff. of Pressure for the ×s
Log. Pressure
Pressures for the ⊕s
Pressures for the •s
Pressures for the ×s

Fig. 9.

Stucco plate No. 2.

29. These facts will be better understood after examining the experiments on a second stucco plate. The trial of this plate was owing to an accident to the diffusiometer containing stucco plate No. 1. The diffusiometer was thereupon refitted with another plate similar to No. 1; but the old tin plates were replaced by new bright ones, and the new india-rubber rings were somewhat thicker than the old ones.

In making the experiments contained in Tables XII. and XIII., there was a slight change from the former plan, which had been to begin at the higher pressures and thence proceed by successive exhaustions to the lower pressures. This time one series of experiments was made as before, and another in the inverse manner—the diffusiometer being exhausted to commence with and the air or hydrogen being allowed to enter between the observations. Both series are given in the tables and are seen to agree very closely.

In the case of hydrogen it was found as before, that although not great, there was still a greater tendency to irregularity than with air, and as this was evidently due to diffusion through the india-rubber during the very considerable time (4 or 5 hours) which elapsed before the lower pressures were reached, several independent experiments were made. The diffusiometer being filled with pure hydrogen, was exhausted at once down to the particular low pressure at which the reading was taken, so as not to allow time for diffusion.

Differences of temperature brought to light by the log. curves.

30. The results with stucco plate No. 2 are smaller than with No. 1. At first sight it was thought that this difference was entirely owing to No. 2 being somewhat coarser than No. 1, but when the logarithmic homologues of the curves for this plate came to be compared with those for No. 1 and meerschaum No. 3, after the manner described in Art. 28, it became apparent that the difference in the results with plates No. 1 and 2 (stucco) was due to two causes. Some of it was due, as had been supposed, to the greater coarseness of No. 2, but a large part could only be explained on the assumption that from some cause or another the difference of temperature with No. 2 was less than with No. 1.

In fig. 10 it is seen that in order to bring the log. curves for stucco No. 2 into coincidence with the curves for stucco No. 1, it was necessary to increase the abscissæ of the former by ·048 = log 1·117: while the ordinates had to be increased by ·112. The difference in the abscissæ, as shown in Art. 28, represents the difference due to the coarseness of the plate; thus the openings in No. 2 are 1·117 times as broad as the openings in No. 1. And the difference between the differences of the ordinates and the abscissæ

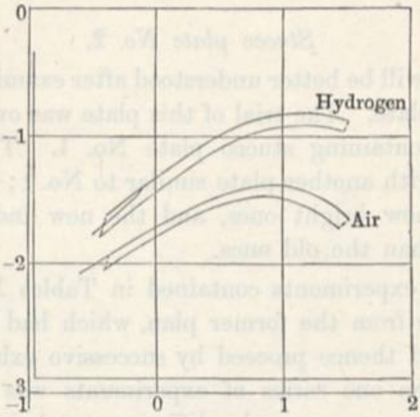


Fig. 10.

$= \cdot 112 - \cdot 048 = \cdot 064 = \log 1 \cdot 16$ is the logarithm of the effect of a difference of temperature, and to produce this effect the temperature of the water would have had to be lowered 15°. There was some difference, from 5° to 7°, leaving from 8° to 10° to be expressed as due to the bright tin plates and thicker rings.

Comparison of the logarithmic homologues.

31. In figures 8 and 9 the curves for meerschaum are drawn in the same position with reference to the axis, *OM, ON*. But while in figure 8 the curves for stucco No. 1 and No. 2 are shown in the position as plotted from the columns of logarithms in the Tables VI., VII., XII. and XIII., in figure 9 these curves have all been shifted until they coincide with the curves for meerschaum No. 3, in each case the two curves for air and hydrogen being shifted together. The axes are also shown as shifted with each pair of curves. The fitting of these curves is very remarkable; nor is it only the curves, for the points indicating the results are shown, and these all fall in so truly that it was hardly necessary to draw a line until the points of low pressure are reached. There is a slight deviation of that part of the curve for hydrogen, stucco No. 1, which represents the pressures below 1 inch; but this has been already explained as being due to the infusion of air through the india-rubber. In order to fully appreciate the force of this agreement, it must be borne in mind that it is not merely the portions of the curves that overlap that agree in direction, but the distance between the curves for hydrogen and air which have been shifted in pairs.

Nothing could prove more forcibly than this, that the different results obtained with different plates are quite independent of the nature of the gas so long as the densities are in the ratio of the fineness of the plates.

So far, therefore, as thermal transpiration is concerned, we have an absolute proof of Law V., Art. 9.

The relative coarseness of the plates.

The shifts in fig. 9 to bring the curves into coincidence being the logarithms of the corresponding pressures, it follows from Law V. that these shifts are the logarithms of the relative coarseness of the plates. Hence for the mean (after some law) diameters of the apertures we have:

Plate.	Coarseness.
Meerschaum No. 3	1
Stucco No. 1	5
Stucco No. 2	5·6

Further comparison of the results with the laws of Art. 9.

32. So far as the manner of variation of the differences of pressures with the density of the gas, this is completely shown by the shapes of the curves in figs. 4, 5, and 6, and is strictly according to Laws II. and III.

The agreement of the log. curves has been shown to confirm Law V. It only remains, therefore, to notice the laws of variation at the greatest and smallest pressures, to see how far these conform to the limits given in Laws III. and IV.

According to Law III., when the density of the gas is sufficient, the differences of pressure should be inversely proportional to the density.

Hence, according to this law, the product of the pressure into the difference of pressure should approximate to a constant quantity as the density increases.

In the case of stucco No. 2, we have, adding the two tables of logarithms, subtracting $·684 - 1 = \log ·483$, and taking out the numbers

Pressure	Pressure × difference of pressure ÷ ·483
30·25	1
30·05	1·004
28·05	·964
27·25	·957
25·85	·940
24·90	·927
23·15	·933

This sufficiently shows that the approximation is very close and according to Law III.

Coming now to the lower pressures, it will at once be seen that in all cases there is a tendency towards constancy. This is best seen in fig. 9, where the curves not only converge towards the left but turn towards the horizontal.

It is clear, however, from these curves, that the limit had not been reached, nor is it possible to say simply from the shape of the curves how far it might be off.

The following comparison, however, will show that the indication is in favour of Law IV., viz.: that the ultimate ratio which the difference of pressure bears to the mean pressure should be as the ratio which the difference of the square roots of the absolute temperature bears to the square root of the mean absolute temperature. According to this, we should have in the case of the meerschaum plate the ratio of the difference of pressure to the mean pressure equal to

$$\frac{\sqrt{212+461}-\sqrt{63+461}}{\sqrt{137\cdot 5+461}}=\frac{1}{8},$$

whereas, supposing that there was a difference of 20° between the surfaces of the meerschaum and the opposite tin plates

$$\frac{\sqrt{192+461}-\sqrt{83+461}}{\sqrt{137\cdot 5+461}}=\frac{1}{11},$$

between which values it is probable that the actual ratio lies.

The highest ratio of the difference of pressure to the mean pressure obtained is 1 to 13, and this may well be considered as an approximation to 1 to 11.

Thus, not only in their general features, but in the approximation towards definite limits, the experimental results show a close agreement with the laws as deduced from the theory.

Section III. Experiments respecting the Rate of Transpiration.

33. The experiments to be described in this section, besides being necessary for the verification of the Laws V., VI., and VII., Art. 9, were necessary to complete the verification of Law I. In the last section no direct notice was taken of the rate of thermal transpiration when unprevented by the difference of pressure on the two sides of the plate, and for this reason.

Although the thermal differences of pressure indicate in a general way the manner in which transpiration would have taken place had the pressure been equal, yet in order to examine the results strictly, as regards the various rates of thermal transpiration to which they correspond, it is necessary to know the exact law of transpiration for gases under pressure. The comparative rates of transpiration for different gases and the rates of transpiration of each gas for different pressures are not sufficient. So far, the laws established by Graham are all that can be desired, but these laws say nothing about the variation in the rate of transpiration consequent on a large variation in the density of the gas. Thus, Graham has shown that, through a fine graphite plate, the time of transpiration of a constant volume (measured at the mean pressure) will be exactly proportional to the difference of pressure, and will diminish slightly with the density, but his experiments were not carried to pressures many times less than the pressure of the atmosphere; whereas, for the purpose of this investigation, it was necessary to compare results at pressures as low as ·01 of an atmosphere. Nor is this the only point in which Graham's results appeared insufficient for the present comparison. Graham had found that the law of transpiration for a fine graphite plate differed essentially from the law for a stucco plate; his experiments having been made in both cases at pressures not many times less than the pressure of the atmosphere. Thus, for the stucco plate, the comparative times of transpiration of air and hydrogen were as 2·8 to 1, while for the graphite plate they were as 3·8 to 1. He had also shown that for plates of intermediate coarseness an intermediate ratio would maintain; but he had given no law that would enable us to predict the result with any particular plate.

In order, therefore, to effect my comparison, it was necessary, by actual experiment, to ascertain the rates of transpiration through my particular plates with the same gases as those used for thermal transpiration, and at similar pressures. It was this consideration which mainly determined the manner of making the experiments.

The apparatus.

34. The thermo-diffusiometer, without the streams of steam and water, after having undergone certain slight modifications, lent itself very well to this part of the investigation.

By means of extra branches from the tube KK, fig. 3, two 8 oz. flasks were connected with the chambers, one on each side of the porous plate, the object of these flasks being simply to enlarge the capacity of the chambers.

The branch to the flask on the right was outside the tap P, so that by closing this tap the flask would be cut off from the instrument, and the action of the pump would be confined to that one flask.

In this condition the mercury pump had a definite capacity—about 6 fluid oz., the capacity of the flasks was definite—about 8 fluid oz. each, and besides these there were the tubes and chambers in the diffusiometer also of definite capacity—about 3 oz. on each side of the plate.

The vacuum gauge was cut off during these experiments, so that the movement of the mercury in the siphon gauge constituted the only source of variation in capacity, and this was small.

This constancy in the capacity of the several parts of the apparatus, if not absolutely essential for these experiments, was very important, as it did away with the necessity of any process of reduction in comparing the results of the experiments at different pressures. This may be seen as follows.

Equal volumes.

35. Starting with the pump full of mercury, and the taps open so that the pressure, whatever it might be, is the same throughout the instrument, both taps being then closed, one stroke of the pump draws a definite proportion of the entire air in the instrument out of the right-hand flask, lowering the pressure in this flask in a definite ratio. Or in other words, one stroke of the pump withdraws from the flask on the right a definite volume of gas as measured at the pressure in the instrument.

This condition would be maintained until the tap P, between the right-hand flask and the instrument, was opened. Then the pressure on the right-hand side of the porous plate would fall in a definite ratio. Transpiration would commence, and by the time the pressure on the two sides of the plate had again become equal, a definite volume of air, about half that withdrawn by the pump, must have passed through the porous plate.

The time from the opening of the tap before complete equalisation is effected, is then seen to be the time of transpiration of a definite volume of gas measured at either the initial or the final pressures in the instrument, under differences of pressure which, although varying, are at corresponding stages proportional to the initial or final pressures in the instrument.

This time, which is called by Graham the time of transpiration of equal volumes, is directly measured in these experiments.

Measurement of the time.

36. The time at which transpiration commenced was the time at which the tap was opened, the tap and the tubes being sufficiently large to allow almost instantaneous adjustment of the pressures on the right of the porous plate. On first opening the tap P, the mercury in the siphon gauge was displaced, and as equalisation was re-established the mercury re-assumed its level position, the instant of complete transpiration being that at which the mercury became level.

The final adjustment of the mercury, however, was very slow, and it was not found possible, even with the cathetometer, to ascertain definitely the instant of complete equalisation. This threatened to be a difficulty, but it was finally overcome in a very simple manner.

Instead of waiting for complete equalisation, the time was taken at which the equalisation had proceeded, until the residual excess of pressure to the left of the plate bore a certain relation to the initial absolute pressure—·002 was the proportion allowed.

It will be seen that in this way the volume which passed, instead of being the volume for complete equalisation, was some definite proportion of this, and that the differences of pressure under which it passed were proportional to the initial difference of pressure, and hence the time occupied was the time of transpiration of equal volumes according to the previous definition.

The manner of experimenting.

37. The temperature of the room in which the diffusiometer was, having been read, the pump being full of mercury, and the taps D and P open so as to allow of complete equalisation through all the chambers of the instrument, the experiment commenced. The vacuum gauge was read; this gave the initial pressure in the instrument. The position of the mercury on the left side of the differential gauge was then read with the cathetometer.

From this reading was subtracted ·001 of the reading on the vacuum gauge, *i.e.*, the micrometer screw was turned through ten divisions for every inch pressure in the instrument.

The vacuum gauge was then cut off by pinching the india-rubber tubing; the taps P and D closed; one stroke of the pump was taken; a definite volume of air being thus drawn out of the flask, the pump was replaced so as to be full of mercury. Then at a given second, marked by a chronometer, the tap P was opened. A watch was then kept through the cathetometer, until the mercury in the differential gauge descended to line in the cathetometer. As the mercury was still in motion, this instant was well marked by merely raising the eyes to the chronometer.

The small losses of time (personal equations) between reading the chronometer and opening the tap, and reading the cathetometer and the chronometer, were determined as approximately equal to one second, which was accordingly subtracted from the time noticed.

In one set of experiments, that of hydrogen through stucco, the time of equalisation was so small (between 20 and 30 seconds) that a fraction of a second became a matter of some importance, and as the instant at which the eye reached the chronometer did not always correspond with the complete second or half second, there was a liability to this error; but this was to some extent obviated by making successive experiments for such small differences of pressure, that the differences in the reading were much less than a second, and passing over all the observations except those which corresponded with the beat of the chronometer.

With the stucco plates, both for air and hydrogen, three series of readings were taken, and the agreement was found to be very close.

With the meerschaum, the interval of transpiration was so long, about 12 minutes for air and about 3 minutes for hydrogen, that one series of experiments was considered to be sufficient.

It is important to notice here, that while making these experiments I had not the least idea as to how the results would come out when they came to be compared. This comparison was not made for several weeks, as the logarithmic method of comparing them had not occurred to me at the time the experiments were made.

The very remarkable agreement which has been found in the results cannot, therefore, be owing to any bias in my mind, but must be entirely attributed to the accuracy of the means of observation.

Purity of gases.

38. The greatest care was taken to get the gas pure and dry. And as it had been found in the previous experiments that when the pressure in the instrument was low, the gas, particularly the hydrogen, was liable to become contaminated by infusion through the india-rubber, the experiments were not continued to very low pressures and were made as rapidly as possible.

The results of the experiments.

39. Two plates were tried, meerschaum No. 3 and stucco No. 2, which were both in their respective diffusiometers just as they had been used for thermal transpiration. The results are given in the following tables:—

TABLE XIV. Time of transpiration of equal volumes of air at different pressures through stucco plate No. 2.

Initial pressure	Time of transpiration in seconds, July, 1878	Log of pressure	Log of time
inches			
30·10	54·5	1·478	1·7364
29·95	55	1·476	1·7404
26·25	58	1·418	1·7634
26·15	60	1·416	1·7781
22·95	62	1·36	1·7924
20·50	65	1·31	1·8129
19·90	66	1·30	1·8195
17·75	68	1·25	1·8325
15·40	72	1·19	1·8573
13·45	75	1·13	1·8750
11·60	79	1·06	1·8976
10·05	82	1·00	1·9138
8·75	85	·94	1·9294
5·90	90	·77	1·9542
5·35	93	·73	1·9684
5·20	92	·72	1·9638
4·75	95	·68	1·9777
4·50	95	·65	1·9777
3·90	97	·59	1·9867
3·85	97	·58	1·9867
3·40	98	·53	1·9912
2·95	100	·47	2·0000
2·50	101	·40	2·0040
·95	102	·98 − 1	2·0128

TABLE XV. Time of transpiration of equal volumes of hydrogen at different pressures through stucco plate No. 2.

Pressures before transpiration	Time of transpiration in seconds	Log of pressure	Log of time
inches			
30·10	19	1·48	1·2787
26·30	20	1·42	1·3010
18·6	21	1·27	1·3222
7·35	25	0·87	1·3979
5·50	26	0·74	1·4149
4·15	27	0·61	1·4313
3·75	28	0·57	1·4471
1·35	28·5	0·13	1·4540
1·20	28·5	0·08	1·4540

TABLE XVI. Time of transpiration of equal volumes of air at different pressures through meerschaum plate No. 2.

Initial pressure	Time of transpiration in seconds	Log of pressure	Log of time
inches			
30·	674	1·477	2·8286
15·10	716	1·179	2·8549
12·75	720	1·105	2·8573
11·25	724	1·051	2·8579
6·40	725	1·806	2·8600
6·00	725	1·792	2·8603

TABLE XVII. Time of transpiration of equal volumes of hydrogen at different pressures through meerschaum plate No. 3.

Initial pressure	Time of transpiration in seconds	Log of pressure	Log of time
inches			
32·5	187	1·5118	2·27184
13·25	198	1·1222	2·29665
12·40	198	1·0934	2·29666
4·05	200	1·6074	2·30103
3·85	200	1·5854	2·30103

From these tables it appears that the transpiration times at pressures nearly equal to that of the atmosphere are for air and hydrogen, through stucco, as 55 to 19, or 2·9 to 1, while through meerschaum they are as 3·6 to 1.

Graham found the ratio for stucco 2·8 to 1, and for graphite 3·8 to 1.

The small difference between these numbers may be well explained by supposing, as is quite probable, that the stucco used by Graham was rather coarser than plate No. 2, also that the graphite was finer than the meerschaum; but even allowing the difference, the present results are in very fair accord with Graham's as far as the conditions of pressure corresponded.

When, however, we come to compare the times for air and hydrogen at lower pressures, we see that not only does this ratio differ very greatly from that obtained by Graham for stucco, but that it approaches what he obtained with graphite. Thus at a pressure of 4 inches the ratio of the times are as 96 to 27, or 3·56 to 1, or they are the same as with the meerschaum at the pressure of the atmosphere. For lower pressures we have indications of a still higher ratio. Thus at 1 inch the ratio is 103 to 28·5, or 3·62 to 1.

In the same way we see that with the meerschaum, as the pressure falls, we have an increase in the difference of the times for air and hydrogen.

This variation in the comparative times for air and hydrogen is strictly in accordance with Law VI., Art. 9, as is also the manner of variation, as the pressure falls, of the times for each particular gas. These variations indicate that there are certain pressures for the stucco plate corresponding with certain other pressures for the meerschaum, at which the relation between the times for hydrogen and air are equal, and the variation of these times with the pressure similar.

Logarithmic homologues.

40. To test this, the logarithms of the pressures and times are plotted, and curves drawn, as explained in Art. 28. These are shown in fig. 11.

ab and *cd* are the curves for air and hydrogen through meerschaum, *ef* and *gh* are the curves for air and hydrogen through stucco. The figure consisting of the two curves *ef* and *gh* is found to fit on to the figure consisting of *ab* and *cd*, the displacement being from *ef* and *gh* to *e′f′g′h′*. The scale of the figure is too small to allow of the position of the points marking the experiments being shown, but these are shown in figure 12, page 298.

The agreement is there seen to be very close—the very considerable portions of the curves which overlap coming into actual coïncidence.

As previously explained with reference to the log. curves for thermal transpiration, the displacement $O'M$ in the direction of the abscissæ represents the logarithm of the ratio of corresponding pressures, while the displacement $O'N$ in the ordinates represents the log. of the ratio of the corresponding times for the two plates. This latter ratio cannot be made use of for the sake of comparison, as it involves the number of openings through the plate as well as their diameters.

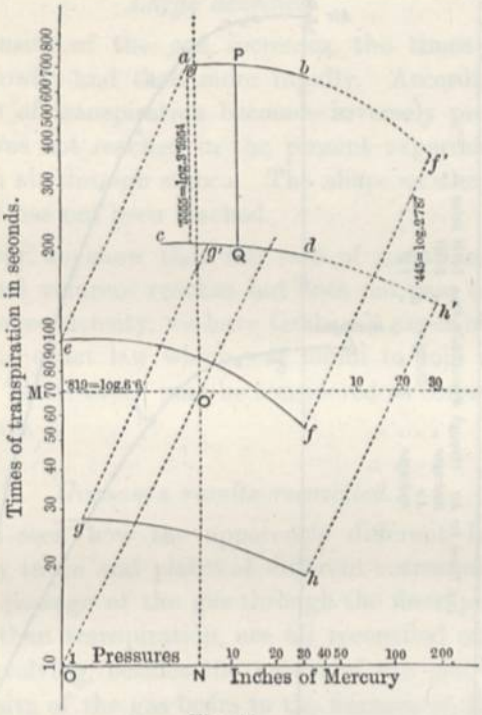


Fig. 11.

The relative coarseness of the plates.

41. The ratio of the corresponding pressures, as given by the difference in the abscissæ, is of the greatest importance. This ratio, according to Law V., Art. 9, corresponds with the ratio for the coarseness of the plates, and as these were the same plates as were used in thermal transpiration, it was to be expected that the results should agree; that is to say, the displacement $O'M$, fig. 11, should be equal to the displacement $O'M$, fig. 7. The actual measures give

For thermal transpiration, $O'M = \cdot 748 = \log 5\cdot 6$,

" transpiration under pressure, $O'M = \cdot 819 = \log 6\cdot 5$.

By this comparison the two independent and distinct experimental results check one another.

The difference in the results, although too small to cast a doubt upon their agreement, is too large to be attributed to experimental inaccuracy. But it must be remembered that the conditions under which the plates are compared differs in an important particular. In the experiments on thermal transpiration the plates were heated, whereas in the experiments on transpiration under pressure they were at the normal temperatures, and it appears

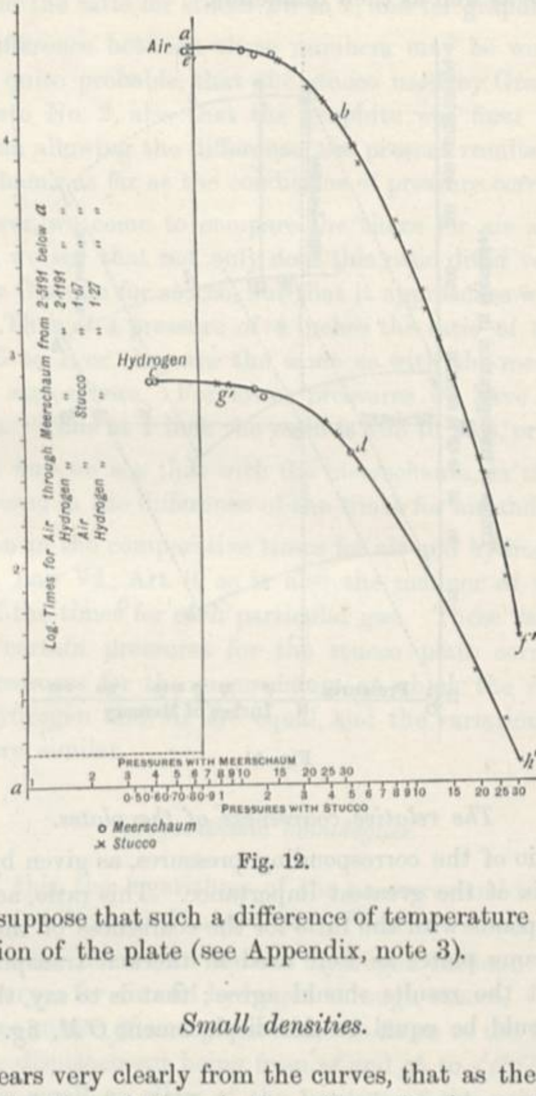


Fig. 12.

only natural to suppose that such a difference of temperature would somewhat alter the condition of the plate (see Appendix, note 3).

Small densities.

42. It appears very clearly from the curves, that as the pressure of the gas diminishes, the time of transpiration of equal volumes tends to become constant; approximate constancy having been reached in the experiments.

The ultimate ratio of the times of different gases was found by Graham to be as the square roots of the atomic weights of the gases, and the same ratio is obtained for air and hydrogen in these experiments. The square roots of the densities of dry air and hydrogen are 3·8 (3·79) to 1. The ratio of the times for air and hydrogen at the smallest pressures tried is 3·624, and as this is the result for both stucco and meerschaum the approximation is too close to be questioned, particularly when it is remembered that the smallest trace of impurity in the gases might cause the difference.

Large densities.

43. As the density of the gas increases, the times of transpiration diminish, at first slowly, and then more rapidly. According to Law VII., ultimately the time of transpiration becomes inversely proportional to the density; this rate was not reached in the present experiments, the nearest approach being with air through stucco. The shape of the curves, however, shows that the limit has not been reached.

In order, however, to show that the rate of variation of the times of transpiration of equal volumes reaches but does not pass beyond the rate of variation of the inverse density, we have Graham's experiments on capillary tubes, this being the exact law which was found to hold with all the gases and all the tubes. These tubes may be considered as corresponding with an extremely coarse plate.

Graham's results reconciled.

44. It is thus seen how the apparently different laws obtained by Graham for capillary tubes and plates of different coarseness, which led him to suppose that the passage of the gas through the finer plates more nearly resembled effusion than transpiration, are all reconciled and brought under one general law, involving, besides the nature of the gas, nothing but the ratio which the density of the gas bears to the fineness of the plate.

The verification of Law I.

45. The deduction of the comparative rates of thermal transpiration which would have ensued if the tap D in the thermo-diffusiometer had been open, is now only a matter of calculation. We have only to calculate by Law VI. the comparative rates of transpiration that would have resulted from the thermal differences of pressure. Hence it will be seen that Law I. follows from Laws II. and VI., Art. 9, and as these have both been verified, Law I. has also been verified.

SECTION IV. EXPERIMENTS WITH VERY SMALL VANES.

First experiments.

46. Before commencing the experiments on thermal transpiration described in Section II., I made an attempt to ascertain how far were borne out the theoretical conclusions that the necessity for extremely small pressures in the radiometer was owing to the comparatively large size of the

vanes, and that with smaller vanes similar results would be obtained at proportionally higher pressures.

The pressure, at which the impulsive force in the radiometer first becomes sensible, is so extremely small that it may be increased several hundred fold without becoming what may be called sensible—measurable by a mercurial gauge. So that on the assumption that the pressure, at which the effect would be apparent, increases proportionally as the size of the vanes diminishes, it was clear that in order to obtain the repulsive effect at the pressure of the atmosphere the size of the vanes must be reduced several thousand times.

The only means of obtaining such small vanes was to suspend a fibre of silk or a spider line. A single fibre of silk has a diameter of $\frac{1}{2000}$th of an inch (about), which is less than $\frac{1}{1000}$th the breadth of the vanes of the light mill on which my previous experiments had been made. But in order that the pressures at which the results would be sensible might be inversely proportional to the size of the vanes, the vanes should preserve the same shape; whereas the vanes in the light mill were square, while the fibre of silk was only narrow in one direction, which would be considerably to the disadvantage of the fibre of silk. More than this: it appeared probable that the thinness and transparency of the fibre, together with the cooling action of the air, would only allow an extremely small difference of temperature to be maintained on its opposite faces by radiant heat falling on one side; whereas air currents in the tubes, which would tend to carry the fibre with them, would be caused by the greater temperature of the glass on that side of the tube on which was the hot body, and these, which would be quite independent of the size of the fibre or vane, would exercise, proportionally, as great an effect on the fibre as on the larger vanes.

For the foregoing reasons a result was hardly probable, but as a preliminary step I suspended a fibre in a test tube ·7 inch in diameter and 5 inches long; I then brought a gas flame near to the tube to see if it would cause any motion in the fibre, the pressure of the air within the tube being that of the atmosphere.

The result was that the hair moved very slightly and somewhat uncertainly towards the flame.

As I had more than suspected that such would be the result at the pressure of the atmosphere, and as I had no means at hand for exhausting the tube, I postponed further experiments in this direction in order to take up the more promising investigation with the porous plates. When, however, I had concluded this, and succeeded almost beyond my expectation, I returned to the experiments on the fibre with the intention of exhausting the tube and using hydrogen as well as air.

Subsequent experiments.

47. These experiments were commenced on July 24, 1878.

A single fibre of unspun silk, having a thickness of ·0005 of an inch, was suspended in a test tube 1 inch in diameter and 7 inches long. The tube was closed with an india-rubber cork, through which passed a small glass tube to allow of exhaustion; this tube was connected with the vacuum gauge and the mercury pump, also with drying tubes for admitting dry air or hydrogen. A microscope with micrometer eye-piece reading $\frac{1}{10000}$th of an inch (the same as had formed the cathetometer in the previous experiments) was arranged for the observation of the motion of the fibre.

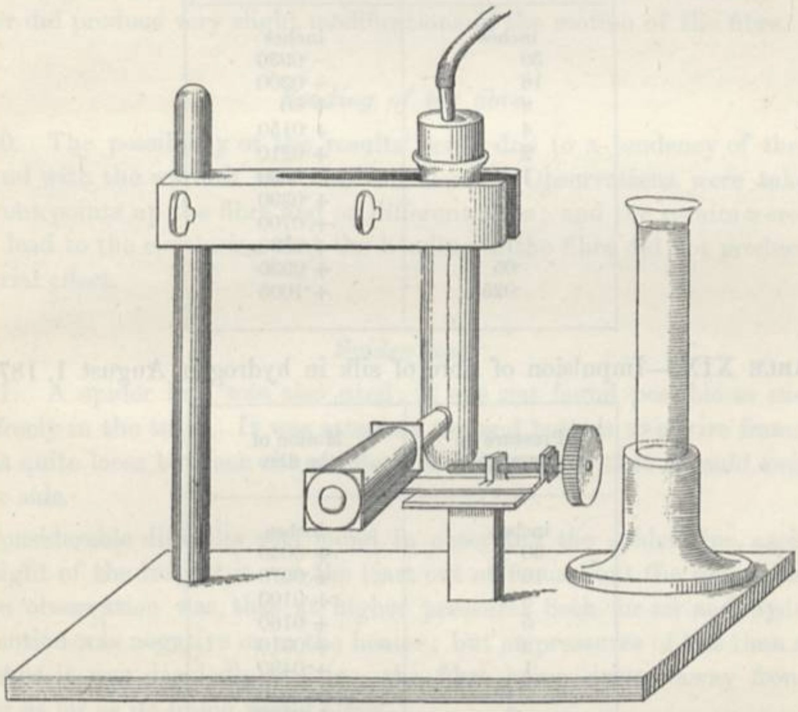


Fig. 13.

The apparatus as arranged is shown in fig. 13.

The tube having been dried was filled with dry air at the pressure of the atmosphere. A hot body was then brought near it.

In order to secure uniformity in the hot body, a test tube filled with boiling water was placed on a stand, which stand remained in the same position throughout the experiment, the water in the test tube being boiled the instant before the tube was placed on the stand.

The motion of the fibre was then watched through the microscope and measured.

Having ascertained the motion, the heater was removed and the fibre allowed to return to its normal position, which it always did with more or less exactness.

The tube was then exhausted to a limited extent and the operations repeated.

48. In this way were obtained a series of observations both for air and hydrogen at various pressures. These are shown in the following tables.

TABLE XVIII.—Impulsion of fibre of silk in air, August 1, 1878.

Pressure by vacuum gauge	Motion of the fibre
inches	inches
30	− ·0930
16	− ·0300
8	+ ·00
4	+ ·0150
2	+ ·0210
1	+ ·0230
·5	+ ·0390
·2	+ ·0700
·1	+ ·0830
·05	+ ·0930
·025	+ ·1005

TABLE XIX.—Impulsion of fibre of silk in hydrogen, August 1, 1878.

Pressure by vacuum gauge	Motion of the fibre
inches	inches
30	+ ·0040
16	+ ·0070
8	+ ·0100
5	+ ·0160
2	+ ·0310
1	+ ·0490
·40	+ ·0710
·2	+ ·0880
·1	+ ·1070

Table XVIII. shows that with air the result was negative until a pressure of less than 8 inches was obtained, it then became positive, and it was measurable at a pressure of 4 inches, and then steadily increased as the pressure fell, until for very small pressures the fibre moved through about 1,000 divisions on the micrometer.

With hydrogen, Table XIX. shows that the results were positive from the pressure of the atmosphere and for small pressures were somewhat larger than with air.

Although only one series of such observations is recorded in the table, the experiments were repeated several times with each gas. Also a flame was used instead of a heater, and the results were consistent throughout.

Elevation of the heater.

49. The effect of having the heater at different elevations was carefully studied, for it was obvious that this would affect the air currents in the tube. It was found, however, that the elevation of the heater did not produce any effect on the direction in which the fibre moved at pressures of less than 6 or 8 inches of mercury for air, and less than 20 inches for hydrogen. For pressures greater than these, considerable alterations in the elevation of the heater did produce very slight modifications in the motion of the fibre.

Bending of the fibre.

50. The possibility of the results being due to a tendency of the fibre to bend with the warmth was also considered. Observations were taken at different points up the fibre and on different sides; and the results were such as to lead to the conclusion that the bending of the fibre did not produce any material effect.

Spider line.

51. A spider line was also used: it was not found possible to suspend this freely in the tube. It was attached top and bottom to a wire frame, but it was quite loose between the points of attachment, so that it could swing to either side.

Considerable difficulty was found in observing the spider line, as it was lost sight of the instant it was the least out of focus; but the general result of the observation was, that at higher pressures both for air and hydrogen the motion was negative or to the heater; but at pressures of less than about 8 inches it was decidedly positive, the fibre being driven away from the heater as far as its frame would allow.

From the fact that the fibre of silk had shown positive motion so nearly up to the pressure of the atmosphere it might have been anticipated that the spider line, on account of its much greater thinness, would have shown positive motion even at pressures considerably above that of the atmosphere. But the reasoning of Art. 46 respecting the differences of temperature to be maintained and the effect of the air currents, obviously applies with greater force to the spider line than to the fibre of silk, and at once accounts for the observed fact that the positive motion with the spider line was not obtained until the pressures were somewhat lower than those necessary for the fibre of silk.

52. Both with the fibre of silk and the spider line, the phenomena of impulsion (the excess of pressure against warm surfaces) were apparent and consistent at densities many hundred times greater than the highest densities at which like results are obtained with vanes several hundred times broader than the fibre of silk; this verifies the theoretical conclusion on which this part of the investigation was based. The results in this case are not so definite as is the agreement of the logarithmic homologues in the instances of transpiration; but the one fact supports the other, and we may consider the law of impulsion—Law VIII., Art. 9—to have been sufficiently proved.

This concludes the experimental investigation.

PART II. (THEORETICAL).

Section V. Introduction to the Theory.

53. In suggesting in a former paper that the results discovered by Mr Crookes were due to the communication of heat from the surface of the solid bodies to the gas surrounding them, I pointed out as the fundamental fact on which I based my explanation, that when heat is communicated from a solid surface to a gas, the mean velocity of the molecules which rebound from the surface must be greater as they rebound than as they approach, and hence the momentum which these particular molecules communicate to the surface must be greater than it would be if the surface were at the same temperature as the gas.

So far the reasoning is incontrovertible. But in order to explain the experimental results, it was necessary to assume that the number of cold molecules which approached the hot surface would be the same as if the surface were at the same temperature as the gas, or at any rate, if reduced, the number would not be sufficiently reduced to counteract the effect of increased velocity of rebound.

Although at that time I could not see any definite proof of this, nor any way of definitely examining the question, yet I had a strong impression that the assumption was legitimate; and although I hoped at some future time to be able to complete the theoretical explanation, I was content for the time to rest the evidence of the truth of the assumptions involved on the adequacy of the reasoning to explain the experimental results obtained.

As other suggestions respecting the cause of the phenomena, widely different in character from mine, had found supporters, and a good deal of scepticism was expressed as to the fitness of the cause which I had suggested, my attention was occupied in deducing the actions which must result from such a force, and comparing them with experimental results. Having, however, at length satisfied myself, and seeing that a conviction was spreading that what I suggested contained the germ of the explanation, I set to work in earnest to complete the explanation, and ascertain by an extension of the dynamical theory of gases what effect the hot molecules receding from the surface should produce on the number and temperature of those approaching.

My first attempts to accomplish this were altogether unsuccessful. When contemplating the phenomena it seemed to me that I could perceive a glimmering of the method of reasoning for which I was in search, but as soon as ever I attempted to give definite expression to it this glimmering vanished.

The reason for this I now perceive clearly. When contemplating the phenomena, I had a surface of limited extent before me, and I considered the effect on such a surface without recognising the fundamental importance of the limit to size.

On the other hand, when I came to definite reasoning, for the sake of what appeared to be a simplification of the conditions of the problem, I assumed the surface to be without limit, thus introducing a fundamental alteration into the conditions of the problem without perceiving it.

The importance of this limit only became apparent to me when I found, by simple dynamical reasoning, that with surfaces of unlimited extent such results as those actually obtained would be impossible. This appeared as follows:—

No force on unlimited surface.

54. If we had two plane plates of unlimited extent, H and C, the surface of H opposite to C being hotter than the surface of C which was opposite to H, the outside surfaces of both plates being at the same temperature, then in order to produce results similar to those obtained with limited plates, the gas between the two plates must maintain a greater steady pressure on the plate H, than that which it exerts on the colder plate C. Whereas it is at once obvious that such a condition is contrary to the laws of motion, which require that the gas between the two surfaces should exert an equal and opposite pressure on both surfaces.

Having once perceived the force of this reasoning, it became clear to me that if, as I had supposed, the results obtained in the experiments were due to gaseous pressure, then they must depend on the limited extent of the surfaces.

This gave me the clue, in following which I have not only had the satisfaction of finding the explanation complete as regards the phenomena from which it originated, but I have also found that the theory indicated the phenomena of thermal transpiration, and explains much that hitherto has been considered anomalous respecting the laws of transpiration of gases through small channels—suggesting the experiments by which might be established the relation between these actions.

The manner in which the force arises in the case of a limited surface was at first rendered much clearer to me by considering an illustration, which I introduce here, although it forms no part of the proof which will follow.

55. Instead of H and C being plates with gas between them, let them be earthen batteries of *unlimited length*, and suppose that guns are distributed at uniform intervals along those batteries; suppose, also, that all the shot fired from H bury themselves in the earth of C, and *vice versâ*.

Then, in the first place, it is obvious that since on firing a shot the momentum imparted to the gun is equal and opposite to the momentum given to the shot, every shot fired from H will exercise the same force to move the battery H away from C as the shot will exercise to move C away from H; and in the same way the recoil of the guns on C will exercise the same tendency to move C away from H as the shot will exercise to move H away from C. And this will be the case whether the guns are supposed to be pointed straight across the interval between the batteries, or, *as I shall suppose, are pointed with various degrees of obliquity*.

Since, then, the result of every shot, whether fired from H or C, causes equal and opposite forces on the two batteries, the result of all the firing, no matter how much harder one battery may bombard than the other, must be to cause an equal force on each battery, the batteries being of unlimited length.

This case will be seen to be strictly analogous to the effect of the gas between two plates of unlimited extent to cause equal pressures on the plates, no matter what may be the differences in the temperature of the plates.

If now we consider the batteries of limited extent, then, owing to the obliquity of the guns, some of the shot from H may pass beyond the ends of C, and *vice versâ*; and in this case the force of recoil on the battery which fires will no longer be balanced by the stopping of the shot on the other battery. So that supposing the directions of firing to be similar, that battery which fires the hardest will be subject to the greatest tendency to move back.

The battery which fires the hardest corresponds with the hottest plate; and hence we perceive by analogy that, if of limited extent, the hottest plate will experience the greatest pressure from the gas between the plates.

56. The analogy between the batteries and the plates is rendered more strict if we suppose the batteries H and C to be two limited batteries, each placed in front of a battery of unlimited extent, and that these unlimited batteries are pounding away in an exactly similar manner.

The effect of the shot from these unlimited batteries on H and C will be analogous to the effect of the gas outside and beyond the plates. And it is at once seen that these unlimited batteries will produce similar effects on H and C respectively, and that the effect of the firing between H and C will be uninfluenced by the batteries behind, and therefore, as before, that battery will be subject to the greatest tendency to move back which fires the hardest.

To make the analogy between the two cases complete, suppose that H and C, in addition to pounding away at each other, are exactly returning the fire of the batteries from behind, and that the mean rate at which H fires at C and C at H are exactly the same as the rate at which the other firing goes on, but that the velocity of the shot from H is just as much greater than the mean velocity, as the velocity of the shot from C is below the mean. Then it is at once seen that the total tendency on H is to move back, while the total tendency on C is to move forward.

It obviously follows from the foregoing that the inequality in the forces on H and C could only occur at a certain distance from their ends, which distance would depend on the distance between the batteries; and hence that the ratio which this inequality (due to any particular rate of firing) would bear to the whole reaction on either battery would increase as the length of the batteries diminished; or in other words, the inequality of force would be proportional to the distance between the batteries, and would be constant whatever might be the length of the batteries beyond a certain point.

At first sight it may appear that the distance between the batteries H and C should be analogous to the distance between the hot and cold plates; but it is necessary to remember that it is only in case of the gas being extremely rare, as compared with the distance between the plates, that the molecules can be supposed to go straight from the one plate to the other. In ordinary cases the molecules encounter other molecules, and the effect of such encounters is to reduce the motion to a mean. Hence it appears that the distance between the batteries as affecting the equality in the reactions is somewhat analogous to the distance which a molecule may be supposed to travel without losing its characteristic motion. And hence it would appear that in the case of gas the inequalities of force on the two plates would be proportional to the inverse density of the gas and the extent of the boundaries of plates.

57. The shot from H which miss C, and those from C which miss H,

must be stopped by the outside batteries. Therefore the inequalities in the forces on H and C will be balanced by inequalities in the forces on the batteries behind, and the sum of the forces on H and the battery behind will be equal to the sum of the forces on C and the battery behind.

And this is strictly analogous to the result of Schuster's experiment, viz.: that the effect upon the vanes of the light mill is exactly balanced by the effect on the containing vessel.

58. The batteries also serve to illustrate the action of thermal transpiration. In the case already considered (Art. 57) the inequality between the shot from H which miss C and those from C which miss H is transferred to the outside batteries, or in the case of the gas, to the containing vessel. The better to illustrate the present point, suppose that the outside batteries are ranged across the ends of the open space between H and C. This will make no difference to the result. The inequality of the action of the shot which miss H and C must now cause a force parallel to the end batteries, tending to cause these batteries to move end-wise in the direction of C.

Suppose that the two batteries H and C were free to move together in the direction from C to H (suppose them on a truck). The inequality in the force would set them in motion in this direction, which motion would increase until the actual velocity of the shot from C equalled the actual velocity of the shot from H; then all inequalities in the reactions would cease, and there would be no reactions on the limiting batteries.

In this case the limiting batteries are obviously analogous to the sides of a tube, and the interval between the planes H and C corresponds with a layer of gas at equal pressures, but across which the heat is being conducted by the greater velocity of the molecules which move from H to C; and the conclusion is that such a layer of gas when maintained at rest exerts a tangential force on the sides of the tube tending to move the tube in the direction of the flow of heat, whereas if the gas were free to move it would flow towards the hottest end; and this is the phenomenon of thermal transpiration.

59. The foregoing illustration, with the exception that the action is confined to a plane instead of being distributed through a space, is more than analogous: it is strictly parallel to the case of gas as long as the gas is so rare that the molecules proceed straight across the intervals between the plates or sides of a tube. When this is the case, therefore, the example of the batteries explains the phenomena of thermal transpiration as well as the phenomena of the radiometer. But when the gas is so dense that in crossing the interval between the surfaces the molecules undergo several encounters, the parallelism no longer holds. Even then, however, the analogy holds, for

the gas at any point may be considered as consisting of two sets of molecules which are moving across a plane from opposite sides. And by examining the difference in the velocity of these two sets of molecules a general explanation of many of the phenomena may be obtained without recourse being had to a strict analytical investigation. The analogy has, however, been pursued far enough to serve the purpose of an introduction.

Before proceeding to the mathematical investigation, which is novel and somewhat intricate, I have thought it advisable to further introduce it by a short description of the method used and the assumptions involved.

Prefatory description of the mathematical method.

60. The characteristic as well as the novelty of this investigation consists in the method by which not only the mean of the motions of the molecules at the point under consideration is taken into account, but also the manner in which this mean motion may vary from point to point in any direction across the point under consideration. It appears that such a variation gives rise to certain stresses in the gas (tangential and normal), and it is of these stresses that the phenomena of transpiration and impulsion afford evidence.

Instead of considering only the condition of the molecules comprised within an elementary unit of volume of the gas, what is chiefly considered in this investigation is the condition of the molecules which cross a plane supposed to be drawn through the point, which plane may or may not be in motion along its normal.

The molecules which cross this plane are considered as consisting of two groups, one crossing from the positive to the negative side of the plane, and the other crossing from the negative to the positive side. Considered in opposite directions, the mean characteristics (the number, mass, velocity, momentum, energy, &c.) of these two groups are not necessarily equal: they may differ in consequence of the motion of the gas, the motion of the plane through the gas, or a varying condition of the gas. And the determination of the effects of these causes on the mass, momentum, and energy that may be carried across by either group is the more general result of the investigation.

61. As a preliminary step, it is shown that whatever may be the nature of the encounters between the molecules within a small element, the encounters can produce no change on the mean component velocities of the molecules which in a definite time pass through the element; and hence, whatever may be the state towards which the encounters tend to reduce the gas, this state must be such that the mean component velocities of the

molecules which pass through the element in a unit of time remain unaltered. These mean component velocities, it is to be noticed, are not the mean component velocities of the molecules within an element at any instant.

Certain assumptions are then made. These do not involve any law of action between the molecules. They are equivalent to assuming that the tendency of the encounters within an element is to reduce the gas to a uniform state.

From these assumptions two theorems (I. and II.) are deduced. From theorem I. it follows that the rate of approximation to a uniform gas is inversely proportional to a certain distance s, which distance is inversely proportional to the density, and is some unknown function of the mean velocity of the molecules. From theorem II. it follows that the molecules which enter a small element from any particular direction, arrive as if from the uniform gas, to which the actual gas tends at a point distant s in the direction from which the molecules come.

When the gas is continuous about the element for distances large compared with s, then s is independent of the direction from which the molecules come; but near a solid surface s is a function of this direction and of the position of the element with respect to the solid surface.

These theorems are fundamental to all the reasoning which follows; and the distance s enters as a quantity of primary importance into all the results obtained.

It is proposed to call this distance the *mean range* of the characteristics of the molecules. Thus we have the mean range of the mass, the mean range of momentum, and the mean range of energy. By qualifying the term "mean range" by the name of the quantity carried, instead of considering it as a general characteristic of the condition of the gas, two things are avoided—

(1) It is not implied that the mean range is the same for all the quantities which may be considered;

(2) There is no fear of confusing the mean range with the mean path of a molecule.

The mean range, whatever may be the nature of the quantity considered, is obviously a function of the mean path of the molecules, and is a small quantity of the same order as the mean path, but it also depends on the nature of the impacts between the molecules.

The symbol s is used to express the mean range of any particular quantity Q.

62. Assuming that the mean value of Q for the molecules in an elementary unit of volume at a point is a function of the position of the point, the aggregate value of Q carried across the plane at a point is obtained in a series of ascending powers of s. And by neglecting the terms which involve the higher powers of s, which terms also involve differentials of Q of orders and degrees higher than the first, equations are obtained between s and the aggregate value of Q carried across the plane.

63. The dynamical conditions of steady momentum, steady density, and steady pressure are next considered. General equations are obtained for these conditions, which general equations involve s, the motion of the plane and other quantities depending on the condition of the gas.

The condition that there may be no tangential stress in the gas is also considered.

It is found that when there is no tangential stress on a solid surface wherever it may be in the gas, the mean component velocities of all the molecules which pass through the element in a definite time must be zero at all points in the gas.

64. The equations of motion are then applied to the particular cases which it is the object of this investigation to explain. Two cases are considered. The first, that of a gas in which the temperature and pressure only vary along one particular direction, so that the isothermal surfaces and surfaces of equal pressure are parallel planes; this is the case of transpiration. The second case is that in which the isothermal surfaces and the surfaces of equal pressure are curved surfaces (whether of single or double curvature); this is the case of impulsion and the radiometer.

As regards the first case, the condition of steady pressure proves to be of no importance; but from the conditions of steady momentum and steady density an equation is obtained between the velocity of the gas, the rate at which the temperature varies, and the rate at which the pressure varies; the coefficients being functions of the absolute temperature of the gas, the diameters of the apertures, and the ratio of the diameters of the apertures to the mean range. These coefficients are determined in the limiting conditions of the gas, when the density is small and large, and as they vary continuously with the condition of the gas, the limiting values afford indications of what must be the intermediate values.

From this equation, which is the general equation of transpiration, the experimental results, both as regards thermal transpiration and transpiration under pressure, are deduced.

In dealing with the second case, that in which the isothermal surfaces are curved, the three conditions—steady momentum, density, and pressure—are all of them important. These conditions reduced to an equation between the motion of the gas, the variation in the absolute temperature, and the variation in pressure, in which, as in the equation of transpiration, the coefficients are functions of the absolute temperature, the diameters of the apertures, and the ratios of the diameters of the apertures to the mean range.

The reduction of the conditions of equilibrium to this equation, however, involves the assumption that the gas should not be extremely rarefied. In order to take this case into account a particular example is examined, and the equation so obtained, together with the equation obtained from the conditions of steady motion, is shown to lead to the results of impulsion and the phenomena of the radiometer.

Section VI. Notation and Preliminary Expressions.

65. In arranging the notation I have endeavoured as far as possible to make it similar to the notation already adapted to the kinetic theory of gases by previous writers. With this object I have adopted almost entirely, both as regards symbols and expressions, the notation used by Professor Maxwell in his paper "On the Dynamical Theory of Gases*." But his notation, copious as it is, has fallen far short of my requirements. I have had to take under consideration certain quantities which have not hitherto been recognised; and what has particularly taxed my resources in symbolising, is that I have had, according to my method, to devise symbols to express each of twenty-four partial or component quantities which spring from any one of certain quantities, which have hitherto been dealt with as simple quantities.

Explanation of the symbols.

66. u, v, w, are used to represent the component velocities of a molecule with reference to the fixed axes x, y, z.

ξ, η, ζ are used to represent the component velocities of a molecule with reference to axes parallel to x, y, z, but which move with the halves of the mean component velocities of the molecules which pass through an element in a definite time.

U, V, W are used to represent the component velocities of the moving axes.

* *Phil. Trans.* 1867.

Throughout this investigation bars over the symbols indicate the mean taken over some group of molecules; when no further indication as to the particular group is given, it is to be understood that the mean is taken from the entire group in a unit of volume at the same instant. Thus $\overline{\xi^2}$, $\overline{\eta^2}$, $\overline{\zeta^2}$ indicate the mean squares of ξ, η, ζ respectively for all the molecules in a unit of volume of uniform gas which is in the same mean condition as the gas at the point considered.

Q is used to represent any quantity belonging to a molecule, such as its mass, momentum, energy, &c.

$\Sigma(Q)$ is used to represent the aggregate value of Q for a group of molecules as existing in a unit of volume; and when no further indication is given it will be understood that the aggregate is that of the entire group.

$\sigma(Q)$ indicates the aggregate value of Q carried across a unit of plane area in a unit of time by a group of molecules, which in the absence of further indication will be understood to be the entire group which crosses the plane.

$\sigma_x(Q)$, with the suffix, is used to express the direction of the plane as well as the aggregate value carried across it.

$\overset{u+}{\sigma_x}(Q)$, with the superimposed symbol, expresses the group over which the summation extends; $u+$ indicates that the summation is taken over all those molecules which are moving in the positive direction as regards the axis of x. By varying the superimposed symbol, the general symbol may be made to express the value of Q carried by a group of molecules having any particular motion across the plane indicated by the suffix.

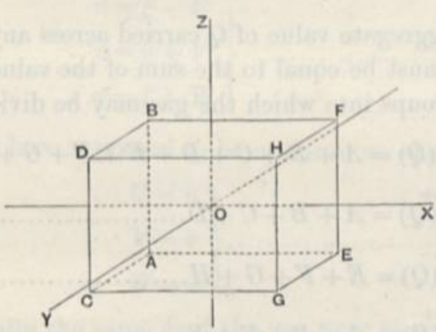


Fig. 9.

67. As indicated by the signs of the component velocities, the molecules in a unit of volume, or the molecules which cross a surface at a point in a unit of time, will be divided into eight groups.

These groups may be indicated by the eight corners of a cube, having its edges parallel to the axes, circumscribed about the point considered. Thus in fig. 9 the group which have $u+$, $v+$, $w+$, will approach O from the region indicated by the corner A, and similarly there will be a corner for each group. The particular groups, therefore, may be distinguished by the letters at the corners of the cube, fig. 10. And instead of

$$\overset{\substack{w+\\v+\\u+}}{\Sigma}(Q),\ \overset{\substack{w-\\v+\\u+}}{\Sigma}(Q),\ \overset{\substack{w+\\v-\\u+}}{\Sigma}(Q),\ \overset{\substack{w-\\v-\\u+}}{\Sigma}(Q),\ \overset{\substack{w+\\v+\\u-}}{\Sigma}(Q),\ \overset{\substack{w-\\v+\\u-}}{\Sigma}(Q),\ \overset{\substack{w+\\v-\\u-}}{\Sigma}(Q),\ \overset{\substack{w-\\v-\\u-}}{\Sigma}(Q)$$

we have respectively,

$$\overset{a}{\Sigma}(Q),\ \overset{b}{\Sigma}(Q),\ \overset{c}{\Sigma}(Q),\ \overset{d}{\Sigma}(Q).\ \overset{e}{\Sigma}(Q),\ \overset{f}{\Sigma}(Q),\ \overset{g}{\Sigma}(Q),\ \overset{h}{\Sigma}(Q).$$

And in order still further to simplify the notation, instead of

$$\overset{a}{\sigma}(Q),\ \overset{b}{\sigma}(Q),\ \overset{c}{\sigma}(Q),\ \overset{d}{\sigma}(Q),\ \overset{e}{\sigma}(Q),\ \overset{f}{\sigma}(Q),\ \overset{g}{\sigma}(Q),\ \overset{h}{\sigma}(Q)$$

we may write respectively the simple letters

$$A,\ B,\ C,\ D,\ E,\ F,\ G,\ H.$$

68. The method of considering the value of Q carried across a plane by groups of molecules distinguished by the directions in which they are moving, constitutes the essential means by which the results of this investigation are arrived at. And as it does not appear that this method has been resorted to by any previous writer, it appears necessary for me to describe at some length the preliminary steps.

The rate at which Q is carried across a plane.

69. Since the aggregate value of Q carried across any plane by the entire group of molecules must be equal to the sum of the values of Q carried across by all the various groups into which the gas may be divided, we have

$$\sigma(Q) = A + B + C + D + E + F + G + H \text{ ....(1)}$$

$$\overset{u+}{\sigma}(Q) = A + B + C + D \text{ ....(2)}$$

$$\overset{u-}{\sigma}(Q) = E + F + G + H \text{ ....(3)}$$

$$\overset{v+}{\sigma}(Q) = A + B + E + F \text{ ....(4)}$$

$$\overset{v-}{\sigma}(Q) = C + D + H + G \text{ ....(5)}$$

$$\overset{w+}{\sigma}(Q) = A + C + E + G \text{ ....(6)}$$

$$\overset{w-}{\sigma}(Q) = B + D + F + H \text{ ....(7)}$$

Gas in uniform condition.

70. When the gas is uniform, whether at rest or in motion, the value of $\sigma(Q)$ has already been determined by Professor Maxwell, but it is necessary to transform the expressions to the notation of this paper.

We have by a well-known formula*

$$\left.\begin{aligned} A_x &= \overset{a}{\Sigma}(uQ), \\ A_y &= \overset{a}{\Sigma}(vQ), \\ A_z &= \overset{a}{\Sigma}(wQ), \end{aligned}\right\} \quad \text{.................................} (8)$$

in which the suffixes x, y, z, indicate that it is the planes yz, zx, xy, that Q is being carried across, and the superimposed symbols a a a indicate the group of molecules over which the summation extends.

We have also

$$\left.\begin{aligned} B_x &= \overset{b}{\Sigma}(uQ), \\ B_y &= \overset{b}{\Sigma}(vQ), \\ B_z &= \overset{b}{\Sigma}(wQ), \end{aligned}\right\} \quad \text{.................................} (9)$$

and similar expressions for the values of Q carried across each of the other planes by all the other groups.

In the equation (8) and similar equations we may obviously substitute for u, v, w their values

$$\left.\begin{aligned} u &= \xi + U \\ v &= \eta + V \\ w &= \zeta + W \end{aligned}\right\} \quad \text{.................................} (10)$$

And since the gas is here supposed to be uniform, we shall have

$$\left.\begin{aligned} U &= \bar{u} \\ V &= \bar{v} \\ W &= \bar{w} \end{aligned}\right\} \quad \text{.................................} (11)$$

ξ, η, ζ being identically the same as if the gas were at rest.

71. For the purpose of this investigation it is necessary to express such quantities as $\overset{u+}{\sigma_x}(Q)$, $\overset{u-}{\sigma_x}(Q)$ in terms of the groups distinguished by the signs of ξ, η, ζ, instead of u, v, w; and owing to the fact that in all the cases to be

* "On the Dynamical Theory of Gases," Maxwell; *Phil. Trans.* 1867, p. 69.

considered U^2, V^2, W^2 are of the second order of small quantities compared with $\overline{u^2}$, $\overline{v^2}$, $\overline{w^2}$ this may be done. For we may put

$$\overset{u+}{\sigma_x}(Q) = \Sigma\,\overset{u+}{\{(\xi + U)\,Q\}} = \Sigma\,\overset{\xi+}{\{(\xi + U)\,Q\}} + \Sigma\,\overset{u+\text{ and }<U}{\{(\xi + U)\,Q\}} \;\ldots\ldots (12)$$

$$\overset{u-}{\sigma_x}(Q) = \Sigma\,\overset{u-}{\{(\xi + U)\,Q\}} = \Sigma\,\overset{\xi-}{\{(\xi + U)\,Q\}} - \Sigma\,\overset{u+\text{ and }<U}{\{(\xi + U)\,Q\}} \;\ldots\ldots (13)$$

and when U is small compared with $\sqrt{\overline{u^2}}$, the last term on the right in each of these equations will be small to the second order as compared with the first term. For the number of molecules over which the summation in these terms extends is to the whole number of molecules in a unit of volume in something less than the ratio of U to $\sqrt{\overline{u^2}}$. Hence, as will subsequently appear, in neglecting these last terms we shall be neglecting nothing within the limits of our approximation. We have therefore

$$\left.\begin{aligned} \overset{u+}{\sigma_x}(Q) &= \Sigma\,\overset{\xi+}{\{(\xi + U)\,Q\}} \\ \overset{u-}{\sigma_x}(Q) &= \Sigma\,\overset{\xi-}{\{(\xi + U)\,Q\}} \end{aligned}\right\} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (14)$$

and similarly for all other groups. Thus it appears that the letters a, b, c, &c., may be used indifferently to indicate the groups as distinguished by the signs of u, v, w or of ξ, η, ζ.

Distribution of velocities amongst the molecules.

72. Although not actually essential to this investigation, as it will tend greatly to simplify the results obtained, I shall adopt the conclusion arrived at by Professor Maxwell* with respect to the distribution of velocities amongst the molecules of a uniform gas, viz.:—

$$dN = \frac{N}{\alpha^3 \pi^{\frac{3}{2}}} e^{-\frac{\xi^2+\eta^2+\zeta^2}{\alpha^2}} d\xi d\eta d\zeta \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (15)$$

where N is the whole number of molecules in a unit of volume, and dN the number whose component velocities lie between ξ and $\xi + d\xi$, η and $\eta + d\eta$, and ζ and $\zeta + d\zeta$.

From equation (15) we have for a uniform gas

$$\overset{a}{\overline{\xi}} = -\overset{e}{\overline{\xi}} = \frac{\alpha}{\sqrt{\pi}} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (16)$$

$$\overset{a}{\overline{\xi^2}} = \overset{e}{\overline{\xi^2}} = \frac{\alpha^2}{2} \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (17)$$

$$\overset{a}{\overline{\xi^3}} + \overset{a}{\overline{\xi\eta^2}} + \overset{a}{\overline{\xi\zeta^2}} = \frac{2}{\sqrt{\pi}}\alpha^3 \ldots\ldots\ldots\ldots\ldots\ldots\ldots (18)$$

$$\overset{a}{\overline{\xi\eta}} + \overset{b}{\overline{\xi\eta}} = \overset{a}{\overline{\eta\zeta}} + \overset{e}{\overline{\eta\zeta}} = \overset{a}{\overline{\zeta\xi}} + \overset{e}{\overline{\zeta\xi}} = \frac{\alpha^2}{\pi} \ldots\ldots\ldots\ldots\ldots (19)$$

* *Phil. Trans.* 1867, p. 65.

Also if τ is the absolute temperature of the gas, p the intensity of pressure, M the mass of a molecule, and ρ the density of the gas, we have for uniform gas

$$\frac{\tau}{M} = \kappa^2 \alpha^2 \qquad (20)$$

$$p = \rho \frac{\alpha^2}{2} \qquad (21)$$

$$\frac{p}{\rho} = \frac{2}{\kappa^2} \frac{\tau}{M} \qquad (22)$$

in which κ^2 varies with the nature of the gas, and is otherwise constant.

73. The adoption of equations (15) to (22) restricts the application of the results that may be arrived at to gases of uniform molecular texture such as air and hydrogen. For these equations do not apply to a varying mixture of gases. In order to render them applicable to such a mixture it would be necessary to consider throughout the investigation the presence of at least two systems of molecules. This would add greatly to the complication, whereas none of the experimental results which it is my immediate object to explain involve a varying mixture.

It will be seen, however, that at least one important result which has not hitherto been explained could be fully explained in this way. This is the transpiration of a varying mixture of two gases through a porous plate. The possibility of such an explanation will be seen from the results obtained for a simple gas.

74. Table XX. contains all the values of $\sigma(Q)$ carried across the axial planes by the several groups of molecules in a uniform gas, for all the quantities Q which are important in this investigation.

TABLE XX. Showing the values of Q carried across the axial planes by the several groups of molecules.

	$Q=M$	$Q=Mu$	$Q=Mv$	$Q=Mw$	$Q=M(u^2+v^2+w^2)$
A_x	$\frac{\rho}{8}\left(U+\frac{a}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(\frac{a^2}{2}+2\frac{aU}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(+\frac{aV}{\sqrt{\pi}}+\frac{a^2}{\pi}+\frac{aU}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(+\frac{aW}{\sqrt{\pi}}+\frac{a^2}{\pi}+\frac{aU}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(\frac{5}{2}a^2U+\frac{2a^3}{\sqrt{\pi}}+\frac{2a^2}{\pi}(V+W)\right)$
B_x	+	+	+ + +	+ − −	+ + −
C_x	+	+	+ − −	+ + +	+ − +
D_x	+	+	+ − −	+ − −	+ − −
E_x	−	−	− − +	− − +	− − −
F_x	−	−	− − +	− + −	− − +
G_x	−	−	− + −	− − +	− + −
H_x	−	−	− + −	− + −	− + +
A_y	$\frac{\rho}{8}\left(V+\frac{a}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(+\frac{aU}{\sqrt{\pi}}+\frac{a^2}{\pi}+\frac{aV}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(\frac{a^2}{2}+\frac{2aV}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(+\frac{aW}{\sqrt{\pi}}+\frac{a^2}{\pi}+\frac{aV}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(\frac{5}{2}a^2V+\frac{2a^3}{\sqrt{\pi}}+\frac{a^2}{\pi}(W+U)\right)$
B_y	+	+ + +	+	+ − −	+ − +
C_y	−	− − +	−	− − +	− − −
D_y	−	− − +	−	− + −	− + −
E_y	+	+ − −	+	+ + +	+ + −
F_y	+	+ − −	+	+ − −	+ − −
G_y	−	− + −	−	− − +	− − +
H_y	−	− + −	−	− + −	− + +
A_z	$\frac{\rho}{8}\left(W+\frac{a}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(+\frac{aU}{\sqrt{\pi}}+\frac{a^2}{\pi}+\frac{aW}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(+\frac{aV}{\sqrt{\pi}}+\frac{a^2}{\pi}+\frac{aW}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(\frac{a^2}{2}+\frac{2aW}{\sqrt{\pi}}\right)$	$\frac{\rho}{8}\left(\frac{5}{2}a^2W+\frac{2a^3}{\sqrt{\pi}}+\frac{a^2}{\pi}(U+V)\right)$
B_z	−	− − +	− − +	−	− − −
C_z	+	+ + +	+ − −	+	+ + −
D_z	−	− − +	− + −	−	− − +
E_z	+	+ − −	+ + +	+	+ − +
F_z	−	− + −	− − +	−	− + −
G_z	+	+ − −	+ − −	+	+ − −
H_z	+	− + −	− + −	−	− + +

In reading Table XX. it is to be understood that the several terms for B, C, D, E, F, G, H are the same as for A, with the exception of such changes of signs as are indicated in the table.

Terms of the order U^2 and U^3 have been omitted as being too small to affect the results of the investigation.

Section VII. The Mean Range.

75. So far the gas has been supposed to be in uniform condition as regards space as well as time. When the condition varies from point to point, the results given in Table XX. will not hold good, for the condition of the molecules, arriving from any particular direction, which cross a plane at a point A will not be determined by the mean condition of the gas at A, but rather by the mean condition at the points at which the molecules receive the direction and velocity with which they cross the plane.

These points will not necessarily be the points at which the molecules last undergo encounter before crossing the plane, for one encounter may not be sufficient completely to modify their motion. In order, therefore, to determine from first principles the manner in which the molecules approach the point A, we must know the law of action between the molecules, and even then the complete solution would present difficulties which appear to be insuperable.

Fortunately, however, for the purposes of the present investigation a complete solution is not necessary. The point that has mainly to be considered is the effect of a solid surface on the mean condition of the molecules which cross a plane in its immediate neighbourhood. And the principal question is not how far such an effect would extend into gas in a particular condition, but what would be the nature of the effect at points to which it does extend, and what would be the comparative range of similar effects in gases the condition of which differ with respect to density and variation of temperature? If it should be found that the number and mean condition of the molecules which arrive at A from a given direction, partake in a definite manner of the condition of the gas at a point in that direction whose distance s from A is a definite function of the density of the gas and some function of the temperature; such a solution would be sufficient to allow of the deduction of results corresponding to the experimental results.

Now it appears to follow from the view propounded at the commencement of this article, that in the interior of the gas there *must* be some distance s from a point A at which the mean condition of the gas must represent the mean condition of the molecules which reach A from that direction. This language is somewhat vague, but so must be the first idea. On closer inspection the question naturally arises as to what is meant by the mean condition of the gas, and by the mean condition of the molecules which reach A? Nor does this question at first sight appear to be difficult to answer.

The mean condition of the gas appears most naturally to resolve itself into that which we can measure—the density ρ, the mean pressure

$$\frac{\rho}{3}\left(\overline{u^2} + \overline{v^2} + \overline{w^2}\right)$$

and the mean component velocities $\bar{u}, \bar{v}, \bar{w}$; and with respect to the mean condition of the arriving molecules why should not this be measured by their density, their mean energy and their mean component velocities? On comparing these with the corresponding quantities for the gas just mentioned, one point of doubt presents itself: in the mean component velocities of the molecules arriving from one direction we have a very different thing from the mean component velocities of the gas. However, ignoring this caution, the most obvious supposition appears to be that as an approximation towards the condition of the molecules as they arrive at A, we may suppose them to come from a *uniform* gas having the density, mean pressure and component velocities of the gas at a point distant s from A in the direction from which they arrive. Such an assumption can be worked out, and the results compared with known experimental results. But we need go no farther than the case of gas at equal pressure and varying temperature. As applied to this case, our supposition leads to the inevitable conclusion that, unless s is zero, such a gas must be in motion from the colder to the hotter part with a velocity greater than its actual velocity, whatever this may be, which is absurd. This brings us back to the caution already mentioned respecting the difference between the component velocities of the group of molecules approaching A, and the component velocities of the gas. Without attempting to investigate this difference from first principles, we may follow the obvious course of attributing certain arbitrary mean component velocities to the uniform gas, as from which the molecules are supposed to arrive at A.

We now suppose the molecules to arrive at A as from a uniform gas having the mean pressure and density at a distance s as before, but having arbitrary component velocities U, V, W (where U, V, W are so small that their squares may be neglected). This gets over the difficulty in the case mentioned above, for U, V, W being arbitrary can be so determined that the gas resulting from all the groups arriving at A shall have any mean velocity, and hence the mean velocity of the gas. It is only one such case, however, that we can meet in this way; for having once determined U, V, W, they are no longer arbitrary, and hence if the calculated results fit, to the same degree of approximation, all other cases, it must be that the approximation is a true one.

This test, however, can only be partially applied. As worked out in the subsequent sections of this paper, it was found that the supposition explained the phenomena of the radiometer and suggested the laws of transpiration and thermal transpiration exactly as they were afterwards realised. And in so far as they can be compared there is a complete agreement between the theoretical and experimental results.

Under these circumstances, the course which I first adopted in drawing up this paper was to found the theoretical investigation on such an assumption as has just been discussed.

The only other course was to look to first principles for the evidence wanting to establish the truth of the assumption. This I had attempted.

Obviously the first step in this direction was to examine the values of U, V, W, as determined by the case of gas at varying temperature and uniform pressure. This showed *that if a plane be supposed to be moving through the gas with velocities U, V, W, then, measured with respect to the moving plane, the aggregate momenta carried from opposite sides across the plane are equal.*

This fact appeared pointed, but the exact point of it was not at once obvious, nor did it fully occur to me until I had completed the investigation founded on the assumption as already described.

Subsequently, however, working at the subject from the other end, so to speak, I came to see that whatever might be the action between the molecules, the probable effect of encounters in a varying gas would *not* tend to reduce the molecules after encounter to the same state as those of a *uniform gas* moving with the *mean component velocities of the varying gas*, but to a *uniform gas* moving with the halves of the mean component velocities *of all the molecules which cross a unit of surface in a unit of time—which pass through an element in a unit of time.*

I had not till then apprehended, nor do I know that it has anywhere been pointed out, that the mean component velocities of the molecules which pass through an element in a given time are not, in the case of a varying gas, the doubles of the component velocities of the gas, as they would be in that of a uniform gas (neglecting the squares of the mean component velocities). But it turns out to be so (see Art. 77). And what is more, these mean component velocities are the very velocities U, V, W, which had been found to be necessary as already described.

The recognition of this fact therefore removed all fundamental difficulty as regarded the velocities U, V, W.

There still, however, remained the question as to whether the molecules might be considered to arrive in all respects, to the same degree of approximation, as from the same uniform gas—whether the molecules would arrive in respect to density from the same uniform gas as in respect to mean velocity, &c.; or whether severally in respect of density, mean velocity, &c.,

the uniform gas would correspond to different values of s? The answer to this question depends on the law of action between the molecules, and hence it is of necessity left for such light as accrues from the experiments and other known properties of gas.

It is, however, now proved (not altogether from first principles, but on certain elementary assumptions which might, it is thought, be deduced from first principles) that as regards number the molecules will arrive at A as from a uniform gas having the density, mean pressure and U, V, W, of the actual gas at a certain distance s from A, and that as regards mean velocity, mean square of velocity, and mean cube of velocity, the molecules will arrive as from uniform gas corresponding in each respect with the same or another point.

So that instead of having one value of s there are four; the numerical relations between which have not been determined from the elementary assumptions, but which are all shown to be functions of the temperature and inversely proportional to the density, and when the gas varies continuously independent of the direction from which the molecules arrive.

On comparing the theoretical results with those of experiment it is found—

1. That the values of s for density and mean square of velocity are equal;

2. That s for the mean cube does not enter into any of the experimental results of this investigation;

3. That s for the mean velocity has a real value, but there are no data for effecting a numerical comparison between this and the other value of s.

As this foundation of the theory on elementary assumptions renders it more satisfactory, it is introduced at length into this section of the paper. The argument, which is long and occupies Arts. (79 to 84), may be sketched as follows :—

Sketch of the method by which the fundamental theorems are deduced.

76. Upon certain elementary assumptions, which do not involve any particular law of action between the molecules, it is first shown that, in respect of density, mean velocity, &c., considered separately, any group of molecules whose directions of approach differ by less than a given small angle from any given direction BA, will enter the element at A (within a sufficient degree of approximation) *as if the gas were uniform* and had the same density and mean pressure as at B, and had mean component velocities which, although not the mean component velocities at B, are equal to one-half the

mean component velocities of all the molecules which enter an indefinitely small element at B in a unit of time. These component velocities, which are written U, V, W, cannot in the first instance be expressed in terms of known quantities, but they are shown to be functions of the position of B in the gas.

The distance AB or s is shown to be a function of the pressure and density of the gas, which function, although not completely expressed, as such an expression would involve the law of action between the molecules, is shown to be approximately independent of the variation of the density and pressure, and hence of the direction of AB.

The relations between ρ, α, U, V, W for a uniform gas may thus be used to express *severally* the density, mean velocity, &c., for each elementary group of molecules arriving at A. And since ρ, α, U, V, W are functions of the position of the point B (if $x\ y\ z$ are the coordinates of A, and $l\ m\ n$ are the direction cosines of AB) they are functions of $x + ls$, $y + ms$, $z + ns$, s having the value for the particular quantity to be represented. Therefore ρ, α, U, V, W for B may, by expansion, be represented by ρ, α, U, V, W for A, and their differential coefficients multiplied by powers of s. Thus the density, mean velocity, &c., of the molecules of each group arriving at A may *severally* be expressed in terms of ρ, α, U, V, W at A, and their differential coefficients multiplied by a particular value of s.

Therefore as the elementary portions of $\sigma(Q)$ for the group can always be expressed in terms of the density, mean velocity, &c., and l, m, n, it can be expressed in terms of ρ, α, U, V, W, for A, their differential coefficients multiplied by certain values of s and l, m, n. And, since all these quantities but l, m, n are independent of the direction of the group, by integrating for all values of l, m, n, $\sigma(Q)$ is found in terms of ρ, α, U, V, W for A, and their differential coefficients multiplied by s.

It also appears that within the limits of the necessary approximation, terms multiplied by U^2, V^2, W^2, or differentials of the second order, may be neglected; so that $\sigma(Q)$ is expressed in terms of ρ, α, U, V, W, and their differential coefficients of the first order multiplied by some one or other of the several values of s.

U, V, W, are then at once found by putting $Q = M$, so that $\sigma_x(M)$, $\sigma_y(M)$, and $\sigma_z(M)$, are respectively $\bar{u}$, $\bar{v}$, and $\bar{w}$, which form the left sides of three equations (48) in which U, V, and W respectively appear on the right side.

It is difficult to give an intelligible sketch of so complicated a series of operations, but what has been stated above may serve to indicate the general scheme of this section.

Mean component velocities of the molecules which pass through an element.

77. It has been already pointed out that when the condition of the gas varies, the mean component velocities of all the molecules which in a unit of time pass through an element are *not*, to the same degree of approximation as they would be if the gas were uniform, the *doubles* of the *mean component velocities of the molecules in the element at the same instant.*

To express this, suppose that the condition of the gas varies only in the direction of x, so that the mean momentum in any direction perpendicular to x carried across all surfaces is zero.

Then taking a rectangular element, so that its edges are parallel to the axes, and its edges parallel to x are indefinitely short compared with its edges perpendicular to x, the only momentum carried through the element will be by molecules entering and leaving the faces perpendicular to x; and, since the condition of the element remains unchanged, the aggregate momentum of the molecules which enter must be equal to the aggregate momentum of the molecules which leave.

The aggregate momentum which enters at the face on the left is $\overset{u+}{\sigma_x}(Mu)$, or as it may be written $\overset{u+}{\Sigma}(Mu^2)$, while the aggregate momentum which enters on the right is $-\overset{u-}{\sigma_x}(Mu)$ or $-\overset{u-}{\Sigma}(Mu^2)$.

Therefore the whole momentum in the direction of x carried through the element in a unit of time is

$$\overset{u+}{\sigma_x}(Mu) - \overset{u-}{\sigma_x}(Mu) \text{ or } \overset{u+}{\Sigma}(Mu^2) - \overset{u-}{\Sigma}(Mu^2).$$

And since the aggregate mass of the molecules which pass through the element in the same time is

$$\overset{u+}{\sigma_x}(M) - \overset{u-}{\sigma_x}(M) \text{ or } \overset{u+}{\Sigma}(Mu) - \overset{u-}{\Sigma}(Mu)$$

the mean component velocity of all the molecules which pass through the element in a unit of time is

$$\frac{\overset{u+}{\sigma_x}(Mu) - \overset{u-}{\sigma_x}(Mu)}{\overset{u+}{\sigma_x}(M) - \overset{u-}{\sigma_x}(M)} \text{ or } \frac{\overset{u+}{\Sigma}(Mu^2) - \overset{u-}{\Sigma}(Mu^2)}{\overset{u}{\Sigma}(Mu) - \overset{u-}{\Sigma}(Mu)},$$

which will not be, neglecting $\bar{u}^2$, the same as $2\bar{u}$, as it would be if the gas were uniform and moving with the velocity $\bar{u}$.

The same thing may be shown for faces parallel to x, and for variations in the directions y and z.

In all the phenomena considered, the velocity

$$\frac{\overset{u+}{\sigma_x}(Mu) - \overset{u-}{\sigma_x}(Mu)}{\overset{u+}{\sigma_x}(M) - \overset{u-}{\sigma_x}(M)} - \bar{u}$$

is very small compared with the mean velocity of a molecule; but the relation is of the same order as that of the unhindered rate of thermal transpiration and the mean velocity of a molecule.

78. The following limitations and definitions will tend to the simplification of subsequent expressions.

The condition of the gas.

All the assumptions and theorems, as indeed the entire investigation, with the exception of Arts. 108 A and 109, relate to a simple gas in which the diameters of the molecules may be neglected in comparison with the mean distance which separates them, the condition of which gas is at all points steady as regards time, and the molecules of which are subjected to no external forces, such as gravity and electric attractions; and the term *gas* is to be understood in this sense unless otherwise defined.

The small quantities neglected.

As a first approximation, *i.e.*, in theorems (I.) and (II.) no account is taken of variations of the second order, such as are expressed by

$$\frac{d^2\rho}{dx^2}, \ \frac{d^2a}{dx^2}, \text{ \&c.}$$

the effects of such variations being too small to make any difference in the results of the first approximation.

Also throughout the investigation the velocity of the gas is assumed to be so small that such quantities as $\bar{u}^2$ and

$$\left(\frac{\overset{u+}{\sigma_x}(Mu) - \overset{u-}{\sigma_x}(Mu)}{\overset{u+}{\sigma_x}(M) - \overset{u-}{\sigma_x}(M)}\right)^2$$

may be neglected.

Definitions.

An elementary group of molecules. In addition to the separation of the molecules into the groups A, B, C, D, E, F, G, H, as explained in Art. 67, a further subdivision is necessary in order to render the reasoning of this section definite.

From any one of the eight groups are selected all the molecules having directions of motion which differ by less than certain small angles from a given direction, or, in other words, those molecules of which the directions of motion are parallel to some line which may be included within a pyramidal surface having indefinitely small angles at the apex. Such a group will be called *an elementary group*, and in this sense only will the term elementary group be used. The mean ray or axis of the pyramid is the mean direction of the group. And it is to be noticed that only those molecules that are moving in the *same direction* parallel to the axis of the pyramid are included in the *same group*, those with *opposite motion* constituting *another* elementary group.

The distinguishing features of an elementary group, apart from the direction of the group, are the number of molecules at any instant in a unit of volume—the symbol N will be used to signify this number; their mean velocity, mean square of velocity, &c., will be indicated without regard to direction by the symbols $\overline{V}$, $\overline{V^2}$; and to avoid confusion, instead of using Q to indicate the two latter quantities the letter G will be used to represent severally N, $\overline{V}$, $\overline{V^2}$, &c.

The resultant uniform gas. It has been already pointed out (Art. 75) that if the encounters within an element of volume resulted in the molecules leaving the element in the same manner as they would leave if the gas about and within the element were uniform, this uniform gas must have component velocities which are one-half the mean component velocities of all the molecules of the varying gas which in a unit of time pass through the element. This uniform gas, which would also have approximately the mean pressure and density of the actual gas in the element, is called *the resultant uniform gas* of the gas within the element. U, V, W are used to designate its component velocities, ρ to express its density, and $\frac{\rho\alpha^2}{2}$ to express its pressure. U, V, W are functions of $\bar{u}$, $\bar{v}$, $\bar{w}$ and of the variations of ρ, α, or, in other words, they are functions of the condition of the gas at the point considered, but they cannot be completely determined in the first stage of the investigation.

The inequalities in elementary groups. All the elementary groups relating to a unit of volume in a varying gas are compared with corresponding elementary groups in the resultant uniform gas for the element, and the differences in respect of the density and velocities of the molecules are spoken of as the *inequalities* of the group. There are only four quantities in respect to which the groups can be compared, namely: the *numbers of molecules*, the *mean velocity*, the *mean square*, and the *mean cube* of the velocity; essentially, therefore, the differences in these constitute the inequalities of the group.

Thus, if G standing for N, $\overline{V}$, $\overline{V}^3$ or $\overline{V}^2$ refers to an elementary group of the resultant uniform gas for an *indefinitely small element*, and $G+I$ refers to the corresponding elementary group of the varying gas, then I represents the inequality in an elementary group at a point as compared with the resultant uniform gas at that point.

When the element has small but definite dimensions (δr) the inequalities of the elementary groups entering or leaving will be

$$\pm \frac{dG}{dr}\frac{\delta r}{2} + I \pm \frac{dI}{dr}\frac{\delta r}{2};$$

for the inequality has reference to the uniform gas at a point distant $\frac{\delta r}{2}$ from the point at which I represents the inequalities, and therefore the change in $G+I$ must be added to or subtracted from the inequality, as the case may be.

79. The following assumptions may all be deduced from first principles, but the necessary reasoning is long, and it is thought that the assumptions are sufficiently obvious.

Assumptions.

I. *That the condition of the gas, as already defined* (Art. 78), *at any instant within an element of volume depends entirely on the numbers and component velocities of the molecules which, in a unit of time, enter at each part of the surface of the element; and hence if the molecules enter one element in exactly the same manner as the molecules enter a geometrically similar element, the condition of the gas within the elements must be similar.*

II. *That the number and component velocities of the molecules which leave each elementary portion of the surface of an element, depend only on the condition of the gas within the element and the manner in which the molecules enter;* and therefore by (I.) depend only on the manner in which molecules enter. Also since the gas immediately outside the element consists of the molecules entering and leaving, its condition depends only on the molecules entering. So that if molecules enter corresponding portions of the surfaces of two geometrically similar elements in exactly the same manner the gas about the elements must be exactly similar.

III. *That whatever be the nature of the action between the molecules, the effect of encounters within an element must always tend to produce or maintain the same relative motion amongst the molecules, which relative motion is that of a uniform gas;* and hence the encounters must render the manner in which the molecules leave the element, as compared with that in which they enter, more nearly similar to the manner of a uniform gas.

That is to say, if A, B, C, D, &c., be a series of geometrically similar spherical elements, and the gas about B is such that the molecules enter B in exactly the same manner as they leave the opposite sides of A, and the gas about C such that the molecules enter as they leave the opposite side of B and so on, the gas about each element being such that the molecules enter the element exactly as they leave the opposite side of the preceding element, then, according to the assumption, the gas about each element will be more nearly uniform than that about the preceding element, so that eventually about the nth element the gas would be uniform, n being indefinitely great.

This may be expressed algebraically. Putting h for the number of encounters necessary to obliterate the inequalities in the groups which pass through A in a unit of time, h will be infinite, and as I is so small that it may be considered as taking no part in the distribution, the rate of distribution will depend on the number of encounters in a unit of volume, and on some function $f(\alpha)$ of α, $\frac{2\alpha}{\sqrt{\pi}}$ being the mean velocity of the molecules.

Therefore approximately

$$\frac{dI}{dh} = -f(\alpha)\, I \quad\text{.............................. (23).}$$

So that if I' is the initial value of I, then after h encounters we have integrating

$$I = I'e^{-f(\alpha)h},$$

and if h is infinite

$$I = O \quad\text{....................................(24).}$$

$f(\alpha)$ is a positive function of α, and is not a function of I; but both as regards form and coefficients $f(\alpha)$ may depend on the nature of the quantity G.

The question whether $f(\alpha)$ is different for any or all of the quantities N, $\overline{V}$, $\overline{V^2}$, &c., must depend on the nature of the action between the molecules during encounters.

If therefore by comparing the mathematical results with those from experiments the several values of $f(\alpha)$ can be compared, a certain amount of light would be thrown on the action between the molecules. So far, however, the conditions of equilibrium in the interior of gas of which the temperature varies, form the only instance in which the values of $f(\alpha)$ are brought into direct comparison. This instance affords means of comparing the values of $f(\alpha)$ for N and $\overline{V^2}$, and shows that these values must be equal. As regards $f(\alpha)$ for $\overline{V}$ or $\overline{V^3}$, there are no experimental results which furnish any further light than that $f(\alpha)$ has real positive values.

These questions do not rise in this investigation, since $f(\alpha)$ for $\overline{V}^2$ does not appear in the results, and should $f(\alpha)$ have a different value for $\overline{V}$ from that which it has for N and $\overline{V}^2$, the only result would be a numerical difference in certain coefficients as to the comparative value of which the experiment affords no approximate evidence.

IV. *That when the molecules which enter or leave an element of volume in a unit of time are considered separately, the proportion of the molecules* ($N\ \overline{V}$) *entering in a unit of time, in each entering group, which will subsequently undergo encounters within the element, and the proportion of the molecules leaving in a unit of time, in each leaving group, which have undergone encounters within the element, are approximately proportional to the mean distance* (δr) *through the element in direction of the group, and to the number of molecules in each unit of volume of the element.*

V. *That the mean effect of encounters in distributing the several inequalities of the molecules which, entering in a unit of time, encounter within the element, is a function* ($f(\alpha)$) *of the mean velocity of the molecules within the element at the instant.*

Fundamental Theorems.

80. On the assumptions I. to V., remembering the fact pointed out in Arts. 75 and 79 with respect to the component velocities of the resultant uniform gas, the following theorems are established :—

Theorem (I.). *Each of the several inequalities, as defined in Art.* 78, *in every elementary group of molecules which in a unit of time leave an element of volume of small but definite size, will severally be less than in the corresponding elementary group, which in the same time enter the element in the same direction, by quantities which bear approximately the same relation to the mean inequalities of the two groups, as the distance through the element in direction of the group bears to a distance* (s) *which is a function of the density of the gas, and the mean square of the velocity of the molecules only.*

To express this theorem algebraically, let G and I, as explained in the last article, refer to the point in the middle of the element. Then the inequality in the entering group is expressed by

$$-\frac{dG}{dr}\frac{\delta r}{2}+I-\frac{dI}{dr}\frac{\delta r}{2},$$

and for the leaving group by

$$\frac{dG}{dr}\frac{\delta r}{2}+I+\frac{dI}{dr}\frac{\delta r}{2}.$$

And what the theorem asserts is

$$\frac{d}{dr}(G+I)\,\delta r = \frac{\delta r}{s} I \quad \text{.........................(25)}$$

wherein s is a function of ρ and α^2 only.

Proof of Theorem (I.).

(*a*) From assumptions I. and II., Art. 79, it follows at once that when the condition of the gas varies from point to point, the molecules cannot enter an element of volume in the same manner as they would from any uniform gas.

(*b*) From (*a*) and assumption III. it follows that the effect of encounters within an element in a varying gas is to render the manner in which the molecules leave as compared with that in which they enter more nearly similar to that of some uniform gas.

(*c*) The uniform gas referred to in (*b*) must, as has been already pointed out, have component velocities equal to half the mean component velocities of all the molecules which in a unit of time pass through the element.

This at once follows from the illustration appended to assumption III., Art. 79. For the molecules which leave an element in a unit of time must have the same mean component velocities as those which enter, their aggregate mass being the same and the momentum within the element remaining unaltered, and as the molecules enter each successive element in the same manner as they left the preceding, the molecules which enter the nth element in a unit of time must have the same mean component velocities as those which enter the first; but in the nth element the gas is uniform. Therefore, if U, V, W are the component velocities of the uniform gas, when these are small so that we may neglect U^2, V^2, W^2

$$U=\tfrac{1}{2}\frac{\overset{u+}{\sigma_x}(Mu)-\overset{u-}{\sigma_x}(Mu)}{\underset{u+}{\sigma_x}(M)-\underset{u-}{\sigma_x}(M)},\quad V=\tfrac{1}{2}\frac{\overset{v+}{\sigma_y}(Mv)-\overset{v-}{\sigma_y}(Mv)}{\underset{v+}{\sigma_y}(M)-\underset{v-}{\sigma_y}(M)},\quad W=\tfrac{1}{2}\frac{\overset{w+}{\sigma_z}(Mw)-\overset{w-}{\sigma_z}(Mw)}{\underset{w+}{\sigma_z}(M)-\underset{w-}{\sigma_z}(M)} \quad \text{......(26).}$$

The number of molecules which enter the nth element will also be equal to the number which enter the 1st.

Therefore putting $\bar{w}^2+\bar{v}^2+\bar{u}^2=\frac{3\alpha^2}{2}$, and using the dash to indicate the first element

$$\rho\alpha=\rho'\alpha' \quad \text{...................................(27).}$$

And the energy carried into the nth is equal to the energy carried into the first element. Therefore

$$\rho\alpha^3 = \rho'\alpha'^3 \quad\text{..............................(28).}$$

From which equations

$$\alpha^2 = \alpha'^2 \text{ and } \rho = \rho' \quad\text{..............................(29).}$$

Or the density and pressure of the uniform gas is approximately the same as the density and mean pressure of the actual gas. This uniform gas is, therefore, the resultant uniform gas according to the definition Art. 78.

(*d*) From assumptions IV. and V. it follows directly that the several changes in the inequalities, considered separately, of each elementary group which enters the element in a unit of time, will be proportional to the mean inequalities of the group as it enters and leaves, multiplied by $f(\alpha)$ and by the product of $\frac{\rho}{M}$ and the mean distance through the element traversed by the group.

Or, as before, putting I for the mean inequality of the group as it enters and leaves in respect of G, the separate inequalities are

$$-\frac{d}{dr}(G+I)\frac{\delta r}{2} + I \text{ and } \frac{d}{dr}(G+I)\frac{\delta r}{2} + I.$$

Whence from assumptions IV. and V. it follows that

$$\frac{d}{dr}(G+I)\,\delta r = f(\alpha)\,\frac{\rho}{M}\,\delta r I \quad\text{.................... (30).}$$

And from the dimensions of this equation it follows that $\frac{M}{f(\alpha)\rho}$ represents a distance. Therefore putting s for this distance

$$\frac{d}{dr}(G+I)\,\delta r = \frac{\delta r}{s} I \quad\text{..........................(31).}$$

[Q. E. D.]

Corollary to Theorem (I.).

When $\frac{dG}{dr}$ is nearly constant, so that we may neglect $s\,\frac{d^2G}{dr^2}$ as compared with $\frac{dG}{dr}$, then integrating equation (31) we have

$$\left.\begin{aligned} I &= s\frac{dG}{dr} + Ce^{-\frac{r}{s}} \\ \text{or} \qquad & \\ \frac{dG}{dr} &= \frac{I}{s} - \frac{C}{s}e^{-\frac{r}{s}} \end{aligned}\right\} \quad\text{..........................(32).}$$

Near a solid surface.

Equation (32) shows the nature of the inequalities as affected by discontinuity such as may arise at a solid surface. The last term on the right gives the effect of discontinuity for an element at a distance r from the surface, r being measured in the direction of the group. This effect diminishes as r increases.

In the first of equations (32) we may obviously put

$$s_1 \frac{dG}{dr} \text{ for } s\frac{dG}{dr} + Ce^{-\frac{r}{s}},$$

s_1 being a function of the position of the element and of the direction of the group.

Theorem (II.). *When the variation in the condition of the gas is approximately constant, then in respect of any one of the quantities* N, $\bar{V}$, $\bar{V}^2$, *&c., each elementary group of molecules entering a small element of volume at any point will enter approximately as if from the resultant uniform gas at a point in the direction from which the group arrives, the distance of which point from the element is a function of the mean velocity of a molecule, and inversely proportional to the number of molecules within a unit of volume, and is independent of the variation of the gas and the direction of the elementary group.*

To illustrate this, supposing a small spherical element at A, and considering the group arriving in the direction BA, then if the gas varies in the

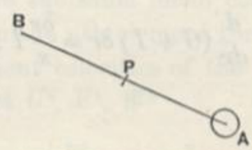


Fig. 10.

direction BA the resultant uniform gas for points along BA will differ, and if A were to be surrounded by a gas identical with the resultant uniform gas at a point P, the elementary group in the direction BA or PA would arrive at the element with different values as to density, mean velocity, &c., from a similar group if the gas were identical with the resultant uniform gas at another point in AB.

Now what the theorem asserts is, that there is some point P_1 at which the *resultant uniform gas* is such that the elementary group in direction BA

would arrive with approximately the same value of N as the actual group, and that the distance P_1A is independent of the direction of BA, *i.e.*, would be the same for all directions from A. In the same way there is some point P_2 at which the resultant uniform gas is such that the group of molecules BA would have the same value of $\bar{v}$ as for the actual group, and so for $\bar{v}^2$ and $\bar{v}^3$.

It is not however asserted that AP_1, AP_2, &c., either are or are not identical.

Proof of Theorem (II.).

This follows directly from theorem (I.).

Taking a series of elements bounded by a cylindrical surface described about the element at A and having its axis in the direction of the group, then all the molecules of the group leaving one element may be supposed to enter the next.

In entering the first element at B there will be a difference I between the value of G for the actual group and the value of G for the resultant uniform gas. If G_B is taken for the resultant uniform gas, $G_B + I_B$ will represent the corresponding value for the actual gas at B.

On emerging from the first element $G + I$ for the group will, by theorem (I.), have been diminished by $\frac{\delta r}{s} I$, δr being the thickness of the element; on emerging from the next element, $G + I$ will be still further diminished by $\frac{\delta r}{s} I$, and so on through all the elements, the total diminution of $G + I$ being equal to

$$\int_0^s \frac{I}{s} dr.$$

And by the corollary to theorem (I.), since the variation in the condition of the gas is approximately constant, I is approximately constant through the distance s, and s will be approximately constant through this distance; therefore

$$\int_0^s \frac{I}{s} dr = I_B \qquad (33).$$

Hence, having traversed the distance s, the group will emerge having

$$\begin{aligned} G + I &= G_B + I_B - I_B \\ &= G_B \qquad (34). \end{aligned}$$

That is, on arriving at A, the molecules will, in respect of G, enter the element as if from the resultant uniform gas at B, a point in the direction of the group, the distance s of which from A is a function of α, is inversely proportional to $\frac{\rho}{M}$, and is independent of the variation of the gas and of the direction of the group. [Q. E. D.]

Corollary to Theorem (II.). The effect of a solid surface.

If in the neighbourhood of A there is a solid surface such that, if B is a point on this surface, BA is of the same order of magnitude as s, then putting $r = BA$ for the group arriving at A from the direction BA, equation (33) gives

$$G_A + I_A = G_B + I_B - \int_0^r \frac{I}{s}\,dr \quad\text{.......................}(35)$$

and substituting for $\frac{I}{s}$ from equation (32) and integrating

$$G_A + I_A = G_B + I_B - r\frac{dG}{dr} - C(e^{-\frac{r}{s}} + 1),$$

or since $G_B = G_A + r\frac{dG}{dr}$, and $I_B - C = s\frac{dG}{dr}$, therefore

$$I_A = s\frac{dG}{dr} - Ce^{-\frac{r}{s}} \quad\text{..............................}(36)$$

C will be a function of l, m, n, and it may be written $f(lmn)\,s\frac{dG}{dr}$; therefore

$$I_A = s\frac{dG}{dr}\{1 - f(lmn)\,e^{-\frac{r}{s}}\} \quad\text{.....................}(37).$$

The mean range.

81. The distance s, or $\frac{M}{f(\alpha)\rho}$ (equation 30) is thus shown to be the distance at which the elementary groups radiating outwards from a point have the mean value of G for the molecules which, in a unit of time, pass the central point. And hence it is proposed to call s the *mean range of the quantity G*.

The mean range is thus seen to be approximately independent of the space variations of the gas, but since s involves $f(\alpha)$, which, as pointed out in assumption III., Art. 78, may, so far as is yet known, have different values for $\overline{V}$ and $\overline{V^3}$ from its values for N and $\overline{V^2}$, which latter are equal, so the values of s for the mean velocity and mean cube of the velocity may be different from the values of s for the density and mean square of the velocity, which latter are equal.

Such a difference in the values of s, however, is not important as regards the immediate results of this investigation, and in the absence of any evidence to the contrary all values of s will be treated as equal.

The mean component values of s and general expression of $\sigma(Q)$. Gas continuous.

82. When the gas is continuous, by theorem (II.) all values of $\sigma(Q)$ for the groups A, B, C, &c., at any point may severally be expressed as functions of ρ, α, U, V, W for this point, their space variations, and s.

The first step is to express as a function of ρ, α, U, V, W, the elementary portion of $\sigma(Q)$ belonging to an elementary group of molecules, and then to integrate for the larger groups.

Putting $\sigma'(Q)$ for the value of $\sigma(Q)$, which would result from the resultant uniform gas at a point, and $\delta\sigma(Q)$ for the elementary portion of $\sigma(Q)$ belonging to an elementary group whose directions are l, m, n, then since ρ, α, U, V, W, for any point x, y, z, are functions of x, y, z, $\delta\sigma(Q)$ is a function of x, y, z, and for the point $x + ls$, $y + ms$, $z + ns$ we have

$$\delta\sigma(Q) = \delta\sigma'(Q) + s\left(l\frac{d}{dx} + m\frac{d}{dy} + n\frac{d}{dz}\right)\delta\sigma'(Q) \quad \text{.........(38)}$$

together with terms which are neglected as small.

Whence integrating for all the groups in A, and putting A for $\sigma'(Q)$

$$\sigma^a(Q) = A + s\int_0^{\pi}\int_0^{\frac{\pi}{2}}\left(l\frac{d}{dx} + m\frac{d}{dy} + n\frac{d}{dz}\right)\delta A \sin\theta d\theta d\phi \quad \text{......(39)}$$

where $\cos\theta = l$, $m = \sin\theta\cos\phi$, $n = \sin\theta\sin\phi$, and similarly for the other seven groups, B, C, &c.

The values of A, &c., are given in Table XX.

The integrals of $s\left\{\left(l\frac{d}{dx} + m\frac{d}{dy} + n\frac{d}{dz}\right)\delta A\right\}\sin\theta d\theta d\phi$ will involve terms which will be the differentials of the corresponding terms in Table XX., multiplied by s and by certain numerical coefficients which are the mean values of l, of l^2 divided by l, and so on, and which may be written $\overline{l}$, $\frac{\overline{l^2}}{\overline{l}}$, $\frac{\overline{lm}}{\overline{l}}$, &c. The values of s multiplied by these coefficients are the mean component values of s for ρ, α, α^2, &c., for the groups A, B, C, &c. As it is these component values which come into comparison with the distances from a solid surface, it is important to preserve the coefficients, therefore instead of using the numerical values they will be indicated by the letters L_1, L_2, &c., as about to be assigned.

Putting i for unity or any power of α or U, V, W, the coefficients by which the differentials of the corresponding terms in Table XX. must be multiplied, are as follows:

$$\left.\begin{array}{llll}
\dfrac{d\overline{\rho i}}{dx}, \text{ \&c.} & \text{by} & \bar{l} = \dfrac{1}{2} & \text{expressed by } L_1, \\[2ex]
\dfrac{d\overline{\rho\xi i}}{dx}, \dfrac{d\overline{\rho\eta i}}{dy}, \dfrac{d\overline{\rho\zeta i}}{dz} & \text{,,} & \dfrac{\overline{l^2}}{\bar{l}} = \dfrac{2}{3} & \text{,,} \quad L_2, \\[2ex]
\dfrac{d\overline{\rho\xi i}}{dy}, \dfrac{d\overline{\rho\xi i}}{dz}, \text{ \&c.} & \text{,,} & \dfrac{\overline{lm}}{\bar{l}} = \dfrac{4}{3\pi} & \text{,,} \quad L_3, \\[2ex]
\dfrac{d\overline{\rho\xi^2 i}}{dx}, \dfrac{d\overline{\rho\eta^2 i}}{dy}, \dfrac{d\overline{\rho\zeta^2 i}}{dz} & \text{,,} & \dfrac{\overline{l^3}}{\overline{l^2}} = \dfrac{3}{4} & \text{,,} \quad L_4, \\[2ex]
\dfrac{d\overline{\rho\xi^2 i}}{dy}, \dfrac{d\overline{\rho\xi^2 i}}{dz}, \text{ \&c.} & \text{,,} & \dfrac{\overline{l^2 m}}{\overline{l^2}} = \dfrac{3}{8} & \text{,,} \quad L_5, \\[2ex]
\dfrac{d\overline{\rho\xi\eta i}}{dx}, \dfrac{d\overline{\rho\xi\zeta i}}{dx}, \text{ \&c.} & \text{,,} & \dfrac{\overline{l^2 m}}{\overline{lm}} = \dfrac{3}{16}\pi & \text{,,} \quad L_6,
\end{array}\right\} \text{.....(40).}$$

The coefficients L_1, L_2, &c., all occur in some one or other of the expressions for A, B, C, &c.; but when these expressions come to be added together it is found that L_2 is the only coefficient of s which has to be considered. This being the case, instead of $L_2 s$, the simple s will be used, so that in all subsequent expressions

$$s = \frac{2}{3}(\text{mean range}) \text{ ....(41).}$$

The signs of the products of s and the differentials of the several terms in the table may, as will be seen from Art. 69, be expressed in the following manner, ignoring the numerical coefficients L_1, L_2, &c.

$$\left.\begin{aligned}
\sigma_x(Q) &= A_x + B_x + C_x + D_x + E_x + F_x + G_x + H_x \\
&\quad - s\frac{d}{dx}(A_x + B_x + C_x + D_x - E_x - F_x - G_x - H_x) \\
&\quad - s\frac{d}{dy}(A_x + B_x - C_x - D_x + E_x + F_x - G_x - H_x) \\
&\quad - s\frac{d}{dz}(A_x - B_x + C_x - D_x + E_x - F_x + G_x - H_x)
\end{aligned}\right\} \text{...(42),}$$

with similar expressions for $\sigma_y(Q)$ and $\sigma_z(Q)$, the suffix to the letters being the same as the suffix to $\sigma(Q)$ on the left.

The following are the resulting values of $\sigma(Q)$ which are required for this investigation, terms of the order U^2 having been neglected.

$$\left.\begin{aligned}\sigma_x(M)&=\rho U-\frac{s}{\sqrt{\pi}}\frac{d\rho\alpha}{dx},\\ \sigma_y(M)&=\rho V-\frac{s}{\sqrt{\pi}}\frac{d\rho\alpha}{dy},\\ \sigma_z(M)&=\rho W-\frac{s}{\sqrt{\pi}}\frac{d\rho\alpha}{dz},\end{aligned}\right\}\dots\dots(43)$$

$$\left.\begin{aligned}\overset{u+}{\sigma_x}(Mu)&=\frac{\rho\alpha^2}{4}+\frac{\rho\alpha U}{\sqrt{\pi}}-\frac{s}{\sqrt{\pi}}\frac{d\rho\alpha U}{dx}-\frac{s}{4}\frac{d\rho\alpha^2}{dx},\\ \overset{u-}{\sigma_x}(Mu)&=\frac{\rho\alpha^2}{4}-\frac{\rho\alpha U}{\sqrt{\pi}}-\frac{s}{\sqrt{\pi}}\frac{d\rho\alpha U}{dx}+\frac{s}{4}\frac{d\rho\alpha^2}{dx},\end{aligned}\right\}\dots\dots(44)$$

$$\left.\begin{aligned}\overset{v+}{\sigma_y}(Mu)&=+\frac{\rho\alpha U}{2\sqrt{\pi}}-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha U}{dy}-\frac{s}{2\pi}\frac{d\rho\alpha^2}{dx}-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha V}{dx},\\ \overset{v-}{\sigma_y}(Mu)&=-\frac{\rho\alpha U}{2\sqrt{\pi}}-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha U}{dy}+\frac{s}{2\pi}\frac{d\rho\alpha^2}{dx}-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha V}{dx},\end{aligned}\right\}\dots(45)$$

$$\left.\begin{aligned}\sigma_x(Mu)&=\frac{\rho\alpha^2}{2}-\frac{2s}{\sqrt{\pi}}\frac{d\rho\alpha U}{dx},\\ \sigma_y(Mu)&=-\frac{s}{\sqrt{\pi}}\frac{d\rho\alpha V}{dx}-\frac{s}{\sqrt{\pi}}\frac{d\rho\alpha U}{dy},\\ \sigma_z(Mu)&=-\frac{s}{\sqrt{\pi}}\frac{d\rho\alpha W}{dx}-\frac{s}{\sqrt{\pi}}\frac{d\rho\alpha U}{dz},\end{aligned}\right\}\dots\dots(46)$$

with corresponding equation for $Q=Mv$, $Q=Mw$,

$$\sigma_x\{M(u^2+v^2+w^2)\}=\frac{5}{2}\rho\alpha^2U-\frac{2s}{\sqrt{\pi}}\frac{d\rho\alpha^3}{dx}\dots\dots(47)$$

and similar equations.

The values of U, V, W.

Hitherto U, V, W have been treated merely as functions of x, y, z. They are, however, completely expressed by equation (43).

For remembering that $\sigma_x(M)$, $\sigma_y(M)$, $\sigma_z(M)$ are respectively equivalent to $\rho\bar{u}$, $\rho\bar{v}$, $\rho\bar{w}$, we have

$$\left.\begin{aligned}U&=\bar{u}+\frac{s}{\rho\sqrt{\pi}}\frac{d\rho\alpha}{dx},\\ V&=\bar{v}+\frac{s}{\rho\sqrt{\pi}}\frac{d\rho\alpha}{dy},\\ W&=\bar{w}+\frac{s}{\rho\sqrt{\pi}}\frac{d\rho\alpha}{dz},\end{aligned}\right\}\dots\dots(48).$$

The neighbourhood of a solid surface.

83. In this case we have by the corollary to theorem (II.)

$$\delta\sigma(Q) = \delta\sigma'(Q) + s\{1 - f(lmn)e^{-\frac{r}{s}}\}\left(l\frac{d}{dx} + m\frac{d}{dy} + n\frac{d}{dz}\right)\delta\sigma'(Q) \ldots (49)$$

or as in equation (39)

$$\overset{a}{\sigma}(Q) = A + s\int_0^{\frac{\pi}{2}}\int_0^{\frac{\pi}{2}}\{1 - f(lmn)e^{-\frac{r}{s}}\}\left(l\frac{d}{dx} + m\frac{d}{dy} + n\frac{d}{dz}\right)\delta A \sin\theta d\theta d\phi \qquad \ldots\ldots\ldots\ldots (50).$$

In this case it is clear that the coefficients which correspond to L_1, L_2, &c., Art. 82, will be functions of the position of the point with respect to the solid surface, and will depend on the value of $f(lmn)$. $f(lmn)$ will obviously depend to some extent on the action between the molecules and the solid surface. It appears, however, that when the solid surface extends in all directions in its own plane to distances which are great as compared with s, and the variation Q is perpendicular to this plane, the result of the integration of equation (50) is the same as that of equation (39). For taking the solid surface parallel to xy

$$\sigma'(Q) = \overset{w+}{\sigma}(Q) + \overset{w-}{\sigma}(Q),$$

and by symmetry, since Q varies only in the direction z, for two opposite groups such as a, b

$$\overset{a}{\sigma}(Q) + \overset{b}{\sigma}(Q) = \overset{a}{\sigma'}(Q) + \overset{b}{\sigma'}(Q)$$
$$= A + B \qquad \ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots (51).$$

Therefore the integral of the last term of equation (50) for A will have the same value but the opposite sign as for B. Hence since the solid surface can only be on one side of the element, say the side $ACGE$, fig. 10, Art. 66, and r/s will be infinite for the group B, or for this group equation 50 is identical with equation 39, therefore for either of the opposite groups the results of the integration of (50) are the same as of (39).

Near the edge of a solid surface, or when Q varies in some direction parallel to the surface, equation (51) no longer holds good, and then the coefficients corresponding to L_1, L_2, &c., will depend on the position of the element with respect to the solid surface and on the action between the molecules and the solid surface.

In dealing with such cases two courses were open—the one was to try and find some form for $f(lmn)$ which would satisfy the equations, the other course, and that which is here adopted, is to introduce arbitrary functions s_1, s_2, in place of s, and subsequently to determine the form of s_1, s_2, so as to satisfy the experimental results.

84. The only case of importance in this investigation is that in which the temperature varies along a solid surface and is constant at right angles.

Taking $z=-c$ as the equation to the solid surface, and supposing the gas uniform in the direction y, and that $\dfrac{d\alpha}{dz}=\dfrac{d\rho}{dz}=0$, then if x, y, z are the coordinates of a point P and z is greater than $-c$ the effect of the solid surface will be to alter the values of s in the terms involving the differentials of ρ and α. Using a suffix to indicate that the values of s for such terms is arbitrary, we may proceed to determine the values of $\sigma(Q)$, as in Art. 82. The important cases are $Q=Mu$ and $Q=M$.

Remembering that s_1 refers to such terms in A, C, E, G, as involve $\dfrac{d\alpha}{dx}$ or $\dfrac{d\rho}{dx}$, and that $W=0$, we have by the method of Art. 82

$$\sigma_z(Mu)=\frac{s-s_1}{2\pi}\frac{d\rho\alpha^2}{dx}-\frac{s}{\sqrt{\pi}}\rho\alpha\frac{dU}{dz}\;\ldots\ldots\ldots\ldots\ldots\ldots\ldots(52)$$

$$\left.\begin{aligned}\overset{w+}{\sigma_x}(M)&=\tfrac{1}{2}\rho U-\frac{s_1}{2\sqrt{\pi}}\frac{d\rho\alpha}{dx}-\frac{s\rho}{2}\frac{dU}{dz}\\ \overset{w-}{\sigma_x}(M)&=\tfrac{1}{2}\rho U-\frac{s}{2\sqrt{\pi}}\frac{d\rho\alpha}{dx}+\frac{s\rho}{2}\frac{dU}{dz}\end{aligned}\right\}\;\ldots\ldots\ldots\ldots\ldots\ldots(53).$$

Further, to adapt these equations to the form required, put $\bar{u}_1$ and $\bar{u}_2$ for the mean velocity of the opposite groups $w+$ and $w-$, so that

$$\overset{w+}{\sigma_x}(M)=\rho\frac{\bar{u}_1}{2},\quad \overset{w-}{\sigma_x}(M)=\rho\frac{\bar{u}_2}{2}.$$

Then since $\bar{u}$ may be taken as constant in the direction of x, we have by corollary to theorem (II.) and equation (51)

$$\rho\frac{\bar{u}_1}{2}-\rho\frac{\bar{u}_2}{2}=-s\rho\frac{d\bar{u}}{dz}\;\ldots\ldots\ldots\ldots\ldots\ldots\ldots\ldots(53\,a).$$

Subtracting equations (53)

$$-s\rho\frac{d\bar{u}}{dz}=\frac{s-s_1}{2\sqrt{\pi}}\frac{d\rho\alpha}{dx}-s\rho\frac{dU}{dz},$$

and substituting for $\dfrac{dU}{dz}$ in equation (52)

$$\sigma_z(Mu)=\frac{s-s_1}{2\pi}\rho\alpha\frac{d\alpha}{dx}-\frac{s}{\sqrt{\pi}}\rho\alpha\frac{d\bar{u}}{dz}\;\ldots\ldots\ldots\ldots\ldots\ldots(54).$$

If the point P lies between two surfaces, then putting s_2 as an arbitrary function we have

$$\sigma_z(Mu) = \frac{s_2 - s_1}{2\pi} \rho\alpha \frac{du}{dx} - \frac{s}{\sqrt{\pi}} \rho\alpha \frac{d\bar{u}}{dz} \text{ ....(54 } a).$$

For further consideration of $s_2 - s_1$ see Art. 96.

Section VIII. The Equations of Steady Motion.

85. If Q is a quantity of such a nature that $\Sigma(Q)$ cannot change on account of any mutual action between the molecules within a unit of volume; and further, if we assume that the molecules within a unit of volume at any instant are not subject to any influence other than those which they exert on one another, then whatever change may take place in an elementary volume must be on account of the excess of Q carried into the unit of volume over and above that which is carried out; and we have

$$\frac{d\Sigma(Q)}{dt} = -\frac{d\sigma_x(Q)}{dx} - \frac{d\sigma_y(Q)}{dy} - \frac{d\sigma_z(Q)}{dz} \text{ ....(55).}$$

$\dfrac{d\Sigma(Q)}{dt}$ is the rate at which $\Sigma(Q)$ is increasing at a point fixed in space. Hence if the condition of the gas is steady

$$\frac{d\Sigma(Q)}{dt} = 0 \text{ ....(56).}$$

Therefore if the condition of the gas is steady, we have

$$\frac{d\sigma_x(Q)}{dx} + \frac{d\sigma_y(Q)}{dy} + \frac{d\sigma_z(Q)}{dz} = 0 \text{ .... (57).}$$

86. If, therefore, we put $Q = M$, equation (57) gives us the condition of steady density.

Whereas if we put successively $Q = Mu$, $Q = Mv$, $Q = Mw$, we have from equation (57) the conditions of steady momentum in the directions of the axes.

And if we put $Q = M(u^2 + v^2 + w^2)$ we have the condition of steady pressure.

The condition that the gas may be subject to no distortion or shear stress.

87. In order that $\sigma_x(Mv)$, $\sigma_x(Mw)$, $\sigma_y(Mw)$, $\sigma_y(Mu)$, $\sigma_z(Mu)$, and $\sigma_z(Mv)$ may respectively be zero for all positions of the axes, we must have

$$\sigma_x(Mu) = \sigma_y(Mv) = \sigma_z(Mw) \text{ ....(58).}$$

Therefore from the first of equations (46) and like equations

$$s\frac{d\rho\alpha U}{dx}=s\frac{d\rho\alpha V}{dy}=s\frac{d\rho\alpha W}{dz} \quad\text{..................... (59).}$$

These are the conditions that there shall be no tangential stress within the gas at a distance from a solid.

Coupled with the conditions for steady density, steady momentum, and steady pressure, these equations are, within the limits of our approximation, equivalent to

$$\frac{d^2\alpha^2}{dx^2}=\frac{d^2\alpha^2}{dy^2}=\frac{d^2\alpha^2}{dz^2}=0 \quad\text{.........................(60)}$$

and

$$\frac{\rho\alpha^2}{2}=p \quad\text{...................................(61)}$$

where p the pressure is constant throughout the gas.

88. The important condition in this investigation is that the tangential force on a solid surface shall be zero.

This condition can only be obtained by the aid of some *assumption* as to the action between the molecules and the surface. An extremely obvious assumption will suffice, viz.: that the tangential force on the surface has the same *direction* as the momentum, parallel to the surface, of all the molecules which reach the surface in a unit of time.

The condition that there shall be no force on the surface is, then, that the momentum parallel to the surface which is carried up to the surface shall be zero.

Thus, if the axial planes be solid surfaces, we have from the values of $\overset{w-}{\sigma_z}(Mu)$, $\overset{w-}{\sigma_z}(Mv)$, &c., equations (45), that

$$U=V=W=0 \quad\text{..............................(62)}$$

at the surface.

If, further, there is no tangential stress within the gas, it appears from equations (59), (60), and (61), that equation (62) must hold throughout the gas.

The condition that there shall be no tangential stress on a particular solid surface, say, the plane of xy, is satisfied if at that surface $\rho\alpha^2$ is constant and

$$U=0,\ V=0 \quad\text{.............................. (63)}$$

and

$$\frac{dU}{dz}=\frac{dV}{dz}=\frac{dW}{dx}=\frac{dW}{dy}=0 \quad\text{......................(64).}$$

This appears at once from the values of $\overset{w-}{\sigma_z}(Mu)$, $\overset{w-}{\sigma_z}(Mv)$ obtained as equations (45).

Section IX. Application to Transpiration through a Tube.

89. It will be sufficient to consider the simplest cases; hence it is supposed that the gas is transpiring through a tube of uniform section, and further that the tube is of unlimited breadth, the surfaces being planes parallel to the plane xy; the axis of x is taken for the axis of the tube, and it is assumed that all perpendicular sections of the tube are surfaces of equal pressure and temperature, the variation of temperature and pressure being in the direction x.

The equations to the surfaces of the tube are taken

$$z = \pm c \quad \text{.................................(65).}$$

90. From equation (57) we have for steady momentum

$$\frac{d}{dx}\{\sigma_x(Mu)\} + \frac{d}{dz}\{\sigma_z(Mu)\} = 0 \quad \text{.................(66),}$$

for steady density

$$\frac{d}{dx}\{\sigma_x(M)\} + \frac{d}{dz}\{\sigma_z(M)\} = 0 \quad \text{.....................(67),}$$

and for steady pressure

$$\frac{d}{dx}\{\sigma_x M(u^2+v^2+w^2)\} + \frac{d}{dz}\{\sigma_z M(u^2+v^2+w^2)\} = 0 \quad \text{.........(68).}$$

Steady pressure not important.

91. In a tube, since heat may be communicated from the surface to the gas, the temperature may be maintained constant; and if the density be steady the pressure will also be steady, hence the condition of steady pressure ceases to be important. The law of variation of temperature is determined by the sides of the tube.

Transpiration when s is small as compared with c.

92. If s is so small that it is unnecessary to consider the layer of gas throughout which the direct influence arising from the discontinuity at the surface extends, substituting in equation (66) from equations (46), and

putting $\dfrac{dp}{dx} = \dfrac{d}{dx}(\sigma_x(Mu))$, which we may do within the limits of our approximation, we have for steady momentum, since $W = 0$ in the tube,

$$\frac{dp}{dx} = \frac{1}{\sqrt{\pi}}\frac{d}{dz}\left(s\frac{d\rho\alpha U}{dz}\right) \text{..........................} (69).$$

And from equations (43) and (67), since ρ and α do not vary across the tube, we have for steady density

$$\frac{1}{\sqrt{\pi}}\frac{d}{dx}\left(s\frac{d\rho\alpha}{dz}\right) = \frac{d}{dx}(\rho U) \text{........................} (70).$$

Since $\rho\bar{u} = \rho U - \dfrac{s}{\sqrt{\pi}}\dfrac{d\rho\alpha}{dx}$ we have from equation (70)

$$\frac{d\rho\bar{u}}{dx} = 0 \text{ ....} (71).$$

And since the action of the tube is symmetrical about the plane xy, we have at this plane

$$\frac{dU}{dz} = 0 \text{ ....} (72).$$

Therefore, integrating between the limits z and 0, we have from equation (69)

$$\frac{dp}{dx}z = \frac{s}{\sqrt{\pi}}\rho\alpha\frac{dU}{dz} \text{ ....} (73).$$

Also, since s is constant across the tube, except within the layer over which the influence of the surface of the tube extends, and which is not taken into account, we have, integrating from z to c, and putting U_c for U at the surface,

$$\frac{1}{2}\frac{dp}{dx}(c^2 - z^2) = \frac{s}{\sqrt{\pi}}\rho\alpha(U_c - U) \text{.....................} (74).$$

From equation (43) we have, since $s\dfrac{d\rho\alpha}{dx}$ does not vary with z,

$$\rho(\bar{u} - \bar{u}_c) = \rho(U - U_c) \text{ ....} (75).$$

Therefore, from equation (74),

$$= -\frac{\sqrt{\pi}}{2s}\frac{1}{\rho\alpha}(c^2 - z^2)\frac{dp}{dx} + \bar{u}_c \text{ ....} (76),$$

or putting

$$\Omega = \frac{\int_0^c \bar{u}dz}{\int_0^c dz} \text{ ....} (77),$$

so that Ω is the mean velocity of the gas along the tube, we have, integrating (76) and putting $p = \frac{\rho\alpha^2}{2}$,

$$\frac{\Omega - \bar{u}_c}{\alpha} = -\frac{\sqrt{\pi}}{6}\frac{c^2}{s}\frac{1}{p}\frac{dp}{dx} \quad \text{.........................(78).}$$

The relation between s and μ.

93. The only respect in which equation (78) differs from the usual equation between the motion of gas and the variation of pressure in a tube is that instead of μ we have

$$\frac{2}{\sqrt{\pi}}\frac{p}{\alpha}s.$$

For, putting

$$\frac{dp}{dx} = \frac{d}{dz}\left(\mu\frac{d\bar{u}}{dz}\right)$$

we have for the usual equation

$$\Omega - \bar{u}_c = -\frac{c^2}{3\mu}\frac{dp}{dx} \quad \text{.............................(79),}$$

and comparing (78) and (79)

$$\mu = \frac{2}{\sqrt{\pi}}\frac{p}{\alpha}s \quad \text{.................................(80).}$$

The difference between equations (78) and (79) is, however, very important. For whereas μ is usually supposed to be constant, *i.e.*, independent of the diameter of the tube, it appears from (78) that such can only be the case so long as c is large as compared with s: s being a distance measured across the tube which by no variation in the condition of the gas can be made larger than the mean diameter of the tube.

This fact that s cannot increase beyond the diameter of the tube at once explains the anomalies (as they appeared to Graham) between the times of transpiration for fine and coarse plugs.

The mean diameter of the interstices of Graham's coarse plugs was so large, that with gas in the condition in which he used it, s was less than this diameter, and not being limited to the diameter of the tube was different for different gases and for different conditions of the same gas; whereas with the fine plugs, s being limited to the diameter of the tube, could no longer vary with the nature of the gas.

The limit to the value of s also indicates, what has been verified by the experiments described in Part I. of this paper, that the results which

Graham obtained with fine plates only, are to be obtained with coarse plates when the condition of the gas is such that s is limited by the diameter of the interstices.

The relation between s and the other properties of the gas.

94. The experiments made by Graham and by Maxwell, in which the distances between the surfaces were such that there was no chance of s being limited by this distance, give consistent results, from which it has been found that

$$\mu \propto \frac{p}{\rho}.*$$

Hence taking $\mu = l\frac{p}{\rho}$ and substituting in equation (80) we have

$$s = \frac{\sqrt{\pi}}{2}\frac{\alpha}{\rho}l \qquad (81).$$

From which it appears that in the same gas

$$s \propto \frac{\alpha}{\rho} \qquad (82)$$

when not limited by the solid objects.

The general case of transpiration.

95. The equation (78) is obtained on the assumption that s is so small compared with the diameter of the tube, that the layer of gas through which the influence of the surface of the tube extends may be neglected, and hence this equation cannot be taken as the law of transpiration when s comes to be limited by the diameter of the tube. And besides this, it is necessary to consider the value of $\bar{u}_c$, which cannot be done without considering the layer of gas throughout which the effect of discontinuity at the surface extends.

In order to take the discontinuity at the surfaces $z = \pm c$ into account, the values of $\sigma_x(M)$ and $\sigma_z(Mu)$ must be taken from equations (53) and (54 a). These values substituted in equations (66) and (67) give equations which correspond to equations (69) and (70), but which involve the quantity $s_1 - s_2$, which quantity it will be well to examine before proceeding to the substitution.

* Added Dec. 1879.—Subsequent observers have found that $\mu \propto \left(\frac{p}{\rho}\right)^{.77}$ so that Maxwell's conclusions are not borne out.—See *Phil. Trans.*, Part I., 1879, p. 240. This makes no difference to the subsequent part of this investigation, as no further use is made of equation (81).

The value of $s_1 - s_2$.

96. Remembering that s_1 and s_2 are taken respectively to represent the mean range of the quantity Q for the groups of molecules which have w respectively positive and negative, and taking s_1', s_2' to represent the values of s_1, s_2 at the surface $z = c$, we may express $s_1 - s_2$ as a function of s, c, and z.

The fact that $s_1 = s_2 = s$ when the point considered is without the range of the influence of the surface, shows that whatever may be the value of $s_1' - s_2'$, $s_1 - s_2$ gradually diminishes as the point considered recedes from the solid surface, until at some distance depending on s at which the mean range is unaffected by the surface $s_1 - s_2 = 0$. It also appears from the fact of the gas being symmetrical about the axis of the tube that $s_1 - s_2$ is zero at the axis, so that even if the value of s_1 is limited by the surface, s_2 approximates to s_1 as the point considered approaches the axis of the tube.

The definite manner in which $s_1 - s_2$ varies across the tube could only be deduced by taking into account the distribution of velocities amongst the molecules; but as $s_1 - s_2$ must change after a continuous manner from one surface to another, we may take for an illustration, or even for an approximation, any law of variation which fits the extremes.

Such a law is given by

$$s_1 - s_2 = (s_1' - s_2') \frac{e^{-\frac{c-z}{a_1 s}} - e^{-\frac{c+z}{a_1 s}}}{1 - e^{-\frac{2c}{a_1 s}}} \quad \text{.....................(83)}$$

in which a_1 is a numerical factor depending only on the nature of the gas.

For the sake of distinctness it will for the present be assumed that $s_1 - s_2$ has the values given by equation (83).

The velocity of the gas at the solid surface.

97. Putting q for the tangential force on the solid surfaces $z = \pm c$, we have

$$q = \sigma_z (Mu) \quad \text{..................................(84),}$$

and by equations (53 a) and (54 a)

$$q = \frac{\rho\alpha}{2\sqrt{\pi}} (\overline{u'_1} - \overline{u'_2}) - \frac{s_1' - s_2'}{2\pi} \rho\alpha \frac{d\alpha}{dx} \quad \text{.................. (85).}$$

Also since $\frac{dp}{dx}$ is constant over the section, we have for the equilibrium of the fluid between two perpendicular sections of the tube at distance dx

$$q = -mc \frac{dp}{dx} \quad \text{..................................(86),}$$

where mc is the hydraulic mean depth of the tube (in the case of a flat tube $m=1$); therefore

$$\frac{\rho\alpha}{2\sqrt{\pi}}(\bar{u}_1' - \bar{u}_2') = \frac{s_1' - s_2'}{2\pi}\,\rho\alpha\frac{d\alpha}{dx} - mc\frac{dp}{dx} \qquad (87).$$

Then if $\bar{u}_e$ is the velocity along the solid surface, we have

$$2\bar{u}_e = \overline{u'_1} + \overline{u'_2} \qquad (88).$$

And since $\bar{u}_2'$ is the mean velocity after encounter at the surface of the tube, we may put

$$\overline{u'_2} = f\overline{u'_1} \qquad (89),$$

where f is a factor depending on the nature of the impact at the surface. Hence

$$2\bar{u}_e = \frac{1+f}{1-f}(\bar{u}_1' - \bar{u}_2') \qquad (90),$$

or putting

$$\lambda = \frac{1+f}{1-f} \qquad (90\,a)$$

$$2\bar{u}_e = \lambda(\bar{u}_1' - \bar{u}_2') \qquad (90\,b).$$

And from equation (87)

$$\frac{\rho\alpha}{\sqrt{\pi}}\bar{u}_e = \lambda\left\{\frac{s_1' - s_2'}{2\pi}\,\rho\alpha\frac{d\alpha}{dx} - mc\frac{dp}{dx}\right\} \qquad (91).$$

The coefficient of friction at the solid surface.

98. Since f, or λ, is important as regards that which is to follow, it is necessary to determine, as far as possible, on what these factors depend. I am not aware that any very definite idea has hitherto been arrived at as to the action between the molecules of a gas and a solid surface over which the gas may be in motion. It appears to have been thought sufficient in most cases to assume that the gas in immediate contact with the surface is at rest, which supposition is equivalent to neglecting any small motion there may be.

We see at once that the gas at the surface must have a velocity when the gas further away is in motion. For by our fundamental assumption the molecules which approach the surface will partake of the motion further away; so that, even supposing the surface to be perfectly rough, the entire group, consisting of the approaching and receding molecules, would have a velocity equal to half that of the approaching molecules.

If the surface be less than perfectly rough, we have, as in equation (89),

$$\overline{u'_2} = f\overline{u'_1}$$

where f^{-1} may be considered to be the coefficient of roughness.

Since we have nothing in nature analogous to perfect roughness, we may assume that f is not zero, and the question arises whether f may not largely depend on the angle at which the molecules approach the plane.

Even if the solid surface were a perfectly even plane, $\frac{1}{f}$ would not be the simple coefficient of friction, but must also be a function of the force with which the molecules strike the surface, and the more nearly perpendicular to the surface was the direction of approach the smaller would be the value of f.

Whereas if, as seems highly probable, the action between the molecules and the surface is closely analogous to that between a ball and an uneven but perfectly smooth elastic surface, then for molecules approaching the surface at very small angles f would be unity, while for those approaching in a manner nearly perpendicular f would be zero, or nearly so.

The variation of f with the angle of approach can be of no particular moment so long as there is a sufficient thickness of gas between the surface considered, and any surface which may be opposite, for in that case the mean angle of approach must be the same, whatever may be the condition of the gas. But when the gas is between two surfaces, as in a tube, and these surfaces are so near that the molecules range across the interval, then the fact, that if small, the angle of reflection (measured from the normal) will always be less than the angle of incidence, must cause the molecules to assume directions more and more nearly perpendicular to the surface as the tube becomes narrower, until some limit is reached.

The case of a billiard ball started obliquely along the table will serve to illustrate this. Each time the ball leaves the side cushions its path will be more nearly perpendicular, and if it could maintain its velocity, and the table was sufficiently long, it would eventually be moving directly across the table. This, however, would not be the final condition if the cushions were zigzag, for then a number of balls, in whatever direction they might be started, would finally attain a certain mean obliquity, depending on the unevenness of the cushions. And it would seem probable that the latter case must be that of the molecules in a tube so narrow that they can range across.

The ability of the molecules to range across the tube will depend on the value of $\frac{c}{s}$; hence it would appear that the most probable assumption with regard to the nature of λ is that

$$\lambda = \lambda_1 f_1\left(\frac{c}{s}\right) + \lambda_2 f_2\left(\frac{c}{s}\right) \quad \text{..........................(91 a)},$$

where $f_1\left(\frac{c}{s}\right)$ and $f_2\left(\frac{c}{s}\right)$ are functions of some such form as $e^{-\left(\frac{c}{s}\right)^2}$, $e^{-\left(\frac{s}{c}\right)^2}$

having respectively the values unity and zero when $\frac{c}{s} = 0$, and zero and unity when $\frac{c}{s} = \infty$; and λ_1 is a coefficient independent of the nature of the gas on which λ_2 may depend.

That there is good reason for making this assumption appears from the comparison of the results for hydrogen and air (see result VIII., Art. 106).

The equations of motion as affected by discontinuity.

99. Substituting in equation (66) from equation (54 a), and putting $\frac{dp}{dx}$ for $\frac{d}{dx}(\sigma_x(Mu))$ as in Art. 92, we have for steady momentum along the tube

$$\frac{dp}{dx} + \frac{d}{dz}\left\{-\frac{s\rho\alpha}{\sqrt{\pi}}\frac{d\bar{u}}{dz} - \frac{s_1 - s_2}{2\pi}\rho\alpha\frac{d\alpha}{dx}\right\} = 0 \text{.............. (92).}$$

Whence integrating between the limits 0 and z

$$z\frac{dp}{dx} = \frac{s\rho\alpha}{\sqrt{\pi}}\frac{d\bar{u}}{dz} + \frac{s_1 - s_2}{2\pi}\rho\alpha\frac{d\alpha}{dx} \text{ .... (93).}$$

And substituting for $s_1 - s_2$ from equation (83)

$$\frac{s\rho\alpha}{\sqrt{\pi}}\frac{d\bar{u}}{dz} = z\frac{dp}{dx} - \frac{e^{-\frac{c-z}{a_1 s}} - e^{-\frac{c+z}{a_1 s}}}{1 - e^{-\frac{2c}{a_1 s}}}(s_1' - s_2')\frac{\rho\alpha}{2\pi}\frac{d\alpha}{dx} \text{ ....(94).}$$

Integrating equation (94) between the limits c and z we have

$$\frac{s\rho\alpha}{\sqrt{\pi}}(\bar{u} - \bar{u}_c) = -\frac{c^2 - z^2}{2}\frac{dp}{dx} + a_1 s\left\{\frac{1 + e^{-\frac{2c}{a_1 s}}}{1 - e^{-\frac{2c}{a_1 s}}} - \frac{e^{-\frac{c-z}{a_1 s}} + e^{-\frac{c+z}{a_1 s}}}{1 - e^{-\frac{2c}{a_1 s}}}\right\}(s_1' - s_2')\frac{\rho\alpha}{2\pi}\frac{d\alpha}{dx} \text{ ....(94 } a\text{).}$$

Integrating again between the limits 0 and c and putting $\Omega = \frac{\int_0^c \bar{u}dz}{c}$, we have, substituting for $\bar{u}_c$ from equation (91),

$$\frac{s\rho\alpha}{\sqrt{\pi}}\Omega = -\left(\frac{c^2}{3} + smc\lambda\right)\frac{dp}{dx} + \left(a_1 s\frac{1 + e^{-\frac{2c}{a_1 s}}}{1 - e^{-\frac{2c}{a_1 s}}} - \frac{a_1^2 s^2}{c} - s\lambda\right)(s_1' - s_2')\frac{\rho\alpha}{2\pi}\frac{d\alpha}{dx} \text{ ....(95).}$$

100. Equation (95) is the equation of transpiration in a flat tube on the assumption that

$$s_1 - s_2 = \frac{e^{-\frac{c-z}{a_1 s}} - e^{-\frac{c+z}{a_1 s}}}{1 - e^{-\frac{2c}{a_1 s}}}(s_1' - s_2').$$

A slight modification however is all that is necessary to render the equation perfectly general.

The only way in which the shape of the tube enters into the equation is in the coefficient of the first line on the right-hand side, *i.e.*, the coefficient of $\frac{dp}{dx}$, and whatever may be the shape of the tube this coefficient will be of the same form as far as the linear dimensions of the tube are involved, the only possible difference being in the numerical coefficients of c^2 and $sc\lambda$. Therefore if c^2 be multiplied by a coefficient A, which depends on the shape of the tube, since m also varies with the shape of the tube, we have for the general coefficient of $\frac{dp}{dx}$

$$sc\left(A\frac{c}{s} + m\lambda\right).$$

As regards the coefficient of $\frac{d\alpha}{dx}$, this is affected by the assumption as to the particular form of $(s_1 - s_2)$; and if we assume a general form for $s_1 - s_2$, such as

$$s_1 - s_2 = \Sigma\left\{\frac{e^{-\left(\frac{c-z}{a_1 s}\right)^n} - e^{-\left(\frac{c+z}{a_1 s}\right)^n}}{1 - e^{-\left(\frac{2c}{a_1 s}\right)^n}}\right\}(s_1' - s_2') \quad \text{............} (96),$$

the coefficient of the last term would still be of the form

$$s\left\{f\left(\frac{c}{s}\right) + \lambda\right\}$$

wherein $f\left(\frac{c}{s}\right)$ varies continuously as $\frac{c}{s}$ varies from 0 to ∞, having a finite value when $\frac{c}{s}$ is infinite and being zero of the order $\frac{c}{s}$ when $\frac{c}{s}$ is zero.

101. The factor $s_1' - s_2'$ is clearly a function of c, $\frac{c}{s}$ and λ_3, where λ_3 depends on the nature of the impacts between the gas and the tube. And, moreover, when $\frac{c}{s}$ is small and the molecules cross the tube without encounter, $s_1' - s_2'$ is proportional to c—it may be shown that in the case of a flat tube

$s_1 - s_2 = \pi mc$, and in the case of a round tube $s_1 - s_2 = \pi mc\left(1+\frac{2}{\pi}\right)$, for tubes of other shapes $s_1 - s_2$ would have an intermediate value—so in this case we put

$$s_1' - s_2' = \pi m'c.$$

Again, where $\frac{c}{s}$ is large, then $s_1' - s_2'$ is equal to $s\lambda_3$.

Hence, as a perfectly general form for $s_1' - s_2'$, we have

$$s_1' - s_2' = \pi m'cf_3\left(\frac{c}{s}\right) + s\lambda_3 f_4\left(\frac{c}{s}\right) \dots\dots\dots\dots\dots\dots (97),$$

wherein $f_3\left(\frac{c}{s}\right)$ is zero when $\frac{c}{s}$ is large, and unity when $\frac{c}{s}$ is small; while $f_4\left(\frac{c}{s}\right)$ is unity when $\frac{c}{s}$ is large, and zero when $\frac{c}{s}$ is small.

The general equation of transpiration.

102. Substituting in equation (95) from equations (96) and (97) we have

$$\left.\begin{aligned} \rho\alpha\Omega = &-\sqrt{\pi}c\left\{A\,\frac{c}{s} + m\lambda\right\}\frac{dp}{dx} \\ &+ \frac{c}{2\sqrt{\pi}}\left\{\pi m' f_3\left(\frac{c}{s}\right) + \frac{s}{c}\lambda_3 f_4\left(\frac{c}{s}\right)\right\}\left\{f\left(\frac{c}{s}\right) + \lambda\right\}\rho\alpha\,\frac{d\alpha}{dx} \end{aligned}\right\} \dots\dots (98).$$

Or since, Art. 72, $p = \frac{\rho\alpha^2}{2}$, $\frac{\tau}{M} = K^2\alpha^2$; and Art. 98, $\lambda = \lambda_1 f_1\left(\frac{c}{s}\right) + \lambda_2 f_2\left(\frac{c}{s}\right)$, we have, remembering that M is constant,

$$\left.\begin{aligned} \frac{\Omega}{\sqrt{\frac{2p}{\rho}}} = &-\frac{\sqrt{\pi}}{2}\,c\left\{A\,\frac{c}{s} + m\lambda_1 f_1\left(\frac{c}{s}\right) + m\lambda_2 f_2\left(\frac{c}{s}\right)\right\}\frac{1}{p}\frac{dp}{dx} \\ &+ \frac{c}{4\sqrt{\pi}}\left\{\pi m' f_3\left(\frac{c}{s}\right) + \frac{s}{c}\lambda_3 f_4\left(\frac{c}{s}\right)\right\}\left\{f\left(\frac{c}{s}\right) + \lambda_1 f_1\left(\frac{c}{s}\right) + \lambda_2 f_2\left(\frac{c}{s}\right)\right\}\frac{1}{\tau}\frac{d\tau}{dx} \end{aligned}\right\} \dots\dots\dots (99)$$

in which

A depends only on the shape of the tube and is $\frac{1}{3}$ for a flat tube,

$f\left(\frac{c}{s}\right)$ is of the order $\frac{c}{s}$ when $\frac{c}{s}$ is zero and is finite when $\frac{c}{s}$ is infinite,

$f_1\left(\frac{c}{s}\right)$ and $f_3\left(\frac{c}{s}\right)$ are unity when $\frac{c}{s}$ is zero and zero when $\frac{c}{s}$ is infinite, and

$f_3\left(\frac{c}{s}\right)$ and $f_4\left(\frac{c}{s}\right)$ are zero when $\frac{c}{s}$ is zero and unity when $\frac{c}{s}$ is infinite; all the functions varying continuously between the limits here ascribed.

Also λ_1 depends on the nature of the surface but not upon the nature of the gas, while λ_2 and λ_3 may depend both upon the gas and the surface.

Putting

$$\left.\begin{aligned} F_1\left(\frac{c}{s}\right) &= \frac{\sqrt{\pi}}{2}\left[A\left(\frac{c}{s}\right) + m\left\{\lambda_1 f_1\left(\frac{c}{s}\right) + \lambda_2 f_2\left(\frac{c}{s}\right)\right\}\right] \\ &\text{and} \\ F_2\left(\frac{c}{s}\right) &= \frac{1}{4\sqrt{\pi}}\left\{\pi m' f_3\left(\frac{c}{s}\right) + \frac{s}{c}\lambda_3 f_4\left(\frac{c}{s}\right)\right\}\left\{f\left(\frac{c}{s}\right) + \lambda_1 f_1\left(\frac{c}{s}\right) + \lambda_2 f_2\left(\frac{c}{s}\right)\right\} \end{aligned}\right\} \text{(100)}$$

we have for the general form of the equation of transpiration

$$\Omega = -c\sqrt{\frac{2p}{\rho}}\left\{F_1\left(\frac{c}{s}\right)\frac{1}{p}\frac{dp}{dx} - F_2\left(\frac{c}{s}\right)\frac{M}{\tau}\frac{d\left(\frac{\tau}{M}\right)}{dx}\right\} \text{.........(101).}$$

SECTION X. VERIFICATION OF THE GENERAL EQUATION OF TRANSPIRATION.

103. In this section the general equation obtained in Section IX. is applied to the particular cases of transpiration which have been the subject of experiments. It will thus appear how I was led to infer the results, and thence to make the experiments.

A summary of the experimental results has already been given in Art. 9, but for the immediate purposes the results may be stated as follows:—

Experimental results.

I. The law of corresponding results at corresponding densities, shown by the fitting of the logarithmic homologues. (See Arts. 28 and 40.)

II. The gradual manner in which the results varied as the density increased, shown by the continuous curvature of the curves which express these results. (See Figures 8, 9, and 12.)

III. The uniformity in direction in which both the *time of transpiration* under pressure (see Tables XIV. to XVII.) and the ratio of the *thermal differences of pressure to the mean pressure* (see Tables III. to XIII.) vary as the density increased.

IV. The fact, sufficiently proved by Graham, that, *cæteris paribus*, the times of transpiration are proportional to the ratio of the differences of pressure to the mean pressure, the difference of pressure being small.

V. The fact, to a certain extent taken for granted, that the ratio of the *thermal differences* of pressure to the mean pressure is, *cæteris paribus*, proportional to the ratio of the difference of temperature to the absolute temperature, this ratio being small.

VI. The continual approximation towards constancy of the time of transpiration under pressure as the density diminished. (See Tables XIV. to XVII., and Figure 12, page 298.)

VII. The relation between the ultimate values of the times of transpiration for different gases (air and hydrogen) for small densities; the times are proportional to the square roots of their atomic weights. (See Art. 42.)

VIII. The fact that the times of transpiration for the same gas in capillary tubes, and at considerable densities, are inversely as the density and independent of the temperature.—Maxwell* and Graham†.

IX. The difference in the variation of the times of transpiration for different gases, shown by the fact that the logarithmic curves for hydrogen cannot be made to fit those for air. (Figures 8, 9 and 12.)

X. The approximation towards a constant value of the ratio which the *thermal differences of pressure* bear to the *mean pressure* as the density diminishes, whatever be the gas or plate; the ratio is that of the difference of the square roots of the absolute temperatures to the square root of the absolute temperature.

XI. The approximation, as the density increases, to a linear relation between the thermal differences of pressure and the reciprocal of the density.

XII. The difference between the law of variation of the thermal differences of pressure for different gases, as shown by the non-agreement of the logarithmic homologues for air and hydrogen. (Figures 8 and 9.)

XIII. The transpiration of a varying mixture of gases through a porous plate.—Investigated by Graham.

104. In order to bring out the agreement of the experimental results with those deduced from the equation, we put

$$\frac{\nabla}{\Omega} \text{ for the time of transpiration,}$$

$b\,\dfrac{dp}{dx}$ for the difference of pressure on the two sides of the plate,

$b\,\dfrac{d\tau}{dx}$ for the difference of temperature on the two sides of the plate.

* *Phil. Trans.*, 1866, pp. 249—268, also note to Art. 94. † *Ib.*, 1849, pp. 349—362.

The suffix s will be used to distinguish quantities relating to the stucco plate, and m to distinguish those relating to meerschaum.

x, y are the coordinates of a point on any one of the curves on Fig. 11, page 297, or Figure 8, page 286 A, which are the logarithmic homologues of the experimental curves.

105. The experimental result I. follows from the general form of equation (101).

For, putting, as in the experiment on transpiration under pressure, $\frac{d\tau}{dx}=0$, and M and $\frac{1}{p}\frac{dp}{dx}$ constant, equation (101) becomes

$$\Omega = -cF_1\left(\frac{c}{s}\right)\sqrt{\frac{\kappa^2\tau}{M}}\,\frac{1}{p}\frac{dp}{dx} \quad\text{.......................(102).}$$

The times of transpiration are proportional to $\frac{1}{\Omega}$ for the same tube or plate, and if ∇ be a factor depending on the number and size of the openings through the plate, we have the time of transpiration equal to $\frac{\nabla}{\Omega}$.

Putting

$$y = \log\frac{\nabla}{\Omega},\quad x = \log\frac{1}{s} \quad\text{.......................... (103),}$$

and indicating the quantities referring to particular plates by s and m, we have

$$\left.\begin{aligned} x_s + \log c_s &= \log\frac{c_s}{s_s} \\ x_m + \log c_m &= \log\frac{c_m}{s_m} \end{aligned}\right\} \quad\text{...................... (104).}$$

Whence taking the coefficients A, m, λ_1, λ_2, to be the same for stucco as for meerschaum (see Appendix, note 4), it follows from equation (102) that when $\frac{c_s}{s_s}=\frac{c_m}{s_m}$

$$\left.\begin{aligned} x_s &= x_m + \log\frac{c_m}{c_s} \\ \text{and}\qquad y_s &= y_m - \log\frac{c_s\nabla_m}{c_m\nabla_s} \end{aligned}\right\} \quad\text{..................... (105).}$$

Hence we see that the curves expressing the relation between the logarithms of the reciprocals of the mean ranges, and the logarithms of the times of transpiration, must have the same shape for different plates, such as stucco and meerschaum. And, moreover, that the difference between the abscissæ of corresponding points for the different plates is the logarithm of the ratio of the coarseness of the plates whatever may be the nature of the gas.

In the experiments we have an exactly similar agreement between the curves expressing the log. of the densities, and the log. of the times.

Hence the only point of difference between the results deduced from the equation, and those derived from the experiments is, that the one depends on $\frac{1}{s}$ and the other upon ρ—the temperature being constant. Whereas it appears not only as in Art. 93, but in whichever way we examine s, that however s may vary with the molecular mass and with the temperature, it must be inversely proportional to the density.

Therefore the fitting of the logarithmic curves is a direct inference from the form of the general equation (101).

We also see that the common difference in the abscissæ of the curves deduced from the equation is the logarithm of the ratio of the diameters of the interstices; and hence we infer that the difference in the abscissæ of the experimental curves for meerschaum and stucco gives the ratio of the mean diameters of the interstices in these plates. (See Appendix, note 4.)

The common difference in the ordinates is, according to the equation, the logarithm of the ratio $\frac{\nabla_m c_s}{c_m \nabla_s}$; and although ∇_m and ∇_s are unknown, the experiments verify the theory in as much as they show that the common difference is independent of the nature of the gas—the same difference being obtained with hydrogen as with air—and depends entirely on the plates.

The fitting of the curves which express the logarithms of the thermal differences of pressure follows in a precisely similar manner from equation (101).

In these experiments $\Omega = 0$ and $\frac{1}{\tau}\frac{d\tau}{dx}$ and M were constant, so that equation (101) becomes

$$F_1\left(\frac{c}{s}\right)\frac{1}{p}\frac{dp}{dx} = F_2\left(\frac{c}{s}\right)\frac{M}{\tau}\frac{d\tau}{dx} \text{ ....} (106).$$

And putting

$$y = \log\frac{dp}{dx},$$

$$x = \log\frac{1}{s},$$

we have as in the previous case, supposing the coefficient in F_1 and F_2 to be the same for stucco as for meerschaum (see Appendix, note 4), where $\frac{c_s}{s_s} = \frac{c_m}{s_m}$

$$\left.\begin{aligned} x_s &= x_m + \log \frac{c_m}{s_s} \\ y_s &= y_m + \log \frac{p_m}{p_s} \end{aligned}\right\} \qquad (107).$$

And since τ and M are the same for both plates

$$\frac{p_m}{p_s} = \frac{\rho_m}{\rho_s}$$

Hence in this case, according to the general equation (106), the common difference in the ordinates of corresponding points is the logarithm of the ratios of corresponding densities, while the difference in the abscissæ is the logarithm of the ratio of the coarseness of the plates, which is the reciprocal of the ratios of the mean ranges. If, therefore, as has just been assumed, the densities are proportional to the mean ranges, the common difference of the ordinates should be the same as that of the abscissæ, and the same for these curves as for those of transpiration under pressure.

Thus we have excellent opportunities of verifying the conclusion that s varies inversely as ρ, and the indication as to the manner in which c enters into the relation between dp and $d\tau$.

This verification is complete, for although there is a slight discrepancy between the common difference for the ordinates and that for the abscissæ, this, as has been explained in Art. 30, was in all probability owing to certain discrepancies in the difference of temperature maintained on the two sides of the plates (see Appendix, note 4). And even if unexplained these discrepancies are small enough to be neglected.

The actual differences are as follows:—

Plates.	Thermal Transpiration. Abscissæ.	Thermal Transpiration. Ordinates.	Transpiration. Abscissæ.
Meerschaum No. 3, and Stucco No. 1	·698	·775	
" " " " 2	·745	·890	·819

Thus the dependence of transpiration on the ratio $\frac{c}{s}$ first revealed by the theory, as expressed in equation (101), has been completely verified by the experiments of transpiration under pressure, and on thermal transpiration. And it must be noticed that while the verification has been obtained both for hydrogen and air, the experiments on either gas suffice for complete verification. And thus the exact agreement of the common differences both of ordinates and abscissæ for the two gases (although the absolute ordinates differ widely, and the shapes of the curves differ considerably) not only affords a double verification, but precludes the possibility of accidental coincidence.

It is further to be noticed, both with respect to the foregoing comparison of the theoretical with the experimental results, and also with respect of such further comparisons as will be made, that the reasoning admits of being reversed; and instead of deducing the experimental results from the equation, it might have been shown that a similar equation is the necessary outcome of the experimental results. Indeed, this has been already done, and it is only out of regard to the length of this paper that I refrain from including the inverse reasoning.

106. The experimental results II. and III. follow at once from the fact that the various functions of $\frac{c}{s}$ in equation (101) increase or decrease continuously between the values $\frac{c}{s}=0$ and $\frac{c}{s}=\infty$.

Results IV. and V. also follow so directly from equation (101) as to require no comment.

Results VI. and VII. refer to transpiration under pressure when $\frac{c}{s}$ is small. Under these circumstances, since $\frac{d\tau}{dx}=0$, equation (99) becomes

$$\frac{\Omega}{\sqrt{\frac{\kappa^2\tau}{M}}}=-\frac{\sqrt{\pi}}{2}\,cm\lambda_1\frac{1}{p}\frac{dp}{dx}\;\text{.....................}\;(108),$$

and taking, as in the experiments, τ and $\frac{1}{p}\frac{dp}{dx}$ constant, we have for the same plate

$$\Omega\propto\frac{m\lambda_1}{\sqrt{M}},$$

which is result VI.

And assuming, as in Art. 98, that $m\lambda_1$ is independent of M or any property of the gas, we have

$$\Omega\propto\frac{1}{\sqrt{M}},$$

and therefore the times of transpiration of the different gases through the same plate are proportional to the square roots of the molecular weights, which is experimental result VII.

This result, therefore, verifies the conclusion arrived at in Art. 98, that when the tube is small compared with s, the effect of the impacts at the surface is independent of the nature of the gas.

Result VIII. relates to transpiration under pressure when $\frac{c}{s}$ is large.

Then we have from equation (99)

$$\frac{\Omega}{\sqrt{\frac{\kappa^2\tau}{M}}} = -\frac{\sqrt{\pi}}{2}c\left(A\frac{c}{s} + m\lambda_2\right)\frac{1}{p}\frac{dp}{dx} \quad \text{............ (109).}$$

Therefore since τ, M, and $\frac{1}{p}\frac{dp}{dx}$ are to be taken as constant; when $\frac{c}{s}$ becomes sufficiently large

$$\Omega \propto \frac{1}{s},$$

that is

$$\Omega \propto \rho;$$

and this is result VIII.

In order to compare different gases we have, when $\frac{c}{s}$ is sufficiently large,

$$\Omega = -\frac{\sqrt{\pi}}{2}\sqrt{\frac{\kappa^2\tau}{M}}A\frac{c^2}{s}\frac{1}{p}\frac{dp}{dx} \quad \text{..................... (110).}$$

Therefore

$$\Omega \propto \frac{1}{s\sqrt{M}}.$$

This gives the relative values of s for different gases; as, for instance, air and hydrogen. Graham found that the times of transpiration of these gases through a capillary tube are in the ratio 2·04. The ratio of the square roots of the molecular weights is 3·8. Hence at equal pressures and equal temperatures the mean range for hydrogen is to the mean range for air as 3·8 is to 2·04.

It appears, however, at once from the equation, that these ratios are not constant unless c/s is very large. As c/s diminishes, the term involving λ_2 becomes important, and it is to this term we must look for the explanation of the result IX.—the marked non-correspondence of the curves for hydrogen and air. If λ_2 depends on the nature of the gas then this difference in shape is accounted for, which confirms the conclusion of Art. 98, that when the tube is large compared with s the effect of the impacts at the surface will probably depend on the nature of the gas.

107. Result X. refers to the thermal differences of pressure when $\frac{c}{s}$ is small.

In this case $\Omega = 0$, while $\frac{1}{\tau}$, $\frac{d\tau}{dx}$, and M are constant.

Equation (99) becomes

$$\frac{1}{p}\frac{dp}{dx} = \frac{1}{2}\frac{m'}{m}\frac{1}{\tau}\frac{d\tau}{dm} = \frac{m'}{m}\frac{1}{\sqrt{\tau}}\frac{d\sqrt{\tau}}{dx} \qquad (111).$$

The exact relation between m and m' would appear, as explained in Art. 101, to depend on the shape of the section of the tube, and to be somewhere between 1 and $1 + \frac{2}{\pi}$, its respective values for a flat and round tube. This view, however, is based on the assumption that the molecules are uniformly distributed as regards direction, whereas it appears probable, from reasoning similar to that of Art. 98, that the molecules tend to assume a direction normal to the surface, and in this case for a tube of curvilinear section the value of $\frac{m'}{m}$ would be reduced.

According to the experiments, it appears that as the density diminishes, $\frac{m'}{m}$ approaches to unity; but owing to the impossibility of measuring the exact difference on the two sides of the plate this determination is not very definite.

Result XI. refers to the thermal difference of pressure when $\frac{c}{s}$ is large.

In this case $\Omega = 0$, while $\frac{1}{\tau}$, $\frac{d\tau}{dx}$, and M are constant.

Equation (99) becomes

$$\left(A\frac{c}{s} + m\lambda_2\right)\frac{1}{p}\frac{dp}{dx} = \frac{1}{2\pi}\frac{s}{c}\left(f\left(\frac{c}{s}\right) + \lambda_2\right)\frac{1}{\tau}\frac{d\tau}{dx} \qquad (112)$$

in which $f\left(\frac{c}{s}\right)$ has some finite value.

In the limit, therefore, we may neglect $m\lambda_2$, and we have

$$A\frac{1}{p}\frac{dp}{dx} = \frac{1}{2\pi}\frac{s^2}{c^2}\left(f\left(\frac{c}{s}\right) + \lambda_2\right)\frac{1}{\tau}\frac{d\tau}{dx} \qquad (113).$$

And since $s \propto \frac{1}{\rho}$ and c is constant

$$\frac{1}{p}\frac{dp}{dx} \propto \frac{1}{\rho^2}\frac{1}{\tau}\frac{d\tau}{dx}$$

which is result XI.

Since the coefficient of $\frac{1}{\tau}\frac{d\tau}{dx}$ in equation (113) involves λ_2, which (Art. 98) depends on the nature of the gas, this equation indicates that different results would be obtained with different gases.

And this appears still more in the case of intermediate pressures when $m\lambda_2$ on the left of equation (112) is important.

These conclusions are according to result XII., which therefore affords additional proof of the correctness of the conclusions in Art. 98 respecting the value of λ_2.

108. I have now shown how I was led to predict the experimental results, and how in every particular the experiments have verified the theory, both as regards transpiration under pressure and the thermal differences of pressure. This concludes the application of the theory to those experimental results of transpiration which were revealed by the theory.

There remains, however, an important class of transpiration phenomena of which, as yet, no mention has been made. These are the phenomena of transpiration when the gas on the two sides of the plate differs in molecular constitution.

Transpiration by a variation in the molecular condition of the gas.

108 A. These phenomena are well known, and were experimentally investigated by Graham, but hitherto, I believe, no complete theoretical explanation of them has been given. The diffusion of one gas into another has been explained by Maxwell; but what has not been explained, so far as I know, is, that there should result a current from the side of the denser to that of the lighter gas. Indeed, from the manner in which these phenomena have been for the most part described, it would appear that the importance of this current has been overlooked; for, owing to the fact that a larger volume of the lighter gas passes, the phenomena are generally described as if the current were from the lighter to the denser gas.

These phenomena of transpiration, like those already considered, may be shown to follow directly from the theory. But as has been already mentioned in Art. 73, in order to completely adapt the equations of transpiration to the case of two or more gases, it would be necessary to commence by considering the case of two or more systems of molecules having different molecular weights, after the manner adopted by Maxwell*. Such an adaptation of the equations is too long to be included in this paper; but it may be seen from

* *Phil. Trans.*, 1867.

the equations, as they have already been deduced, that these particular phenomena would, and in some cases do, follow.

Suppose that the gas on the two sides of the plate is at the same pressure and temperature, but that there is a difference in molecular constitution as air and hydrogen. Thus when the condition has become steady there will be a gradual variation of the molecular condition of the gas through the plate; in this case τ is constant and p is constant, but the mean value of M varies.

If we take M_1 and M_2 (as the molecular masses of the two systems of molecules), and consider a case in which M_1 differs but very slightly from M_2, equation (98) becomes

$$\rho\Omega = -cF_2\left(\frac{c}{s}\right)\kappa\sqrt{\tau}\,\frac{\rho}{(\bar{M})^{\frac{3}{2}}}\,\frac{d\bar{M}}{dx} \quad\text{.......... ....(114)}$$

where $\bar{M}$ is the mean mass of the molecules, or if ρ_1 and ρ_2 are the densities of the two gases

$$\bar{M} = \frac{M_1 M_2}{M_2\rho_1 + M_1\rho_2}(\rho_1 + \rho_2).$$

Whence, putting $N_1 = \dfrac{\rho_1}{M_1}$, $N_2 = \dfrac{\rho_2}{M_2}$,

$$\bar{M} = \frac{N_1 M_1 + N_2 M_2}{N_1 + N_2}.$$

And since the pressure and temperature are constant

$$N_1 + N_2 = N$$

where N is constant throughout the gas.

Therefore

$$\frac{d\bar{M}}{dx} = \frac{M_1 - M_2}{N}\,dN_1 = \frac{d\rho}{N};$$

and (114) becomes

$$\rho\Omega = -cF_2\left(\frac{c}{s}\right)\kappa\sqrt{\tau}\,\frac{1}{\sqrt{\bar{M}}}\,\frac{d\rho}{dx} \quad\text{....................(115).}$$

If $\dfrac{c}{s}$ is small, then $F_2\left(\dfrac{c}{s}\right) = \dfrac{1}{4}m'\lambda_1$.

Hence in this case

$$\rho\Omega = -\frac{c}{4}\,m'\lambda_1\kappa\sqrt{\tau}\,\frac{1}{\sqrt{\bar{M}}}\,\frac{d\rho}{dx} \quad\text{....................(116).}$$

And this is in exact accordance with Graham's law, which is that the rate of transpiration is proportional to the difference in the square roots of the densities of the gas. For

$$\frac{d\rho}{dx} = (M_1 - M_2)\frac{dN_1}{dx},$$

and since $M_1 - M_2$ is small

$$\frac{d\rho}{dx} = \sqrt{M}\,\{\sqrt{M_1} - \sqrt{M_2}\}\frac{dN_1}{dx},$$

or

$$\rho\Omega = -\frac{c}{2}m'\lambda_1\kappa\sqrt{\tau}\,\{\sqrt{M_1} - \sqrt{M_2}\}\,\frac{dN_1}{dx}\dots\dots\dots\dots(117).$$

This form of equation is obtained by neglecting the difference of M_1 and M_2; but by taking into account the two systems of molecules throughout the investigation, an equation similar to (117) would have been obtained without any such assumption.

Thus we see that the general equation of transpiration may be made to include not only the cases of transpiration under pressure and thermal transpiration, but also the well-known phenomena of transpiration caused by the difference in the molecular constitution of the gas. And in this case, as in that of transpiration under pressure, the equation reveals laws connecting the results obtained with plates of different coarseness and different densities of gas, which doubtless admit of experimental verification.

This completes the explanation of the phenomena of transpiration through porous plates.

Section XI. Application to Apertures in Thin Plates and Impulsion. Condition of the Gas.

109. When the gas within a vessel is in a uniform condition, excepting in so far as it is disturbed by a steady flow of gas or of heat, from what, compared with the size of the vessel, may be considered as a small space, such as a small aperture in the side of the vessel or a small hot body within the vessel, the effect of such steady flow will be to cause a varying condition throughout the gas. The lines of flow, whether of heat or of gas, will diverge from the exceptional space, and the surfaces of equal pressure and temperature will be everywhere perpendicular to the lines of flow of matter and heat respectively. Except in the case of absolute symmetry, the lines of flow will not be straight, nor will the directions of the lines of flow in the immediate region of any point focus in a point.

But in the immediate neighbourhood of any point P, the direction of the lines of flow must be such that the directions of the lines of flow parallel to some plane, xy, will converge to some point C, while the directions of the lines of flow parallel to the perpendicular plane xz will converge to some point C' in CP, which it will be seen is taken parallel to the axis of x.

Whence putting $PC = r_y$ and $PC' = r_z$, the surface of equal pressure or temperature at P will be a surface perpendicular to x, and having r_y and r_z as its principal radii of curvature in the planes of xy and xz respectively. The simplest cases are those in which either the two radii are equal or one is infinite, and these are the cases which will for the most part be considered.

It will at once be seen that at any point within gas in the condition just described, ρ, α, $\bar{u}^2 + \bar{v}^2 + \bar{w}^2$ and $U^2 + V^2 + W^2$ are functions of r_y, r_z.

Also, remembering that the axis of x is taken in the direction of the lines of flow at P, the point considered, we see that at P, V, W, $\bar{v}$, $\bar{w}$, are severally zero, as are also $\frac{dU}{dy}$, $\frac{dU}{dz}$, $\frac{d^2V}{dy^2}$, $\frac{d^2W}{dz^2}$, $\frac{dV}{dx}$, $\frac{dW}{dx}$; while we have

$$\left.\begin{aligned}
\frac{d^2U}{dy^2} &= \frac{1}{r_y}\frac{dU}{dx} - \frac{1}{r_y^2}U,\\
\frac{d^2U}{dz^2} &= \frac{1}{r_z}\frac{dU}{dx} - \frac{1}{r_z^2}U,\\
\frac{dV}{dy} &= \frac{U}{r_y},\\
\frac{dW}{dz} &= \frac{U}{r_z},\\
\frac{d^2V}{dxdy} &= \frac{1}{r_y}\frac{dU}{dx} - \frac{1}{r_y^2}U,\\
\frac{d^2W}{dxdz} &= \frac{1}{r_z}\frac{dU}{dx} - \frac{1}{r_z^2}U.
\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (118).$$

Also putting $f(\rho\alpha)$ for any function of ρ and α, $\frac{d}{dy}f(\rho\alpha)$ and $\frac{d}{dz}f(\rho\alpha)$ are zero, while

$$\left.\begin{aligned}
\frac{d^2f(\rho\alpha)}{dy^2} &= \frac{1}{r_y}\frac{df(\rho\alpha)}{dx},\\
\frac{d^2f(\rho\alpha)}{dz^2} &= \frac{1}{r_z}\frac{df(\rho\alpha)}{dx},
\end{aligned}\right\} \quad \ldots\ldots\ldots\ldots\ldots\ldots\ldots (119).$$

The equations of steady condition.

110. Equations (118) and (119), together with equations (43) to (47), enable us to obtain from equation (57) the equations of steady condition.

For steady density

$$\frac{d}{dx}\left\{r_y r_z\left(\rho U - \frac{s}{\sqrt{\pi}}\frac{d\rho\alpha}{dx}\right)\right\} = 0 \ldots\ldots\ldots\ldots\ldots\ldots (120).$$

For steady momentum putting, as before, $\frac{dp}{dx} = \frac{d}{dx}(\sigma_x(Mu))$

$$\frac{dp}{dx} - \frac{2s}{\sqrt{\pi}}\frac{d}{dx}\left\{\rho\alpha U\left(\frac{1}{r_y} + \frac{1}{r_z}\right)\right\} = 0 \ldots\ldots\ldots\ldots\ldots\ldots (121).$$

For steady pressure

$$\frac{d}{dx}\left\{r_y r_z\left(\frac{5}{2}\rho\alpha^2 U - \frac{2}{\sqrt{\pi}} s \frac{d\rho\alpha^3}{dx}\right)\right\} = 0 \ldots\ldots\ldots\ldots (122).$$

These equations (120), (121), (122), might be treated in a manner similar to that in which the corresponding equations for the case of the tube were treated in Section IX., but for various reasons another method commends itself.

In the first place we cannot in this case ignore the condition of steady pressure, for there can be no lateral adjustment of temperature as in the case of the tube. (See Art. 91.) The physical meaning of this is, that in this case the condition of the gas cannot be supposed to vary uniformly even along the lines of flow. It must vary after a fixed law, and this fact restricts the conditions under which the equations can be considered to hold to points where $\frac{s}{r}$ is so small that $\left(\frac{s}{r}\right)^2$ may be neglected. So that any general result obtained from these equations would only apply to points at considerable distances from the foci C and C'.

Again, these equations as they stand include the case in which the flow of the gas may be caused by a considerable difference of pressure, as, for example, transpiration through a small aperture under pressure, whereas if we exclude this case we may, by neglecting such terms as $\frac{s^2}{\alpha^2}\left|\frac{d\alpha}{dx}\right|^2$, very greatly simplify the equations without affecting their application to the cases which it is our principal object to explain.

These two cases are as follow—

1. The flow of gas through a small orifice in a thin plate when the mean pressure of the gas is the same on both sides of the plate, the flow being caused by a difference in temperature on the two sides of the plate, or a difference in the molecular condition of the gas.

2. The excess of pressure which the gas exerts on a small body when the body has a higher temperature than the gas.

Thermal transpiration through an aperture in a thin plate.

111. In this case, since there is no tangential stress, we have (Art. 87)

$$U=0.$$

Whence by equation (121)

$$\frac{dp}{dx}=0 \qquad\qquad (122\,a).$$

Since $p=\rho\frac{\alpha^2}{2}$ we have, integrating equations (120) and (122), respectively

$$\left.\begin{aligned} r_y r_z\frac{d\alpha}{dx} &= \frac{\sqrt{\pi}}{2sp}\alpha^2 G,\\ r_y r_z\frac{d\alpha}{dx} &= -\frac{\sqrt{\pi}}{4sp}H. \end{aligned}\right\} \qquad\qquad (122\,b).$$

G and H are constants, such that $\frac{\beta H}{2r_y r_z}$ is the rate at which heat is carried across a unit of area, and $\frac{G}{r_y r_z}$ is the rate at which matter is carried across.

From equation (122 *b*) we have

$$H=-2\alpha^2 G \qquad\qquad (123).$$

Equation (123) can only be approximately true as α^2 is not constant; therefore the condition $U=0$ is not possible, *i.e.*, it is only approximately fulfilled, whence it follows that p is only approximately constant. The closeness of these approximations will depend on the variation of α^2, and within the limits of our approximation we may consider the condition to hold.

From equation (123) we see that the direction of flow of gas is opposite to that of the flow of heat, while since $\alpha^2\propto\frac{\tau}{M}$, the rate of flow of gas is proportional to the flow of heat, to the mass of a molecule, and inversely proportional to τ, the absolute temperature.

By equation (48) we have

$$\rho\bar{u}=-\frac{s}{\sqrt{\pi}}\frac{d\rho\alpha}{dx},$$

or since $\rho\alpha^2$ is constant

$$\bar{u}=\frac{s}{\sqrt{\pi}}\frac{d\alpha}{dx} \qquad\qquad (124)$$

which it may be noticed is of the same form as results from the equation of transpiration through a tube when p is constant.

Thermal Impulsion.

112. In this case there is no motion, therefore

$$\bar{u} = 0,$$

whence from equations (48)

$$\rho U = \frac{s}{\sqrt{\pi}} \frac{d\rho\alpha}{dx} \quad\text{.............................(125).}$$

This satisfies equation (120).

Substituting from equation (125) in equations (121) and (122), and remembering that $p_x = \frac{\rho\alpha^2}{2} - \frac{2s}{\sqrt{\pi}} \frac{d}{dx}(\rho\alpha U)$, Art. 82, we find that these equations lead to the same result if $\rho\alpha^2$ is constant.

Putting $p_1 = \frac{\rho\alpha^2}{2}$ we have from equation (122)

$$\frac{d}{dx}\left(r_y r_z s p_1 \frac{d\alpha}{dx}\right) = 0,$$

whence integrating

$$\frac{d\alpha}{dx} = -\frac{\sqrt{\pi} H}{9 r_y r_z} \frac{1}{s p_1} \quad\text{..........................(126)}$$

where H has the same value as in Art. 111.

Remembering that $\frac{dr_y}{dx} = \frac{dr_z}{dx} = 1$, also considering s constant, we have, differentiating,

$$\frac{d^2\alpha}{dx^2} = \frac{\sqrt{\pi}}{9} \frac{H}{s p_1} \frac{1}{r_y r_z}\left(\frac{1}{r_y} + \frac{1}{r_z}\right)$$

$$= -\frac{r_y + r_z}{r_y r_z} \frac{d\alpha}{dx} \quad\text{..............................(127).}$$

Also putting $r_y = r_z$, $\alpha = \alpha'$ when $r = \infty$, and $\alpha = \alpha_c$ when $r = c$, and integrating equation (126), we have

$$\left.\begin{aligned} \alpha - \alpha' &= \frac{\sqrt{\pi}}{9} \frac{H}{s p_1} \frac{1}{r}, \\ &= \frac{c}{r} \cdot (\alpha_c - \alpha'), \\ &= \frac{1}{2} r^2 \frac{d^2\alpha}{dx^2}. \end{aligned}\right\} \quad\text{.......................(128).}$$

In a similar way we obtain from equation (121)

$$\frac{dp_x}{dx} = -\frac{4s^2}{\pi} p_1 \frac{d}{dx} \left\{ \left(\frac{1}{r_y} + \frac{1}{r_z} \right) \frac{1}{\alpha} \frac{d\alpha}{dx} \right\} \qquad (129)$$

whence integrating

$$\frac{p_x - p_0}{p_1} = -\frac{4s^2}{\pi} \left\{ \frac{1}{r_y} + \frac{1}{r_z} \right\} \frac{1}{\alpha} \frac{d\alpha}{dx} - C_2 \qquad (130)$$

and from (127)

$$\frac{p_x - p_0}{p_1} = \frac{4s^2}{\pi} \frac{1}{\alpha} \frac{d^2\alpha}{dx^2} - C_2.$$

If r be infinite $p_x = p_0 = p_1$ and $\frac{d^2\alpha}{dx^2} = 0$. Therefore $C_2 = 0$ and

$$\frac{p_x - p_0}{p_1} = \frac{4}{\pi} s^2 \frac{1}{\alpha} \frac{d^2\alpha}{dx^2} \qquad (131)$$

which result may be obtained directly from the value of p_x, Art. 82.

From equation (128)

$$\left. \begin{aligned} \frac{p_x - p_0}{p_1} &= \frac{8}{\pi} \frac{s^2}{r^2} \frac{\alpha - \alpha'}{\alpha} \\ &= \frac{8cs^2}{\pi r^3} \frac{\alpha_c - \alpha'}{\alpha} \end{aligned} \right\} \qquad (132).$$

Putting $\frac{\tau}{M} = \kappa^2\alpha^2$ and neglecting $\frac{1}{\alpha} \left|\frac{d\alpha}{dx}\right|^2$ as compared with $\frac{d^2\alpha}{dx^2}$

$$\frac{p_x - p_1}{p_1} = \frac{2}{\pi} s^2 \frac{M}{\tau} \frac{d^2}{dx^2} \left(\frac{\tau}{M} \right)^* \qquad (133)$$

* From an abstract of a paper read before the Royal Society by Professor Maxwell, in April, 1878 (see *Nature*, May 9, 1878), I see that Professor Maxwell has obtained an expression for this inequality of pressure or "stress" arising from the inequality of temperature. The result given by Professor Maxwell is

$$p_x - p_1 = \frac{3\mu^2}{\rho\theta} \frac{d^2\theta}{dx^2}$$

where μ is the coefficient of viscosity, θ the absolute temperature, and x any one of the three directions x, y, z. This result, when transformed to the present notation, becomes

$$p_x - p_1 = \frac{3\mu^2}{\rho\tau} \frac{d^2\tau}{dx^2}.$$

And if we put, as in equation (80),

$$\mu = \frac{2}{\sqrt{\pi}} \frac{p_1 s}{a}$$

we have

$$\frac{p_x - p_1}{p_1} = \frac{6}{\pi} s^2 \frac{1}{\tau} \frac{d^2\tau}{dx^2}.$$

It is thus seen that the two results are identical in form, but that Professor Maxwell makes the pressure just three times as great as that given by equation (133).

In the abstract published in *Nature*, Maxwell has not given the details of the method by which he arrived at his result.

$$\left.\begin{aligned} &= \frac{8}{\pi}\,\frac{s^2}{r^2}\,\frac{\sqrt{\frac{\tau}{M}} - \sqrt{\frac{\tau'}{M}}}{\sqrt{\frac{\tau'}{M}}} \\ &= \frac{8}{\pi}\,\frac{cs^2}{r^3}\,\frac{\sqrt{\frac{\tau_c}{M}} - \sqrt{\frac{\tau'}{M}}}{\sqrt{\frac{\tau'}{M}}} \end{aligned}\right\} \quad \text{.................. (134).}$$

From equation (127) we have

$$\frac{p_x - p_1}{p_1} = \frac{8}{9}\,\frac{1}{\sqrt{\pi}}\,\frac{Hs}{p\alpha}\,\frac{r_y + r_z}{r_y^2 r_z^2} \quad \text{.................. (135)}$$

where, as before, $\frac{\beta H}{2 r_y r_z}$ is the quantity of heat carried across a unit of surface.

At points near to the surface.

113. In equations (131), (132), and (135) no account has been taken of the discontinuity in the immediate neighbourhood of the surface; hence the results obtained from these equations may not hold good within the layer of gas of thickness s, which is adjacent to the surface.

In order to take this discontinuity into account, the equations of steady conditions should be modified in the manner described in Art. 84, but for this particular case the same thing may be accomplished in a somewhat simpler manner.

Suppose the solid surface to be either spherical or cylindrical at the point considered, and put c_1 for the radius. Then it is obvious that when $\frac{c_1}{s}$ is very large the pressure on the surface will be but slightly affected by the layer immediately adjacent to the surface, *i.e.*, putting p_{c_1} for the pressure at the surface, and p_{c_1+s} for the pressure at a distance s from the surface, $1 - \frac{p_{c_1} - p_1}{p_{c_1+s} - p_1}$ is small when $\frac{c_1}{s}$ is large.

When, however, the gas surrounding the surface is limited by another surface, (which for simplicity may be taken concentric and of radius c_2), then in order that $1 - \frac{p_{c_1} - p_{c_2}}{p_{c_1+s} - p_{c_2-s}}$ may be small, we must have $\frac{c_1 - c_2}{s}$ large as well as $\frac{c_1}{s}$.

Our equations, therefore, may be seen to hold good when the radius of the solid surface is large compared with s, and the distance between the opposite surfaces is also large.

On the other hand, in the limit, when either c_1/s or $(c_1 - c_2)/s$ are very small, $p_c - p_1$ and $p_{c_1} - p_c$ will depend entirely on the action of the gas within the layer of thickness s immediately adjacent to the surface. In these cases, however, *when* c_1/s *or* $(c_1 - c_2)/s$ *are small*, the action within this layer may be easily expressed.

114. Let the temperature of the internal surface (sphere or cylinder) be such that the mean value of α for the molecules which rebound from this surface (considered as a group in a uniform gas) is α_{c_1}; while the temperature of the external surface is such that the mean value of α for the molecules which rebound is α'.

The condition that c_1/s or $(c_1 - c_2)/s$ are small, necessitates that the molecules which come up to the inner surface arrive as from a uniform gas such that $\alpha = \alpha'$. That is to say, none of the molecules which rebound from the inner surface can return until their characteristics have been completely modified by the external surface. For if $(c_1 - c_2)/s$ is small, the molecules will cross the interval between the surfaces without encounter, while if c_1/s is small, although $(c_1 - c_2)/s$ may be large, the characteristics of the gas will be but slightly affected by the internal layer at a distance s from that surface, and, by theorem II., the approaching molecules will arrive as from a uniform gas in the mean condition of the gas at a distance s.

I shall first consider the case in which $(c_1 - c_2)/s$ is small.

The number of molecules which arrive at the inner surface is proportional to $\rho'\alpha'$, and the number which rebound is proportional to $\rho_c\alpha_c$, and since the numbers must be the same we have

$$\rho'\alpha' = \rho_c\alpha_c.$$

The momentum imparted to the surface by the incident molecules is $\frac{1}{2}\frac{\rho'\alpha'^2}{2}$,

and that imparted by the rebounding molecules is $\frac{1}{2}\frac{\rho_c\alpha_c^2}{2}$, therefore

$$p_{c_1} = \frac{1}{4}\rho'\alpha'(\alpha_{c_1} + \alpha') \quad\text{..........................(136).}$$

Since the molecules which rebound from the internal surface all proceed to the external surface, and the surfaces are concentric, we have

$$p' = \frac{\rho'\alpha'^2}{2} + \frac{c_1^2}{c_2^2}\frac{\rho'\alpha'}{4}(\alpha_{c_1} - \alpha') \quad\text{....................(137).}$$

Therefore

$$p_e - p' = \frac{\rho'\alpha'}{4}\frac{c_2^2 - c_1^2}{c_2^2}(\alpha_e - \alpha'),$$

or

$$\frac{p_e - p'}{p} = \frac{1}{2}\frac{c_2^2 - c_1^2}{c_2^2}\frac{\alpha_e - \alpha'}{\alpha'} \quad \text{.....................} \quad (138).$$

Equation (138) holds whatever may be the value of c_1/s provided $(c_2 - c_1)/s$ is small, and it also holds when $(c_2 - c_1)/s$ is large, provided c_1/s is small. When c_1/s is small and $(c_2 - c_1)/s$ is large c_1 may be neglected in comparison with c_2, and we have

$$\frac{p_e - p'}{p} = \frac{1}{2}\frac{\alpha_e - \alpha'}{\alpha'} \quad \text{...............................} (139).$$

Equation (139) is almost identical with what equation (132) becomes as s approaches in value to r. If $s = r$, then the only difference in those two equations is in the coefficient. In comparing these equations, however, it must be noticed that in (132) α is not the same as α_e, for α_{e_1} only refers to the one set of molecules—those which are receding from the surface, whereas α refers to both sets.

At the surface when either c_1/s or $(c_2 - c_1)/s$ are small

$$\alpha = \frac{\alpha_{e_1} + \alpha'}{2}.$$

Whence making this substitution in equation (132), and putting $s = r$ the coefficient differs from that in equation (139) by $8/\pi$, which shows the extent to which discontinuity at the surface affects the result.

General equation of impulsion.

115. From equations (132) and (139) we may form an equation which will hold for all values of c/s.

For if the surfaces are spherical

$$\frac{p_e - p'}{p'} = \left\{\frac{1}{2}\frac{c_2^2 - c_1^2}{c_2^2} f_5\left(\frac{c_1}{s}, \frac{c_2}{s}\right) + \frac{8}{\pi}\frac{s^2}{c_1^2} f_6\left(\frac{c_1}{s}, \frac{c_2}{s}\right)\right\} \frac{\sqrt{\frac{\tau_e}{M}} - \sqrt{\frac{\tau'}{M}}}{\sqrt{\frac{\tau'}{M}}} \quad \ldots (140).$$

And for cylindrical surfaces

$$\frac{p_e - p'}{p'} = \left\{\frac{1}{2}\frac{c_2 - c_1}{c_2} f_5\left(\frac{c_1}{s}, \frac{c_2}{s}\right) + \frac{4}{\pi}\frac{s^2}{c_1^2}\frac{1}{\log_e c_1} f_6\left(\frac{c_1}{s}, \frac{c_2}{s}\right)\right\} \frac{\sqrt{\frac{\tau_e}{M}} - \sqrt{\frac{\tau'}{M}}}{\sqrt{\frac{\tau'}{M}}} \quad (141)$$

where $f_5\left(\frac{c_1}{s}, \frac{c_2}{s}\right)$ and $f_6\left(\frac{c_1}{s}, \frac{c_2}{s}\right)$ are respectively unity and zero when either $\frac{c_1}{s}$ or $(c_2 - c_1)/s$ are zero, and respectively zero and unity when both c_1/s and $(c_2 - c_1)/s$ are infinite.

Equations (140) and (141) have been obtained on the assumption that the solid surfaces are either concentric spheres or concentric cylinders. But these equations indicate what would be the difference of pressure consequent on a difference of temperature whatever may be the shape of the surfaces, and particularly so when c_1/s and $(c_2 - c_1)/s$ are finite, which are the most important cases.

SECTION XII. APPLICATION TO THE EXPERIMENTS WITH THE FIBRE OF SILK AND THE RADIOMETER.

116. Comparing the equations (140) and (141) with the equation of transpiration (101), it appears at once that when Ω is zero these equations are identical in form. Hence the curves expressing the relation between the impulsive forces and the density of the gas under any given conditions, would be of the same character as those expressing the relation between the inequalities of pressure and density in the case of thermal transpiration through a particular porous plate, and it is not necessary for me again to examine this relation.

Besides which, the experiments on impulsion, elaborate as they have been, furnish nothing like the definite results which I have obtained in the experiments on thermal transpiration.

117. The principal results to be deduced from experiments other than those which are contained in this paper, are:

(1) That the force and motion are proportional to the difference of temperature, which results are seen to follow directly from equations (124) and (140).

(2) That with a particular instrument the forces increase with the rarefaction up to a certain point, after which they fall off; this result also follows directly from the equation (140).

118. Equations (124) and (140) first revealed to me the fact that the pressure of gas at which the force would become appreciable must vary inversely as the size of the surface.

From equation (140) it appears that up to a certain point

$$p_c - p' \propto \frac{ps^2}{c_1^2},$$

and since $s \propto \frac{1}{\rho}$ and $p \propto \rho$ it appears that

$$p_c - p' \propto \frac{1}{c_1^2 \rho}.$$

So that with gas at a given density the smaller the surface the greater would be the intensity of the impulsive force; and hence I was led to try the fibre of silk, with which I obtained evidence of the force at densities of half an atmosphere; whereas in the radiometer, with vanes something like 500 times as broad as the fibre of silk, the force does not manifest itself until the density is very small indeed.

Earlier conclusions.

119. The equations (124) and (140) show that both the forces and the consequent motion are, *cæteris paribus,* proportional to the heat communicated from the surface to the gas; for by equation (128) $\alpha_c - \alpha' \propto H$ where H is proportional to the heat communicated from the surface to the gas.

The necessity of such a relation was the subject of my former paper.* I then obtained the formula

$$f = \epsilon \sqrt{\frac{d}{3gp}}.$$

To translate this into the symbols of the present paper

$$f = p_c - p',$$

$$d = g\rho$$

and

$$\epsilon = \sqrt{\frac{3}{2}} \frac{\sqrt{\pi}}{18} \frac{H}{c^2}.$$

According to my intention ϵ should have been equal $\frac{H}{c^2}$, but from the manner in which it was obtained it has the value given above (Appendix, note 5 (*b*)). Hence we have

$$p_c - p' = \frac{\sqrt{\pi}}{18} \frac{H}{c^2 \alpha}.$$

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

The corresponding equation (Appendix, note 5 (*a*)) derived from equation (140) is

$$p_e - p' = \left\{\frac{\sqrt{\pi}}{18} f_5\left(\frac{c_1}{s}, \frac{c_2}{s}\right) + \frac{8}{9}\frac{s}{\sqrt{\pi}}\frac{1}{c_1} f_6\left(\frac{c_1}{s}, \frac{c_2}{s}\right)\right\} \times \frac{H}{c^2\alpha},$$

or when $\frac{c}{s}$ is small

$$p_e - p' = \frac{\sqrt{\pi}}{18}\frac{H}{c^2\alpha},$$

and when $\frac{c}{s}$ is large

$$p_e - p' = \frac{8}{9}\frac{1}{\sqrt{\pi}}\frac{s}{c}\frac{H}{c^2\alpha}.$$

It thus appears that the present results entirely confirm the previous results so far as they went; and the present investigation is a completion, not a correction, of the former one.

The present investigation shows that, besides being proportional to the quantity of heat, the force is proportional to the linear divergence of the lines along which the heat flows; and hence, if these lines are parallel, no matter how great may be the difference of temperature, the gas can exert no pressure above the normal pressure which it will exert on all surfaces alike. This is the case, whether the heat is communicated to gas or is spent in causing evaporation from the surface.

The relation between the difference of pressure and the divergence of the lines of flow affords a clear explanation of the complex phenomena of the radiometer; and as these phenomena have attracted a great deal of interest, I feel that an explanation of them will not be out of place.

Divergence of the lines of flow and the radiometer.

120. We may readily obtain a graphic representation of the results expressed by equations (124) and (140).

Let *AB*, fig. 12, be a plate from which heat is being communicated to the surrounding gas. Then the lines representing the flow of heat, drawn according to the law of conduction, are shown in the figure.

(1) The shape of these lines depends on the distribution of temperature over AB.

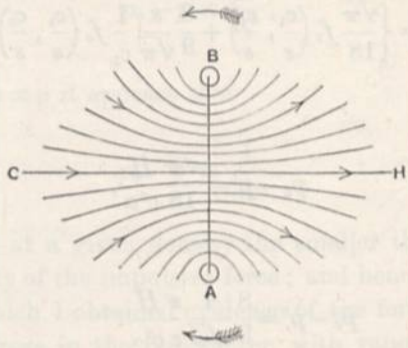


Fig. 12.

Fig. 12 shows what the lines would be if AB were hot on one side and cold on the other, the gas being at the mean temperature and of unlimited extent.

(2) The distribution of temperature on an opposite surface, or containing vessel, will also affect the shape of the lines of flow.

Fig. 13 shows the lines between two parallel plates opposite one another, the inside face, H, being hotter than the opposite face, C, while the gas and the outside faces of the plates are at the mean temperature of C and H.

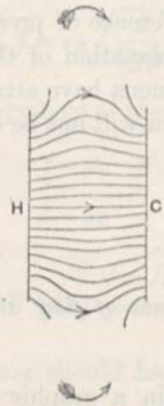


Fig. 13.

(3) The shape of the lines will also depend on the shape of the hot surface, and the nature of the surface as affecting the rate at which it communicates heat to the gas.

Fig. 14 shows the direction of the lines for a cup-shaped surface, supposed to be uniformly at a higher temperature than the gas.

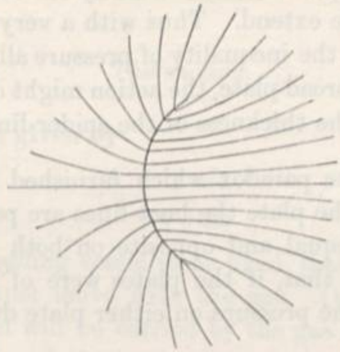


Fig. 14.

In all these figures the lines are supposed to be drawn so that the distance between any two lines is somewhere between s and $2s$, so that the excess of pressure along the lines of flow depends, *cœteris paribus*, on the angle between two consecutive lines. Thus the divergence of the lines indicates the excess of pressure, the excess being, *cœteris paribus*, proportional to the square of the angle of divergence.

The shapes of the curves of flow are independent of the density of the gas, but the distance between these lines varies inversely as the density; and since the angle between the lines at distance s increases with s, we see that the excess of pressure along the lines of flow increases as the density diminishes, as long as the mean range of the molecules is not limited by the size of the containing vessel. When this point is reached, there can be no further increase in the mean range, and the excess of pressure will pass through a maximum value, and then fall with the density, until the ratio of the excess of pressure to the mean pressure becomes constant, which it will be in the limit.

The distribution of the force of impulsion as indicated by the figures.

121. In fig. 12 the divergence of the lines of flow is much greater towards the edges of the plates than in the centre; hence the excess of pressure will be greater towards the edges. In the same way, on the cold side of the plate, the convergence of the lines of flow is greatest towards the edges, and here the pressure will be least.

When the density of the gas is such that the width of the plate is large compared with s, the divergence of the consecutive heat-lines at the middle of the plate is small, which shows that there would be but little action on this part of the plate. At the edges, however, the divergence is greater, and there must always be action at the edges; and the smaller the density of the gas, or the narrower the plate, the more nearly to the middle of the plate will the inequality of pressure extend. Thus with a very narrow plate, such as a spider-line, we may have the inequality of pressure all over the plate, although in the same gas, with a broad plate, the action might only extend to a distance from the edge equal to the thickness of the spider-line.

Fig. 13 illustrates the paradox which furnished the clue to this theory. Towards the middle of the plate the heat-lines are parallel, and consequently the pressure would be equal and opposite on both plates, being the mean pressure of the gas; so that, if the plates were of unlimited extent, there would be no change in the pressure on either plate due to the one being hot and the other cold.

At the edges, however, the heat-lines diverge from the hot plate; hence at this point this plate would be subject to an excess of pressure, which would tend to force the plate back against the mean pressure of the gas on the outside. At the edges of the cold plate the heat-lines converge on to the plate; hence there will be a deficiency of pressure, and the tendency will be for the pressure at the back to force the plate forward toward the hot plate. Thus the action is not to separate the plates, but to force them both to move in the direction of the hotter plate—to cause the hot plate to run away, and the cold plate to follow it.

Fig. 14 shows the inequality of pressure which may exist over a surface, itself at uniform temperature, but differing from the temperature of the gas.

On the convex side the lines diverge much more rapidly than on the concave side, and hence the inequality of pressure due to the communication of heat will be greater on the convex side.

Stability of the equilibrium.

122. The figures give the lines of flow on the supposition that the gas is at rest and the surfaces all rigidly fixed. The condition would then be one of equilibrium. But in order that such a condition might be maintained, it would be necessary that the condition should be one of stable equilibrium. This is a point on which the foregoing reasoning furnishes us with no information.

It is satisfactory, therefore, to be able to see what must happen if the equilibrium is unstable. This is shown by equation (124), which gives the motion of the gas, so that there may be no forces.

If either the surface AB, or the containing vessel, be perfectly free to move, then no inequality of pressure will be possible, but motion must ensue. Equation (124) shows the law of such motion.

The motion.

123. The motion is given by

$$\bar{u} = \frac{s}{\sqrt{\pi}}\frac{d\alpha}{dx}.$$

If we suppose the containing vessel to be fixed, then, to allow of the motion of the gas, the plate must move with the gas. On the other hand, if the plate be held, the vessel will be carried by the gas in the opposite direction. Such is the phenomena of the radiometer. The vanes are as nearly as possible free to move in the vessel, so that their motion merely indicates the motion of the gas caused by the inequality of temperature in the gas, which inequality is maintained by the unequal temperature of the two sides of the vanes arising from their different power of absorbing light, or, in the case of curved vanes, by the greater temperature of the vanes as compared with the vessel.

The constraint which is put upon the vanes in a rotatory manner necessarily disturbs somewhat the free motion of the gas, as must also the friction of the pivot and other resistances. Therefore the condition of the gas within the vessel cannot be one of absolutely equal pressure; and when either the size of the vanes or the density of the gas are too great, the utmost inequality of pressure is insufficient to overcome these resistances, and there is no motion. If, then, exhaustion proceeds, the inequality of pressure increases, and motion ensues—the rate of which, if the vanes were absolutely free, would increase as the density diminished, until the mean range was limited by the size of the envelope, so that the larger the envelope the greater the possible rate of motion. When the paths of the molecules are limited by the size of the vessel, the motion would, if the vanes were perfectly free to move, remain constant for all further exhaustion; but the inequalities of pressure which the gas is capable of exerting diminish with the further rarefaction, and hence, in time, must cease to be sufficient to overcome the resistances to which the motion of the vanes is subject, and then the motion ceases.

124. There are many other points about the phenomena of the radiometer, but with most of these I have already dealt in my former papers, the reasoning of which, so far as it goes, appears to me to be perfectly consistent with the more complete view of the action to which I have now attained.

My chief object in introducing the phenomena of the radiometer in this paper has been to bring out how completely impulsion belongs to the same class of actions as thermal transpiration, and the other phenomena depending on the relation which the size of the external objects bears to the mean range within the gas.

The action does not depend on the distance between the hot and cold plates.

It has been supposed by some writers on the radiometer, that the action depends essentially on the distance between the vanes and the sides of the vessel. This distance, however, is now seen not to be of primary consequence, as no action will result, however close the plates may be, unless they are of limited extent—of sizes comparable with the mean ranges.

SECTION XIII. SUMMARY AND CONCLUSION.

125. The several steps in this investigation have now been described in detail. They may be summarized as follows:

(1) The primary step from which all the rest may be said to follow is the method of obtaining the equations of motion, so as to take into account not only the normal stresses which result from the mean motion of the molecules at a point, but also the normal and tangential stresses which result from a variation in the condition of the gas (assumed to be molecular). This method is given in Sections VI., VII., and VIII.

(2) The method of adapting these equations to the case of transpiration through tubes or porous plates is given in Section IX. The equations of steady motion being reduced to a general equation, expressing the relation between the rate of transpiration, the variation of pressure, the variation of temperature, the condition of the gas, and the dimensions of the tube.

In Section X. is shown the manner in which were revealed the probable existence (1) of the phenomena of *thermal transpiration*, and (2) the law of correspondence between all the results of transpiration with different plates, so long as the density of the gas is inversely proportional to the lateral linear dimensions of the passage through the plate; from which revelations originated the idea of making experiments on thermal transpiration and transpiration under pressure.

(3) The method of adapting the equations of steady motion to the case of impulsion is given in Section XI.

In Section XII. is shown how it first became apparent that the extremely low pressures at which alone the phenomena of the radiometer had been obtained were consequent on the comparatively large size of the vanes, and that by diminishing the size of the vanes similar results might be obtained at higher pressures; whence followed the idea of using the fibre of silk and the spider-line in place of the plate-vanes.

(4) In Section XII. it is also shown that while the phenomena of the radiometer result from the communication of heat from a surface to a gas, as explained in my former paper, these phenomena also depend on the divergence of the lines of flow; whence it is shown that all the peculiar facts that have been observed may be explained.

(5) In Section X. it is also shown that the phenomena of transpiration, resulting from a variation in the molecular constitution of the gas (investigated by Graham), are also to be explained by the equation of transpiration.

(6) Section II. (Part I.) contains a description of the experiments undertaken to verify the revelations of Section X. respecting *thermal transpiration;* which experiments establish not only the existence of the phenomena, but also an exact correspondence between the results for different plates at corresponding densities of the gas.

(7) Section III. contains a description of the experiments on *transpiration under pressure,* undertaken to verify the revelations of Section X. with respect to the correspondence between the results to be obtained with plates of different coarseness at certain corresponding densities of the gas; which experiments proved, not only the existence of this correspondence, but also that the ratio of the corresponding densities in these experiments are the same as the ratio of the corresponding densities with the same plates for thermal transpiration—a fact which proves that the ratio depends on the relative coarseness of the plates.

(8) Section IV. contains a description of the experiments with the fibre of silk and with the spider-line, undertaken to verify the revelations of Section XII.; from which experiments it appears that, with these small surfaces, phenomena of impulsion similar to those of the radiometer occur at pressures but little less than that of the atmosphere.

126. As regards transpiration and impulsion, the investigation appears to be complete. Most, if not all, the phenomena previously known have been shown to be such as must result from the tangential and normal stresses consequent on a varying condition of molecularly constituted gas; while the previously unsuspected phenomena to which it was found that a variation in the condition of a molecular gas must give rise, have, on trial, been found to exist.

The results of the investigation lead to certain general conclusions which lie outside the immediate object for which it was undertaken. The most important of these, viz. that gas is not a continuous plenum, has already been noticed in Art. 5, Part I.

The dimensional properties of gas.

127. The experimental results, considered by themselves, bring to light the dependence of a class of phenomena on the relation between the density of the gas and the dimensions of objects, owing to the presence of which the phenomena occur. As long as the density of the gas is inversely proportional to the coarseness of the plate, the transpiration results correspond; and in the same way, although not so fully investigated, corresponding phenomena of impulsion are obtained as long as the density of the gas is inversely proportional to the linear size of the objects exposed to its action. In fact, the same correspondence appears with all the phenomena investigated.

We may examine this result in various ways, but, in whichever way we look at it, it can have but one meaning. If in a gas we had to do with a continuous plenum such that any portion must possess the same properties, we should only find the same properties, however small might be the quantity of gas operated upon. Hence, in the fact that we find properties of a gas depending on the size of the space in which it is enclosed, and of the quantity of the gas enclosed in this space, we have proof that gas is not continuous—or, in other words, that gas possesses a dimensional structure.

In virtue of their depending on this dimensional structure, and having afforded us proof thereof, I propose to call the general properties of gas on which the phenomena of transpiration and impulsion depend, the *Dimensional Properties of Gases.*

This name is also indicative of the nature of these properties as deduced from the molecular theory; for by this it appears that these properties depend on the mean range—a linear quantity which, *cœteris paribus,* depends on the distance between the molecules.

In forming a conception of a molecular constitution of gas, there is no difficulty in realizing that such dimensional properties exist; there is, perhaps, greater difficulty in conceiving molecules so minute and so numerous that, in the resulting phenomena, all evidence of the individual action is lost. But the real difficulty is to conceive such a range of observational power as shall embrace, on the one hand, a sufficient number of molecules for their individualities to be entirely lost, while, on the other hand, it can be so far localized as regards time and space that, if not the action of individuals, the actions of certain groups or classes of individuals become distinguishable from the action of the entire mass. Yet this is what we have in the phenomena of transpiration and impulsion.

Although the results of the dimensional properties of gases are so minute that it has required our utmost powers to detect them, it does not follow that the actions which they reveal are of philosophical importance only. The actions only become considerable within extremely small spaces, but then the work of construction in the animal and vegetable world, and the work of destruction in the mineral world, are carried on within such spaces. The varying action of the sun must be to cause alternate inspiration and expiration of air, promoting continual change of air within the interstices of the soil as well as within the tissue of plants. What may be the effects of such changes we do not know, but the changes go on; and we may fairly assume that in the processes of nature the dimensional properties of gas play no unimportant part.

Nor is this all. It is by aid of the analogy which gas affords us that we must look forward to solve the mystery of the luminiferous ether. And although all attempts to frame a satisfactory hypothesis as to the molecular constitution of ether have hitherto failed, in none of these hypotheses have the tangential and normal stresses arising from a varying condition been taken into account; whereas the recognition of the part which these stresses play in the properties of gases shows, or at least suggests, the possibility that the phenomena of ether which we observe may depend largely upon analogous stresses.

Appendix.

(Added December, 1879.)

Note 1.

Since the reading of this paper I have had my attention called to a paper by W. Feddersen ("Über Thermodiffusion von Gasen," *Pogg. Ann.*, 1873). Feddersen made some experiments, and seems to have thought that he had discovered some such phenomenon. But the results he obtained were attributed by M. J. Violle to the presence of the vapour of water, against which no precautions appear to have been taken (*Journal de Physique*, 1875, p. 90). That M. J. Violle was right there can be no doubt, for the results obtained are now seen to be much too large for the true results, and are similar to those which I obtained before I had succeeded in sufficiently drying the air.

Note 2.

Graham applied the term "transpiration" to the passage of gases through capillary tubes as distinguished from the passage of gases through larger tubes and through apertures in thin plates, and applied the term "effusion" to the passage of gases through minute apertures in thin plates.

He did not apply either of these terms to the passage of gases through porous plates, because his experiments led him to conclude that the phenomena attending such passage were not the same as the phenomena attending either of the former, but were somewhere between the two.

By the fuller light thrown on to the subject by this investigation it appears that in the limit, when the tubes and holes are small enough according to the condition of the gas, the laws of transpiration are strictly the same as those of effusion, the theory of the phenomena being the same. Hence the continued use of two names appears to be unadvisable.

The term "transpiration" has been chosen in preference to "effusion," because it is found that as the passages become coarser, according to the condition of the gas, the law of the passage of gas through porous plates is still in strict accordance with the law of the passage through tubes, showing that the passages are of the nature of tubes rather than thin plates.

Note 3, Art. 7.

It will be observed that this dependence of the phenomena on a relation between the size of the surfaces and the mean path of a molecule is essentially different from what has been a common, but as is herein shown, erroneous supposition, that the phenomena essentially depend on distance separating the opposite surfaces. The one supposition makes the action of the radiometer depend on the size of the vanes, but leaves it independent of the size of the envelope, while the other makes the action depend on the size of the envelope, but leaves it so far independent of the size of the vanes.

Note 4, Arts. 41 and 104.

The assumption that the coefficients A, m, λ_1, and λ_2, also m' and λ_3, equation (99), are the same for stucco as for meerschaum, is equivalent to assuming that the only respect in which the interstices of these plates differ is that of coarseness. There is no *à priori* ground for making this assumption. The fact that the logarithmic homologues for stucco fit those for meerschaum through such a considerable range of densities proves the approximate truth of the assumption; but it is possible, since c_s and c_m are arbitrary dimensions, that the curves for transpiration under pressure depending on A, m, λ_1, and λ_2 may approximately fit for one value of c_s/c_m, and the curves for thermal transpiration depending on A_1, m, λ_1, λ_2, m', and λ_3 may approximately fit for another value of c_s/c_m. If this were so $\log.(c_s/c_m)$; the shift necessary to bring the curves into coincidence would not be the same for transpiration under pressure as for thermal transpiration, and as has been pointed out (Art. 41), this is to a certain extent the case, this ratio having the values 6·5 and 5·6—a difference which was sufficiently decided to call for notice, but which is not so large but that, as pointed out (Art. 41), it may possibly be due to the plates being hot in the one case and cold in the other. In any case the smallness of the difference is an additional proof that the interstices do not greatly differ as passages in any respect except that of size.

NOTE 5, ART. 119.

(a) $$\frac{H}{c_1^2}=\sigma\{M(u^2+v^2+w^2)\}.$$

Whence at the surface when $\frac{c_1}{s}$ is sufficiently small we have by equation (18)

$$\frac{H}{c_1^2}=\frac{3}{2}\frac{\rho}{\sqrt{\pi}}(a_e^3-a'^3),$$

or neglecting $\left(\frac{a_e-a'}{p'}\right)^2$

$$\frac{H}{c_1^2}=\frac{9p'}{\sqrt{\pi}}(a_e-a').$$

And when $\frac{c_1}{s}$ is large, we have by equation (128)

$$\frac{H}{c^2}=\frac{9p'}{\sqrt{\pi}}\frac{s}{c_1}(a_e-a').$$

Therefore substituting for $\frac{a_e-a'}{a'}$ in equation (140)

$$p_e-p_1=\left\{\frac{\sqrt{\pi}}{18}f_5\left(\frac{c_1}{s},\frac{c_2}{s}\right)+\frac{8}{9\sqrt{\pi}}\frac{s}{c_1}f_6\left(\frac{c_1}{s},\frac{c_2}{s}\right)\right\}\frac{H}{c_1^2 a}.$$

(b) In my former paper (*Proc. Roy. Soc.* 1874, p. 407)

$$\epsilon=\frac{d}{g}\frac{v^2\delta v}{6},$$

$$p=\frac{1}{3}\frac{d}{g}v^2, \text{ and } \frac{d}{g}=\rho.$$

Therefore, since

$$p=\rho\frac{a^2}{2},\ v=\sqrt{\frac{3}{2}}a, \text{ and } \delta v=\sqrt{\frac{3}{2}}\delta a,$$

$$\epsilon=\sqrt{\frac{3}{2}}\frac{p}{2}\delta a,$$

and putting $\delta a=a_e-a'$,

$$\epsilon=\sqrt{\frac{3}{2}}\frac{\sqrt{\pi}}{18}\frac{H}{c^2}.$$

CERTAIN DIMENSIONAL PROPERTIES OF MATTER IN THE GASEOUS STATE.

(An Answer to Mr George Francis Fitzgerald.)

[*From the* "Philosophical Magazine" *for May*, 1881.]

In the February number of the *Philosophical Magazine* there appeared a paper by Mr Fitzgerald, in which he criticised my paper "On Certain Dimensional Properties of Matter in the Gaseous State," *Philosophical Transactions of the Royal Society*, 1879. Mr Fitzgerald courteously put his remarks in the form of questions, expressing the hope that I would answer them. I was prevented by other work from preparing anything in time for insertion in the April number; but I now ask your space for a few remarks.

The objections taken by Mr Fitzgerald to my work may be summed up as three:—

(1) That by dividing space into eight regions I have adopted a method which is at once inelegant and unnecessarily elaborate.

(2) That I have omitted terms which, if retained, would have altered the results.

(3) That I have changed my views and adopted the theory which I had previously combated.

To all these accusations I would most emphatically plead *not guilty*. And I would further suggest, in explanation of Mr Fitzgerald's difficulty, (1) that he has not paid equal attention to all parts of my paper, but has rather confined his attention to those parts which relate to the phenomena of impulsion, in which he seems to be especially interested, and that thus he has failed to see that, in order to obtain any results whatever for transpiration, the division of space into regions is necessary; and (2) that in his anxiety to find a different result in the case of impulsion from that which I had obtained, he has failed to perceive that the terms which I have neglected, and of which he instances one as disproving my conclusion, are of a distinctly smaller order of magnitude than those which appear in my result.

As regards, then, the charge of inelegance, I am sure that Mr Fitzgerald would not for one moment have urged it had he not thought that the particular step to which he objects might be replaced by some other known method. One might as well abuse David because he used a stone and sling, as object to the inelegance of a mathematical method by which alone true results have been obtained. Of course I do not for one moment defend my method as being elegant, nor should I have noticed this remark were it not that, taken together with the more definite criticism to the same effect, it shows conclusively that Mr Fitzgerald has failed to notice the gist of the greater portion of my paper—that he has failed to notice one of the most important terms in the equation of transpiration and the manner in which this term enters. In the paragraph beginning at the bottom of page 104 he says, "With the symbols and notation I have no fault to find; but I must enter a protest against his elaborate and totally unnecessary *division of space into eight regions*. He might have perfectly well calculated equations (43) to (47) without rendering a difficult subject tenfold as elaborate as was necessary." And then he goes on to show how I might have obtained equations for the aggregate results at one integration. Clearly, then, he has seen no object in my division of space into regions, and is at a loss to account for it except as mere clumsiness in the integrations. Had he, however, looked closer, or even been careful to be accurate in his statement, he would have seen that the two equations (44), which are among those to which he refers, only apply to the partial groups for which u is respectively positive and negative, and that they contain a term which apparently disappears if the respective members of the two equations be added; and he would have seen that the same thing is true of equations (45)*, which hold only for groups for which v is respectively positive and negative, and from which two terms disappear when the results are added. Now these terms, which are the first and second, are sufficiently obvious in the partial equations, whereas they do not appear at all if the integration be extended to both groups; and if Mr Fitzgerald had followed the next articles (83) and (84), he would have

* The partial equations (45):—

$$\sigma_y^{v+}(Mu) = \frac{\rho a U}{2\sqrt{\pi}} - \frac{s}{2\sqrt{\pi}}\frac{d\rho a U}{dy} - \frac{s}{2\pi}\frac{d\rho a^2}{dx} - \frac{s}{2\sqrt{\pi}}\frac{d\rho a V}{dx},$$

$$\sigma_y^{v-}(Mu) = -\frac{\rho a U}{2\sqrt{\pi}} - \frac{s}{2\sqrt{\pi}}\frac{d\rho a U}{dy} + \frac{s}{2\pi}\frac{d\rho a^2}{dx} - \frac{s}{2\sqrt{\pi}}\frac{d\rho a V}{dx}.$$

The equation obtained by complete integration:—

$$\sigma_y(Mu) = -\frac{s}{2\sqrt{\pi}}\frac{d\rho a U}{dy} - \frac{s}{2\sqrt{\pi}}\frac{d\rho a V}{dx}.$$

seen why these terms are important. To ignore these two articles is to ignore the method by which the results for transpiration are obtained; and these results were the main purpose of the preliminary work in the paper.

To obtain any results at all for transpiration, it is necessary to divide space into two regions, or else to consider the mean range s as a function of the position of the point, and discontinuous at the solid boundaries; and by the latter method the determination of the form of the function requires that space should be divided. The results depend entirely on the terms which, when s is constant, disappear in the complete integration, but which, if different arbitrary values are assigned to s for the different regions, do not cancel when the partial integrals are added. No result whatever is obtained by complete integration if s be constant; and although Mr Fitzgerald does not seem to have noticed it, the late Professor Maxwell followed me in dividing space into two regions at the bounding surfaces, calling the two groups the *absorbed and evaporated gas*. But without the use of arbitrary coefficients he had no means of dealing with the variable condition of his gas, except by assuming that the same distribution holds in both groups at all points. To meet this assumption (which, he points out at the top of page 253*, is improbable) he had further to assume a highly complex and improbable condition of surface; and the result is that the equation he obtains (77) is short of the most important term. This term is that which gives the result when the tubes are small compared with s; and as this is the only case in which the results are appreciable, when Maxwell came to apply his equation to an actual case there was no sensible result.

In the first instance, I also began by considering space as divided only at the bounding surface, and, assuming the distribution in the two groups the same, integrated for the complete space; and the result I then obtained was precisely the same in form as that subsequently obtained by Maxwell. These results correspond with the experimental results for a tube whose diameter is large compared with s—called by Graham *transpiration*; but they do not at all correspond with the law which Graham found to hold when he used a fine graphite plug, and which I have shown to hold also with coarse stucco plugs when the gas is sufficiently rare, viz. that the times of transpiration of equal volumes of different gases are proportional to the square roots of the atomic weights. Graham had considered this law as depending on the fineness of the pores of the plug, and had suggested that the action then resembled that of effusion through a small aperture in a thin plate, rather than transpiration through a tube of uniform bore; and this is the assumption which Maxwell falls back upon to account for the difference between his calculated results and those of experiment. That I did not do

* "On Stresses in Rarefied Gases," Appendix, p. 249, *Phil. Trans.* 1879.

the same was owing to my having, by reasoning *ab initio*, after the manner explained in the analogy of the batteries, in the very first instance found that the law of the square roots of the atomic weights must hold in a tube whenever the gas was so rare that the molecules ranged from side to side without encounter, and to my having proved by experiment that both laws might be obtained with the same plug by changing the density of the gas. It was thus clear to me that some term had been omitted in my equation; and after a long search it was found that, though the term vanished in the complete integral, it appeared in the partial integrals when space was divided into regions, and that, as the values of s were obviously different in the different regions, the assumptions on which the complete integral had been obtained were clearly at fault. The further division into eight regions was not only for the sake of symmetry, but that all the other terms which enter into the partial integrals might be examined, and as being necessary in particular cases—as, for instance, in that of a round tube, which is also treated of in the paper.

Having thus shown that, however elaborate and inelegant, the division of space into regions is essential, it is unnecessary to defend it on other grounds. But I may remark, by the way, that such a division does tend greatly to simplify the consideration of motion. This, I think, is proved by the universal adoption of north, east, south, west, zenith, and nadir.

I have dwelt at considerable length on the foregoing point, as the misconception of this point is fundamental to all Mr Fitzgerald's criticism. The rest I may answer shortly.

With regard to Professor Maxwell's remarks on my paper, and his own work on the same problem, of course the sad circumstance of his death occurring, so that this was about the last work he did, renders it very difficult to approach the subject; but with reference to what I have already said, and in explanation of the apparently imperfect idea at which he arrived as to the scope and purpose of my method, it may be stated that, before writing his own paper, Professor Maxwell had only seen my paper in manuscript in the condition in which it was first sent in to the Royal Society, when the preliminary part was very much compressed, and, as I fear, somewhat vaguely stated, besides being founded on different assumptions from the present. Without entering further upon this now, I may refer to a letter which I addressed to Prof. Stokes after seeing an early copy of Prof. Maxwell's paper, and before I was aware of his illness, which letter was subsequently published in the *Proceedings of the Royal Society* for April 1880, p. 300.

Mr Fitzgerald has asked me for an explanation of the system on which certain terms are retained and others neglected. This is difficult to give in a few words; but I was under the impression that it is sufficiently explained in the paper. It seems to me that the difficulty which Mr Fitzgerald has found must have arisen from his having adopted the hitherto vague way of looking at the mean path of a particle (or in this case the mean range) as a small quantity, without strictly inquiring as compared with what it is small. In my paper, s is nowhere to be regarded as small except in cases where it comes into direct comparison with some definitely larger quantity. The small factors are $\frac{U}{\alpha}$, $\frac{s}{\alpha}\frac{d\alpha}{dx}$, and $\frac{s}{\alpha}\frac{U}{dx}$; the squares of such quantities being consistently neglected. Such factors as $\frac{s^2}{\alpha}\frac{d^2\alpha}{dx^2}$ and variations of higher order are zero in the case of transpiration, but in the case of impulsion they are of the same order as the results. But the retention of such terms in equations (42) to (48), or in the fundamental theorem, would only give rise in the results to such terms as $\frac{s^3}{r\alpha}\frac{d^2\alpha}{dx^2}$; so that as long as s is small compared with r no error can have arisen from the neglect of these terms. And this is the only case to which these results have been applied, the extreme case where s is large compared with r having been dealt with by a special method which gives rigorous results. In the first instance, all terms of the second order such as $\frac{s^2}{\alpha}\frac{d^2\alpha}{dx^2}$ were retained; and it was only after it was found that these did not in any way affect the results as a first approximation that they were neglected. The terms I have neglected are, as far as I perceive, the same as those neglected by Professor Maxwell; and such was the care taken in this matter (which is of fundamental importance) that I am very confident that there is no mistake. On the other hand, it is difficult for me to see how Mr Fitzgerald can have failed to see that the residual term, which he instances as showing that I am wrong in saying that my equations show that there is no force in the case of parallel flow, is distinctly of the second order of small quantities. But even to this term he has no right; for in order to obtain results to such an order the variations of s would have to be considered. It seems that Mr Fitzgerald is of opinion that the parallel flow of heat does cause stresses in the gas, and that he has been trying to find that I have not disproved the possibility of such stresses. If he confines his attention to stresses of the same order of magnitude as those now shown to exist in the case of converging or diverging flow, he will find that both Professor Maxwell and I have proved the impossibility of their existence; but if he goes, as he appears unwittingly to have done, to a higher order of small quantities, then I have nothing to say, except that he has no inconsiderable task before him.

Lastly, as regards the charge of having changed my views and having adopted a theory which is practically the same as that which I had been previously combating, I can only say that against no theory have I said a word of which I do not maintain the truth. I have never asserted that the variation of pressure in the direction of the flow of heat, which I have consistently maintained to be necessary to the production of the phenomena of impulsion, may not be attended by a difference of pressure in different directions; and, of course, I have known that such must be the case since the time that I have seen and proved by experiment that this direct variation of the pressure depends on the convergence of the lines of flow, which was before the letter referred to appeared in *Nature*. But what I have consistently maintained is, that a difference of pressure in different directions (*i.e.* parallel and normal to the hot and cold surface) will not explain the experimental results; and this was the theory advanced in opposition to mine, and which Mr Fitzgerald still seems inclined to defend.

I am asked to mention the result which is referred to in Art. 54. I can only point to every phenomenon of the radiometer; for there the gas between the hot and cold surfaces always maintains a greater pressure on the hot than on the cold plate—a result which is fully explained in Art. 129, as the consequence of the divergence of the lines of flow from the hot plate and their convergence on to the cold plate, shown in fig. 13. If Mr Fitzgerald will only study the phenomena, he will see that it is he who has misapprehended the entire problem. He says a difference of pressure in different directions might tend to cause the plates to recede from each other. Obviously it would; but then there is not the slightest evidence that the plates do so tend to recede, while they actually move in the same direction, the cold plate following the hot. Hence no force merely causing them to separate can explain the phenomena. I have pointed this out over and over again, and now, so far from having changed my views, I have to go over the same ground again. I will take a simple case—a light mill with two equal radial vanes in the same plane, and on opposite sides of the pivot, one black and one white. Let the light be placed exactly opposite the vanes, and let the vanes be at rest. Also let the surface of the vessel and the gas be generally at the mean temperature of the vanes. If, then, the force were only such as tends to separate the hot and cold surfaces, there would be exactly the same force between the comparatively hot black vane and the colder glass as between the comparatively hotter glass and the colder white vane; for there are the same differences of temperature; and therefore the forces on the two vanes would tend to turn the mill in opposite directions, and the mill would remain at rest, instead of whirling round as it actually does. That the flow of heat caused the surfaces to follow each other was proved from the first by the experiments; and that there is no force causing the surfaces to separate of the same order of magnitude as the force which causes them to follow is now proved by the kinetic theory.

I think that now Mr Fitzgerald will reconsider his protest against § 53;

for while maintaining, on the one hand, a theory fundamentally different from that in my paper, he can hardly maintain, on the other, that there are no such theories, and that they have not found supporters. But, in truth, the remark in Art. 53 was not applied to the theory which Mr Fitzgerald seems to be supporting; and as I am sure that he is not prepared to maintain that the phenomena of the radiometer take place in an *absolute vacuum*, or are due to the *same cause as gravitation*, I am sure that he will not wish to stand sponsor to all the theories set forth since 1874.

In conclusion, I would say one word in acknowledgment of those remarks in Mr Fitzgerald's paper that were the reverse of critical, and to confess that it is a matter of no small satisfaction to have found a reader of Mr Fitzgerald's knowledge and acumen.

OWENS COLLEGE,
March 24, 1881.

34.

NOTE ON THERMAL TRANSPIRATION.

(In a Letter to Professor STOKES, Sec. R.S. Communicated by Professor G. G. STOKES.)

[*From the* "Proceedings of the Royal Society," No. 203, 1880.]

(*Received* October 25, 1879.)

OWENS COLLEGE, 23*rd October*, 1879.

DEAR SIR,

I have just received a copy of a paper by Professor Maxwell from the *Philosophical Transactions of the Royal Society*, read April 11, 1878, "On the Stresses in Rarefied Gases." To this paper I find that there is an appendix added in May, 1879, in the course of which he refers to my investigation in the following words:—

"This phenomenon, to which Professor Reynolds has given the name of Thermal Transpiration, was discovered entirely by him....It was not till after I had read Professor Reynolds's paper that I began to reconsider the surface conditions of a gas, so that what I have done is simply to extend to the surface phenomena the method which I think most suitable for treating the interior of the gas. I think that this method is, in some respects, better than that adopted by Professor Reynolds, while I admit that his method is sufficient to establish the existence of the phenomena, though not to afford an estimate of their amount."

As the abstract of my paper does not contain a sufficient account of what is in the paper to enable a reader to form a fair judgment of the relative merits of the two methods, I venture to request those interested in the subject to withhold their opinion until they have an opportunity of reading my paper. In the meantime I can only express my opinion that Professor Maxwell is mistaken in supposing that the results which are obtained from his method are more definite than those to be obtained by mine.

His method only applies to a particular case, and the equation which he has given is identical with that which I have given for this particular case.

The particular case treated by Professor Maxwell is the extreme limit—when the tube is large as compared with the distances between the molecules; he does not deal at all with the other limit—when the distances between the molecules are large as compared with the tube. Whereas I have given definite values for the coefficients in both limits, as well as indicating the manner in which the coefficients vary between these limits.

It so happens that the case in which the tube is large as compared with the molecular distances is one in which the results are too small to be experimentally appreciable, and hence Professor Maxwell's method does not explain any of the actual experimental results.

In order to explain the experimental results obtained with porous plates, Professor Maxwell has reverted to Graham's assumption that fine plates act as apertures in thin plates, while the coarse plates act like a tube, an assumption which my experiments show conclusively to be unnecessary and erroneous, the only sensible action in either case being that of tubes, and hence the phenomena of porous plates is that of transpiration and not effusion.

I remain,

Yours truly,

OSBORNE REYNOLDS.

Professor STOKES, F.R.S.,

Secretary to the Royal Society.

Note by the Communicator.

In communicating the above letter to the Royal Society, in accordance with Professor Reynolds's wishes, I would beg permission to add a few remarks.

Professor Maxwell did not profess to treat more than the two extreme cases, constituting what Graham called respectively transpiration and diffusion. His statistical method applies, indeed, only to the first of these limits; but he has distinctly considered the second, following a suggestion of Sir William Thomson's. It is true that at the first limit, as Professor Reynolds remarks, the results are too small to be experimentally appreciable;

but this was distinctly stated by Professor Maxwell himself, at the foot of p. 256.

As to the second limit, I must remark, in the first place, that I cannot find that Graham made any assumption that porous plates act as apertures in thin plates. The result that the time of passage varies, *cæteris paribus*, as the square root of the density in the case of fine porous plates, was obtained by pure experiment; and though he could not fail to notice the accordance of this result with that of the mere hydrodynamical passage through a small aperture, he has carefully distinguished between the two. Nor can I agree with Professor Reynolds in regarding the explanation given by Professors Thomson and Maxwell of the phenomenon of thermal transpiration or thermal effusion, whichever it be called, afforded by assimilating a fine porous plate to a thin plate pierced by apertures of ideal fineness as erroneous, even though it should be shown that such assimilation is unnecessary. Professor Maxwell did not profess to treat in his paper the intermediate cases between the two extreme limits.

Perhaps I should mention, that the foot-note at p. 281 in Professor Maxwell's paper was added as the paper passed through the press. I recollect noticing the thing as, in my capacity of Secretary, I looked over the paper before sending it to be printed off, and considering whether I should affix a date. As, however, it seemed to me to contain merely an explanation of an expression in the text, and as Maxwell, who had carefully added the dates of fresh matter in other parts, did not seem to have thought it necessary to do so in this case, I left it as it was. In a letter I received from him at the time, he informed me that he felt very ill, and was hardly fit even to go through his own paper; though a subsequent letter, in which he entered into some scientific matters, was written in his usual cheerful style. No one had, I believe, at that time any notion of the very serious nature of his illness.

G. G. STOKES.

March 13, 1880.

35.

SOME FURTHER EXPERIMENTS ON THE COHESION OF WATER AND MERCURY.

[*From the* "Proceedings of the Literary and Philosophical Society of Manchester." *Session* 1880-81.]

Two years ago I exhibited before this Society a vertical tube, 60 inches long and $\frac{5}{16}$-inch in diameter, in which mercury sustained itself by its internal cohesion and adhesion to the glass, to a height of 60 inches, without any aid from the pressure of the atmosphere*. This tube was subsequently shown at the Royal Society, and was submitted to intermittent observation at the College until about nine months ago, when one day, on being erected, it either collapsed or was broken by the fall of the mercury. The fracture taking place simultaneously with the fall of the mercury, it was impossible to say which.

This tube was of common German glass, such as is used for chemical purposes, and as it proved insufficiently strong I deferred further experiments until I could obtain a tube of greater strength. This led to considerable delay, but I have now a tube 90 inches long, in which mercury suspends itself in a water vacuum, resisting a tension or negative pressure of three atmospheres. Although this is probably still far short of the possible limit, a certain amount of interest attaches to the probability that the tension in this tube is the highest to which any fluid matter in the universe has been subjected.

Since my former communication, in working both with the old tube, and particularly with the new and longer tube, further experience has been gained of which it is my present object to give some account.

During the year and nine months before the old tube broke no great change had been noticed in the water and mercury within the tube; the former became rather cloudy and the latter showed symptoms of a scum.

* *Proceedings Lit. and Phil. Soc.* 1877—8, p. 155.

These changes were but little noticed, as they did not apparently interfere with the suspension of the mercury.

The most noticeable circumstance was that as time went on the difficulty of getting rid of the air and getting the tube into such a condition that its contents would sustain themselves diminished. In the first instance it had been only after a fortnight's attempts that suspension was obtained. The first successful suspension was obtained in the following manner: a little of the water was allowed to pass up by the side of the mercury when the tube was in an inclined position, the tube was then brought gently down so that the water reached the top or closed end of the tube as nearly as the air, generally a small bubble, would admit; then further inclined until the closed end was so low that the air bubble and water would float up to the open end and pass out, leaving the straight portion of the tube and part of the bend full of mercury. The tube was then left in this position for 24 hours, when on being erected the mercury sustained itself. It was then again reversed and left for some days, when on being erected not only was the mercury sustained for the 30 inches above the barometer, but it remained suspended when the pressure of the air on the lower end was reduced by the air-pump to one or two inches of mercury.

No other method ever proved successful with this tube. It was always necessary to leave the tube reversed for a longer or shorter interval.

As to what went on in this interval I changed my opinion. At first I thought it must be that time was necessary to bring the mercury or water into more intimate contact with the tube, but subsequent observation convinced me that the interval was necessary to allow the water with such air as it contained to drain up between the mercury and the glass—that in this way the surface of the glass was freed from air. After arriving at this view I observed the tube carefully to see if after it had remained some days in the reversed position any trace of water was left. I could find none either while the tube was full or after the mercury had fallen; but owing to the fact that there was always water on the open end I could make no such comparison with the barometer as would show that the vacuum in the tube was absolutely free from vapour tension.

Having obtained from Messrs Webb of Manchester tubes 12 feet long, $\frac{7}{8}$-inch external and $\frac{1}{4}$-inch internal diameter, one of these tubes was closed at one end and bent so as to leave the closed branch 7 feet 6 inches long. The bend is a curve of about 2 inches radius, and the two branches or limbs are not quite parallel; they straddle so that at 3 feet 6 inches from the bend they are 7 inches apart. At this point the shorter or open limb was again bent back through an angle of 160 degrees, so that when the main tube is vertical the mouth points downwards. The bending of so large and thick a tube was a matter of some difficulty, but was successfully accomplished by Mr Haywood and Mr Foster of Owens College. The tube was then firmly fastened on to a board by Mr Foster, and the board pivoted on to a stand so that the tube can be turned round in a vertical plane.

The tube being placed so that there was a slight downward incline all the way from the open to the closed end, some water was introduced into the open end. This having passed down to the closed end and filled all the tube, mercury was introduced, which ran down, forcing out the water. As soon as the long limb and the bend were full of mercury, the tube was turned into an upright position, the mercury sinking down and forcing out the water in the shorter limb. Having reduced the water until it only occupied about 9 inches above the mercury, the tube was again brought into a somewhat horizontal position, but this time it was turned so that the mouth was downwards, the incline being from the closed to the open end. Before reaching that position the pressure of the air had caused the mercury to fill the longer limb, leaving only water in the shorter limb; as the inclination continued, the mercury and water began to change places, and the water passed up round the bend into the longer limb; when 5 or 6 inches of water had passed in, the tube was erected and turned over the other way, so that the closed end was lowest, the water and the bubble of air running up and passing out. The tube was then further inclined until nearly vertical, the closed end down, and the tube was left in this position for 24 hours.

This, it will be noticed, was the process by which, after the first trial, had proved almost invariably successful with the former tube, and the only circumstances likely to cause any difference in the new and old tube were the comparatively short time the water and mercury had been together, in the new tube, and the greater length, 90 inches, as against 60 with the old tube. As regarded this latter difference, it would not affect a partial erection of the tube, so that if the time was not an element of importance, it was to be expected that at all events the mercury would sustain itself until the closed end had reached a position 60 inches above the bend.

On examining the tube, however, after it had been standing 24 hours, it presented a very different appearance from that usually presented by the old tube; instead of a polished column of mercury it was frosted with water between itself and the glass; it was clear that the upward draining of the water had been very imperfect, a great deal remaining adhering to the glass.

On slowly erecting the tube the mercury showed no symptom of suspension, leaving the closed end quietly as erection proceeded.

The whole process of passing the water up the tube was again repeated with the same result for three days.

The frosted appearance, however, gradually diminished, and on the fourth day a partial suspension was obtained. The mercury remained up until the tube was nearly erect.

Having obtained so much, and as it appeared by the turbid state of the water that the mercury was impure, the tube was emptied, washed out several times, both with water and a solution of nitrate of mercury, and was then refilled as before with water and carefully purified mercury. At first it presented much the same frosted appearance as before, and there was no suspension.

With a view to expedite matters the board carrying the tube was taken off its pivot and laid flat on a table nearly horizontal; in this position it was so adjusted that the water and mercury both extended all along the tube. The tube was then connected by an indiarubber tube with an air-pump, and the air pumped off until, and so long as, the water boiled in the tube.

The board was then turned over on its edge so that the water might come in contact with that part of the tube which had been previously below the mercury.

Having been turned back into its first position and adjusted so that the closed end and long limb were slightly lower than the rest, the pump was kept working, and the end of the board at which was the closed end of the tube was gently hit with the hand. At first this caused the mercury to chatter all along the tube, and wherever the mercury broke, a minute bubble of air or steam showed itself; these passed slowly along to the open end, until, after this had been continued for some time, the chattering ceased, and the last bubble had passed out.

Keeping the closed end lowest, and without breaking the connection with the pump, the board was replaced on the pivot and the tube erected. The mercury remained suspended until the tube was nearly erect, and this without any assistance from the air on the open end, so that the tension was nearly 90 inches.

The same process of tapping was then repeated, and the tube replaced and left with the closed end downwards and the air pumped off, for 24 hours. There was then no frost, but a bright column of mercury, which on erection remained suspended, the pump having been worked so as to remove the last trace of air. The tube was not left standing, but was inverted and erected for a few minutes each day for 8 days, including this morning. When the pump was again worked, and the tube sealed by clips on the indiarubber before bringing it to the Society's rooms—which somewhat difficult undertaking has been accomplished by Mr Foster, who has assisted me throughout in these experiments. (On being erected in the Society's rooms the mercury remained suspended for about 15 minutes; it then gave way with an audible click and sank to such a level as showed that there had not been air pressure of $\frac{1}{20}$th of an inch on the lower end.)

This experiment shows that the cohesion of water and mercury, and their adhesion to each other and glass, will withstand a tension of 3 atmospheres or 90 inches of mercury, being one atmosphere more than was shown by the former tube.

But as I have been of opinion from the first that the limit of cohesion, whatever may be that of adhesion, is a much greater quantity, my object in making and recounting these experiments has not been so much to prove a somewhat higher cohesion as to throw light upon the circumstances on which the successful suspension depends.

The fact that the frost on the glass, the imperfect draining up of the water, and the non-suspension of the mercury all occur together, and may all be removed by time or by the complete removal of the air from the glass, seems to show that even when glass is completely wet or covered with water, there may be and generally is a considerable quantity of air still adhering to the glass.

As regards the limit of the cohesive or adhesive strength of water and mercury, I conceive this to be beyond any test that can be applied by gravity. Several feet more might be attained, but the difficulties increase with the length of the tube. It has, however, occurred to me that by centrifugal force the limit may be reached in tubes a few inches long, and I am at present preparing some experiments for this purpose, of which I hope soon to be able to give some account.

36.

ON THE BURSTING OF THE GUN ON BOARD THE THUNDERER.

[*From the eighteenth volume of the* "Proceedings of the Literary and Philosophical Society of Manchester." *Session* 1878–79.]

IN the interval which elapsed between the bursting of the gun and the report of the Committee much thought and some trouble has been expended in divining the possible causes which might, under one set of circumstances or another, have led to such a result. It now appears however that different as have been the various suggestions, they all resembled each other in one particular, namely, that they were all wrong.

It is to be hoped, however, that all the ingenuity that has been expended will not have been thrown away and that some improvement may result from the pointing out of such numerous defects. That in some respects, such as the increasing twist and the sudden steps or shoulders on the outside of the gun, the present system is defective is shown quite apart from the recent accident; and although it now appears that the moving forward of the shot as the rammer was withdrawn had probably nothing to do with this accident, it cannot be considered satisfactory that this moving forward should be so much the rule as it is shown to have been in the experiments recently undertaken.

Although at first sight it may appear that the fact of the gun having been loaded with two charges of powder and two shot is amply sufficient to explain the bursting, it may not be useless to examine somewhat closely into what would result under such circumstances. The bursting of a 38-ton wrought-iron gun is an experiment of which we should make the most, as we cannot expect to have it often repeated.

From the first accounts of the accident it appeared as though the gun had simply broken in two, like a carrot, at the first step, and that the front half had gone into the sea. Such a failure would not have implied an excess of pressure. It might have been caused by a great end strain, such as would have resulted had the shot jammed when in full career and carried away the fore part of the gun, or it might have resulted from the gradual weakening of the section of the gun at the shoulder owing to the different degrees of expansion immediately before and immediately behind. One or other of these causes appeared to afford the most probable explanation of the phenomena as described in the early accounts. In various subsequent reports, however, it was stated that fragments of the fore part of the gun were blown about in all directions. So that the gun, instead of having simply broken in two, must have burst like a shell in front of the first shoulder. This fact placed the phenomena in an altogether different light. The explosive bursting of the zone of the gun into fragments implied an enormous excess of pressure at this point of the gun.

In order to cause the tube of the gun to burst longitudinally at all would require several times the normal pressure, and the breaking up of the wrought-iron tube into fragments would show that the force was largely in excess of what was necessary to burst it.

After seeing these reports it appeared certain that the gun had been subjected, at the point of rupture, to a pressure enormously excessive, and the question became, whence could such a pressure have arisen? To me it appeared that nothing short of such an action as might, with a detonating fuse, result from the explosion of gun cotton or dynamite would explain the breaking of the gun into fragments. Had the shot become jammed the pressure might have been raised sufficiently to burst the gun, but with pebble powder even this seemed doubtful, and such an action seemed altogether inadequate to explain the breaking of the gun into fragments. It appeared, therefore, that there was but one conclusion to be drawn—there had been something abnormal in the loading. Had the gun been loaded with small grained powder, gun cotton, or dynamite, instead of pebble powder, such a result might have been produced; but then, the gun would, if it had burst, have burst at the breach unless the shot had slipped forward, and that there should have been two accidents appeared highly improbable. Besides, it was necessary to consider what sort of a mistake was most likely to have occurred; and the only possible mistake that could have been made on the spot appeared to be that of double loading.

The fact that if two complete charges were put into the gun, the powder of the second charge would be directly beneath the point of rupture appeared in favour of this, the easiest mistake. But would, supposing the powder to have been pebble powder, the pressure from the two charges have been sufficient to cause the result? At first it seemed to me that even supposing that the second charge had been ignited by the first, which was doubtful, this would not explain the suddenness or magnitude of the pressure. But on further consideration it appeared certain that the second charge would not be ignited by the fire from the first; and it then became clear that in this very fact we should have an amply sufficient explanation of the excessive pressure.

My object in writing this paper is to point out the probability of this explanation, and so, if possible, to induce the authorities to test it. It occurred to me several days before the report of the Committee appeared, and in spite of the improbability of such a mistake as double loading, I could not shake off the conviction that it afforded the true explanation. As I have pointed out, the blowing into fragments of a wrought-iron tube implied an explosive action such as might result from gun cotton or dynamite but which could not be produced by the slow burning of pebble powder. The point to be explained then is how the second charge could be brought into such a condition that it would explode like gun cotton. To understand this it must be remembered that in the usual way the grains of gunpowder burn from their outside only, so that the thicker the grains the longer will be the time occupied in burning, and for the same weight of powder the slower will the gas be given off. The reason why gun cotton is so much more destructive than gunpowder is not that it gives off more gas weight for weight, but that when ignited by a flash it burns so much quicker. If, therefore, by any means the whole mass of gunpowder could be heated up to the firing point at the same instant, so that the grains fired simultaneously inside as well as out, the action of the powder would be as quick or quicker than the gun cotton. And still further, if besides being heated the powder was compressed into a fraction of the space it usually occupies, the gases so confined would be capable of a still greater pressure.

Now if the after cartridge were fired and the forward cartridge were not ignited by the flash, and considering the length and fit of the shot it could hardly have been so ignited, then the after shot would be driven forward closing on to the forward shot and compressing the powder between until the pressure on the forward shot was at least half as great as the pressure of the gases behind the after shot, which would be between 10 and 20 tons on the square inch. Thus the powder would be subjected to a squeeze between the two shot such as would result from a blow. It would be compressed to a fraction of its former volume. The cubes would be crushed into a cake and the work of compression would be sufficient to heat the powder far beyond its point of ignition. Thus the entire mass of powder would be simultaneously ignited in a highly compressed and heated state. The force of such an explosion would be practically unlimited and would be located at the very point at which the gun burst. Hence in such an action we have ample cause for the effect produced.

But it will be asked why does not the same thing happen when a rifle is doubly loaded? It is said that in that case the second cartridge is generally blown out before it ignites, and this may be so, for in the rifle the pressure of the gas on the shot can never exceed above a twentieth part of what it is in the 12-inch gun, and hence in the case of the rifle its pressure may well be insufficient to ignite the powder between the shot.

This view of the action resulting from the firing of powder by percussion appears to me to be one which it would be well worth while to test, for if proved it would completely re-establish confidence in the strength of the guns, which has been somewhat rudely shaken.

Let a 12-inch gun be loaded with a double charge of powder and a double charge of shot, or a shot of double weight, and fired. If, as is probable, the gun does not burst, confidence in the gun will be re-established. Then let it be loaded twice over with the powder between the shot so as to ascertain whether the action of the powder when fired by percussion would not produce an effect similar to that which we are here considering. The destruction of one gun for the purpose of establishing confidence in all the rest would not seem to be an unworthy sacrifice.

37.

ON THE STEERING OF SHIPS.

[*From the* "British Association Report," 1880.]

I HAVE received an important communication from the Admiralty, upon the steering qualities and turning powers of H.M.S. 'Minotaur' and 'Defence.' As the experiments therein described were made in accordance with the request of the Committee of the British Association upon the Steering of Ships, and as the results obtained are very definite and important, I think it desirable that they should be placed upon record. I therefore append them to this notice. (See Tables, pp. 404—407.)

ADMIRALTY, S.W.,
19th September, 1879.

SIR,

I am commanded by my Lords Commissioners of the Admiralty to forward to you, herewith, for your information, with reference to my letter of the 30th April, 1877, S. $\frac{4136}{4903}$, the accompanying copy of a letter, dated the 31st July last, from Vice-Admiral Lord John Hay, commanding Channel Squadron, enclosing a copy of the tabular statements, forwarded therein, of the Trials of the Steering Qualities and Turning Powers of H.M.S. 'Minotaur' and 'Defence.'

I am, Sir, your obedient servant,

ROBERT HALL.

Osborne Reynolds, Esq.,
The Owens College, Manchester.

TRIAL OF

H.M.S. 'DEFENCE,' off Syracuse, Feb. 14, 1879.

Tonnage, 3720. Length, 280 ft. Beam, $54\frac{1}{2}$ ft.
Fitted with Griffiths's two-bladed left-handed propeller.
Immersion, 6 ft. Draught of water—forward, 25 ft.; aft, $26\frac{1}{2}$ ft.

No. of Trial corresponding to British Association	NATURE OF TRIAL	Experiments in each case to be distinguished by letters	Engines: Time required to stop or reverse (seconds)		Helm: Direction and time required to place it there	Wind: Force and direction relative to ship
			Stop	Rev'rse	Trns. Deg. Sec.	
I.	Ship going full speed ahead, the screw suddenly reversed and the helm put hard over.	A	20	55	Port $3\frac{1}{2}=25$ in 40	Light air
		B	15	55	,, $3\frac{1}{2}=25$,, 35	Light air ahead
		C	14	54	,, $3\frac{1}{2}=25$,, 25	
		D	15	57	,, $3=22$,, 23	
II.	The same repeated with helm set in opposite direction.	A	14	38	Starbd. $3\frac{1}{2}=25$ in 23	Calm.
		B	17	43	,, $3\frac{1}{2}=25$,, 28	9 points from port bow force 3.
		C	13	58	,, $3\frac{1}{2}=25$,, 28	
III.	The ship going fast astern, the screw suddenly started to drive her ahead and helm put hard over as in Trial I.	A	12	32	Port $3\frac{1}{2}=25$ in 25	11 points from port bow force 3
		B	8	35	,, $3\frac{1}{2}=25$,, 26	11 ,,
		C	10	40	,, $3\frac{1}{2}=25$,, 13	7 ,,
IV.	Trial III. repeated with the helm in the opposite direction.	A	15	42	Starbd. $3\frac{1}{2}=25$ in 20	Points: 2 from starbd. Bow force: 3
		B	11	31	,, $3\frac{1}{2}=25$,, 24	11 ,, port 3
		C	13	38	,, $3\frac{1}{2}=25$,, 18	3 ,, ,, 3
V.	Ship going full speed ahead with the helm amidships.	A B C D E F			Amidships	South. 1 to 2 Starboard quarter
VI.	Ship going full speed ahead, then the screw reversed with the helm amidships.	A B C D E F			Amidships	Calm. o.

STEERING QUALITIES.

I. H. P., 1902·5.
Diameter, 18 ft. Pitch, 21 ft.
Speed at trials, 8 knots.

Maximum speed, 8·5 knots.
Revolutions of engines, 63 per minute.
Revolutions of engines, 61 per minute.

RESULTS					REMARKS
Angle and direction ship's head went first	Time taken going ditto	Time ship's head returned to object	Time from first order till way was stopped	Angle and direction of ship's head when way was stopped	
	Min. Sec.		Min. Sec.		
$6\frac{3}{4}$ points starboard $\frac{1}{4}$,, port $\frac{1}{4}$,, starboard $\frac{3}{4}$,, ,,	2 20 0 55 1 45 1 50	Nil Nil Not timed Not timed	3 30 3 45 3 30 3 25	$5\frac{1}{2}$ points starboard, from time engines reversed to way stopped head went to port $1\frac{1}{4}$ points. $4\frac{1}{2}$ points port 5 ,, ,, $3\frac{1}{4}$,, ,,	A. Helm put over when engines were stopped. B C D do. do. when engines were astern.
$\frac{1}{4}$ point port Very slowly to starbd. Gradually to port	1 25 2 12 3 38	Nil Nil Nil	3 13 2 12 3 38	$2\frac{3}{4}$ points port $\frac{1}{4}$,, ,, $1\frac{3}{4}$,, ,,	Helm put over when engines went astern. do. do. do. wind springing up force 3.
Gradually to port	2 18 1 26 2 32	Nil Nil Nil	2 18 1 26 2 32	$2\frac{1}{4}$ points port $2\frac{1}{2}$,, ,, 3 ,, ,,	Helm put over when engines went ahead. NOTE.—With helm amidships the ship's head swings to port with stern way.
Continually to port when going astern	2 20 1 55 1 57	Nil Nil Nil	2 20 1 55 1 57	$5\frac{1}{4}$ points port $2\frac{3}{4}$,, ,, $3\frac{1}{4}$,, ,,	Helm put over when engines went ahead.
X — Y Nil — $\frac{3}{4}$ pts. port $1\frac{3}{4}$ pts. stbd. — $1\frac{1}{2}$,, stbd. 2 ,, — $1\frac{1}{3}$,, ,, 2 ,, — $1\frac{1}{2}$,, ,, $1\frac{1}{4}$,, — 2 ,, ,, $1\frac{1}{4}$,, — $1\frac{3}{4}$,, ,,	Minutes X Y 5 5 5 5 5 5 5 5 5 5 5 5	Nil	31 30	X $1\frac{1}{4}$ points starb'd, from time engines stopped and reversed to way stopped head went to port 6 points. Y $4\frac{3}{4}$ points starb'd from time (as above) head went to port 3 points in 3 min. 30 sec.	Ship going ahead 8 knots at commencement of trial. Y Time taken from starting.
$7\frac{1}{2}$ points port $13\frac{1}{2}$,, ,, $13\frac{3}{4}$,, ,, 14 ,, ,, $13\frac{3}{4}$,, ,, $14\frac{1}{2}$,, ,,	Minutes 5 5 5 5 5 5	1st circle 14 minutes 2nd circle 11 minutes 30 seconds	32 30	$89\frac{1}{4}$ points port, viz. twice round the compass to port and $17\frac{1}{4}$ points, from time engines were stopped to way stopped 4 points to port in 2 min. 30 sec.	First time making a complete circle in 14 minutes. Second time 11 minutes 30 seconds.

(Signed) R. R. CATOR, CAPTAIN.

TRIAL OF STEERING QUALITIES OR TURNING POWERS OF SCREW SHIPS.

GENERAL MEMO., Feb. 14, 1879.
H.M.S. 'MINOTAUR,' Feb. 14, 1879.

Tonnage, 10,627 (6621). Length, 410 ft. Beam, 59 ft. $3\frac{1}{2}$ in. I. H. P. 6702. Fitted with Hirsch's 4-bladed left-handed propeller.
Diameter, 24 ft. $3\frac{1}{2}$ in. Pitch, 27 ft. $0\frac{3}{16}$ in. Immersion, 1 ft. $9\frac{1}{2}$ in. Draught of water—forward, 27 ft. 2 in.; aft, 26 ft. 10 in. Max. speed, 14 knots.
Speed at trials, 8·2 knots. Revolutions of engines, 38 per minute. Revolutions of engines, 55 per minute.

No. of Trial corresponding to British Association	NATURE OF TRIAL	Experiments in each case to be distinguished by letters	Engines	Helm	Wind	RESULTS					REMARKS
			Time required to stop and reverse	Direction and time required to place it there	Force and direction relative to ship	Angle and direction ship's head first went	Time taken going ditto	Time ship's head returned to object	Time from first order till way was stopped	Angle and direction of ship's head when way was stopped	
			Sec.	Deg. / Sec.		Deg.	Sec.		Min. Sec.		
I.	Ship going full speed ahead, the screw suddenly reversed and the helm put hard over.	*a*	17	39 Port 23	Starboard Beam Force 1	Starbrd. $3\frac{1}{2}$	47	Not taken	1 17	Not taken	The ship was steered for mark on island 8 miles off. Angles taken by sextant.
								Min. Sec.			
		b	13	39 ,, 19		,, $7\frac{1}{2}$	57	1 39	2 50	20° to Port	
		c	13	$39\frac{1}{8}$,, 21		,, $1\frac{1}{2}$	33	0 42	2 23	37° ,,	
			Sec.	Deg. / Sec.		Deg.	M. S.	Min. Sec.	Min. Sec.		
II.	The same repeated with helm set in the opposite direction.	*a*	23	$32\frac{1}{4}$ Starb. 26	Starboard Beam Force 1	Port $5\frac{1}{2}$	1 8	2 8	2 30	Not taken	Stern way was never got on the ship, but in all cases the way was completely stopped.
		b	14	$33\frac{1}{8}$,, 17		,, $\frac{1}{2}$	1 30	Not taken	2 21	$32\frac{1}{2}$° to Stb.	
		c	10	33 ,, 16		,, $3\frac{1}{2}$	1 40	1 6	2 35	10° ,,	
III.	The ship going fast astern, the screw suddenly started to drive her ahead, and the helm put hard over as in Trial I.										
IV.	Trial III. repeated with the helm in the opposite direction.										
V.	Ship going full speed ahead, with the helm amidships.	*a*		Amidships	Starboard Beam Force 1	Port 17°	1 min.				
		b		,,		,, 51°	1 ,,				
VI.	Ship going full speed ahead, then the screw reversed, with the helm amidships.	*a*	14 sec.	Amidships	Starboard Beam Force 1	Port 50°	1 min.				

(Signed) HARRY H. RAWSON, CAPTAIN.

H.M.S. 'MINOTAUR' AT VIGO, JULY 31, 1879.

SUMMARY OF RESULTS OF TRIALS OF THE STEERING QUALITIES AND TURNING POWERS OF HER MAJESTY'S SCREW SHIPS 'MINOTAUR' AND 'DEFENCE.'

SHIP	No. of Trial	Experiments	DIRECTION SHIP'S HEAD TENDS TO TURN UNDER INFLUENCE OF				RESULTS			REMARKS
			Reversed effect of rudder	Screw (left-handed)	Wind	Helm	Head first went to	Sec.	Angle and direction of ship's head when way was stopped	
'Minotaur'	1	*abc*	Port	Port	Nil before commencing to turn	Starboard	Starboard 4·2 deg. in...	45½	28½ deg. to Port	The ship in this case answers the reversed effect of rudder and action of screw.
	2	*abc*	Starboard	Port		Port	Port 3·2 deg. in	46	11¾ deg. to Starboard	Ship acts under reversed effect of rudder.
	5	*ab*	Nil	Starboard		Nil	Port 34 deg. in	60		It will be seen that the ship goes 1½ times as far to port (due to screw) as in trial 5.
	6	*a*	Nil	Port		Nil	Port 50 deg. in	60		
'Defence'	1	*a*	Port	Port	Nil before commencing to turn	Starboard	6¾ points Starboard in	140	1¼ points to Port	The final direction of ship's head in all 4 cases is to port, that is, follows the influence of reversed effect of rudder and action of screw as in case of 'Minotaur.'
		b	Port	Port		Starboard	¼ point Port in	55	4½ points to Port	
		c	Port	Port		Starboard	¼ point Starboard in...	105	5 points to Port	
		d	Port	Port		Starboard	¾ point Starboard in...	110	3¼ points to Port	
	2	*a*	Starboard	Port	Nil	Port	¼ point Port in	85	2¾ points to Port	In 2nd case ship is influenced by the reversed effect of rudder, but in the 1st and 3rd by the screw and helm.
		b	Starboard	Port	Port } very slight	Port	Very slowly to Starboard in	132	¼ point to Starboard	
		c	Starboard	Port	Port } very slight	Port	Gradually to Port in...	218	1¾ points to Port	
	3	*a*	Starboard	Starboard	Port	Port	Gradually to Port		2¼ points to Port	In these cases ship obeys the influence of rudder.
		b	Starboard	Starboard	Port	Port			2½ points to Port	
		c	Starboard	Starboard	Starboard	Port			3 points to Port	
	4	*a*	Port	Starboard	Starboard	Starboard	Continually to Port when going astern		5¼ points to Port	In these cases ship obeys the influence of the reversed effect of rudder. These results of 3 and 4 show that the direction ship's head may turn is uncertain.
		b	Port	Starboard	Port	Starboard			2¾ points to Port	
		c	Port	Starboard	Starboard	Starboard			3¼ points to Port	
	5		Nil	Starboard	Starboard at first	Nil	Average rate of 1½ pts. in 5 min.			Two trials were made by reversing the engines with helm amidships with the following results: 1st from time engines stopped and reversed to way stopped, head went to port 6 pts., 2nd to port 3 pts.
	6		Nil	Port	Nil	Nil	12·8 pts. in 5 min. (average rate)			Made 2 complete circles to port in 14 min. and 11½ min. respectively.

(Signed) JOHN HAY, VICE-ADMIRAL.

S. 8087. 1879.

STEERING QUALITIES AND TURNING POWERS OF SCREW SHIPS.

'Minotaur' at Vigo,
31*st July*, 1879.

No. 165.

SIR,

With reference to your letter of the 25th April, 1877, S. $\frac{4136}{4727}$, addressed to my predecessor, Vice-Admiral Sir Beauchamp P. Seymour, relative to the Steering Qualities and Turning Powers of Screw Ships, I have now the honour to enclose for the information of the Lords Commissioners of the Admiralty the results of experiments that have, under my direction, taken place in H.M. Ships 'Minotaur' and 'Defence,' together with a summary of the same—observing that these experiments so far as they go, seem to be useful as illustrating the views of the British Association.

I have, &c.,

JOHN HAY,
Vice-Admiral Commanding.

To the Secretary of the Admiralty.

38.

ON THE EFFECT OF OIL IN DESTROYING WAVES ON THE SURFACE OF WATER.

[*From the* "British Association Report," 1880.]

THIS paper contained a short account of an investigation from which it appeared that the effect of oil on the surface of water to prevent wind-waves and destroy waves already existing, was owing to the surface-tension of the water over which the oil spread, varying inversely as the thickness of the oil, thus introducing tangential stiffness into the oil-sheet, which prevented the oil taking up the tangential motion of the water beneath. Several other phenomena were also mentioned. The author hopes shortly to publish a full account of the investigation.

39.

ON SURFACE-TENSION AND CAPILLARY ACTION.

[*From the* "British Association Report," 1881.]

In the first place it was pointed out that, although surface-tension has hitherto been considered as a statical or hydrostatical force only, such actions as the spreading of oil upon water exhibit phenomena, and those of a very marked kind, which depend, not on a statical force, but on the maintenance of this force while the surface is contracting at a very high velocity. And, in the second place, it was pointed out that the assumptions on which Laplace's theory of surface-tension is founded are insufficient to explain these phenomena, which suggest certain relations between the range of the intermolecular attractive forces and the dimensions of these molecules.

It was shown that if the surface of pure water be touched at some point with a slightly oiled needle, the oil spreads out quickly in a circular patch, which patch at first extends with great rapidity. But it was not the rapidity of extension that was so much the point of remark, as the motion of the surface of the pure water before the advancing oil. In the usual way this motion is shown by a rib or slight elevation of the water immediately at the edge of the oil. When the initial surface is very clean the rib is always formed, but it only becomes apparent under peculiar circumstances. It is often apparent on the surface of a deep pool formed at a sharp bend of a stream, for instance, to anyone fishing. The more rapid flow into the pool causes ascending currents, which, spreading out at the surface, give rise to radial currents of pure water, which sweep back and hold at bay the oil or transparent scum on the surface of the rest of the pool, and which but for the outward motion would rapidly extend over the pure surface. Under these circumstances the edge of the scum is definitely marked by a fine rib, which shows itself in certain lights as though a fine gut-line were floating on the water and were carried, first in one direction and then in the other, according as the radial current or the spreading force of the scum is in the ascendant. It is difficult to render this rib apparent on the surface of water contained in a vessel, although this may be one or two feet in diameter. This may be done, but the motion which gives rise to the rib may be rendered apparent by other means,—by dusting the surface of the pure water with some insoluble powder such as flowers of sulphur. The motion of the surface is rendered apparent by the motion of the dust. It is then seen that the dust does not fall back before the oil as though the surface of the pure water were in a general state of contraction, for there is absolutely no motion in the dust except in the immediate neighbourhood of the edge of the oil. It is as though the dust were swept back by the advancing edge of the oil; the dust, already swept up into a compact mass, coming up to each fresh particle, pushes it before it, until a bright yellow band is formed marking the edge of the oil. The result is to give the impression that the dust is being driven back by the oil—as if the oil were spreading from some inherent expansive force; but, as a matter of fact, the oil is being drawn forward by the contraction of the dust-covered surface of the pure water, and the fact that the dust does not move till the oil reaches it shows that the contraction takes place entirely at the edge of the oil in an almost infinitely narrow band.

This phenomenon of surface-contraction is very remarkable, for it would be inferred from other hydrodynamical phenomena that viscosity would to some extent resist the action of contraction, and thus tend to distribute this action over a considerable area, and that the contraction is not so distributed shows that there is virtually no resistance to contraction, or that the surface-tension at the points at which the surface is contracting is at least equal to the tension at those points of the surface which are at rest.

This conclusion implies much more than the tacit assumption, made by Laplace and subsequent writers, that the forces of cohesion obey the law of statical fluid pressure—equality in all directions. It is well known as regards other phenomena that this law holds only when fluids are at rest or in uniform motion; whereas here we have a case in which the same law holds for a portion of fluid which is moving with great rapidity relative to the fluid in its immediate neighbourhood.

Laplace's theory is founded on an assumed attraction, between the molecules, which attraction does not extend to sensible distances, and on the tacit assumption already mentioned, that the pressure, whether impressed or molecular, is equal in all directions. To explain the apparent absence of viscosity in the dynamical phenomena some further assumptions are necessary. If the force of cohesion is due to molecular attraction these dynamical phenomena require that the molecules under their mutual attractions should not be in a state of equilibrium, except in so far as they are held by the forces transmitted from one part of the fluid to another.

Such a condition would exist if the range of attraction extended beyond the distance of a single molecule, that is, if the molecules are spherical or in such a state of motion that they cannot fit like bricks. But whatever might be the shape of the molecules, if the forces of cohesion acted between adjacent molecules only, then they would be in equilibrium in all positions; there would be no instability and no rapid contraction, although, according to Laplace's theory, the force would be sufficient to prevent extension of the surface, and hence to explain the statical phenomena of capillary tension such as the suspension of drops. It is therefore argued that these dynamical phenomena are important, as throwing a certain amount of light on the character of the forces which cause cohesion between molecules.

40.

ON THE FLOATING OF DROPS ON THE SURFACE OF WATER DEPENDING ONLY ON THE PURITY OF THE SURFACE.

[*From the Twenty-first Volume of the* "Proceedings of the Manchester Literary and Philosophical Society," 1881.]

(*Read* October 4, 1881.)

It is well known that under certain circumstances drops of water may be seen floating on the surface for some seconds before they disappear. Sometimes during a shower of rain these drops are seen on the surface of a pond, they are also often seen at the bows of a boat when travelling sufficiently fast to throw up a spray. Attempts have been made to explain this phenomenon, but I am not aware of any experiments to determine the circumstances under which these drops are suspended. Having been deeply engaged in the experimental study of the phenomena of the surface tension of water, and the effect of the scum formed by oil or other substances, it occurred to me that the comparative rarity of these floating drops would be explained, if it could be shown that they required a pure surface, a surface free from scum of any kind. For, owing to the high surface tension of pure water, its surface is rarely free from scum. The surface of stagnant water is practically never free except when the scum is driven off by wind. But almost any disturbance in the water, such as the motion of the point of a stick round and round in the water, or water splashed on the surface, will serve to drive back the scum for a certain distance. This may be shown by scattering some flowers of sulphur on the surface. This powder is insoluble and produces no scum, and hence it serves admirably to show the motion of the surface and whatever scum there may be upon it. If when the surface is so dusted a splash be made by a stick so as to throw drops on to the sulphured surface, at the first splash no floating drops are produced; but after two or three splashes in rapid succession it will be seen that the sulphured scum has been driven back by the falling water, leaving a patch of clear surface, and on this drops will float in large numbers and of all sizes. These drops are entirely confined to that portion of the surface which is clear. The drops, either by their initial motion or by the current of air, glide rapidly over the surface from the point at which they are formed. When, however, they reach the edge of the scum they disappear, apparently somewhat gradually. I have this summer made the experiment on several ponds and on various days, and I have never found any difference. Any scum, however transparent, prevented the drops, and they always floated in large numbers when the scum was driven back in the manner described, by the wind or any other way.

This result points to the conclusion that whatever may be the cause of this suspension, it depends only on the surface of the water being pure, and not at all on the temperature or condition of the air.

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END OF VOLUME I.

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