On the Contributions of the Rev. Henry Moseley, F.R.S., to the Theory of Glacier Motion, and the present State of the Problem. By William Mathews, Junr.

'On the Descent of Glaciers.' — *Proceedings of the Royal Society*, April 19, 1855.

'On the Motion of a Plate of Metal on an Inclined Plane when dilated and contracted, and on the Descent of Glaciers.'—*Proceedings of the Royal Society*, April 11, 1861.

'On the Mechanical Possibility of the Descent of Glaciers by their Weight only.'—*Proceedings of the Royal Society*, January 7, 1869.

'On the Mechanical Impossibility of the Descent of Glaciers by their Weight only.'—*Philos. Mag.*, May 1869.

'On the Uniform Motion of an Imperfect Fluid.'—*Philos. Mag.*, May 1869.

'On the Descent of a Solid Body on an Inclined Plane when subjected to Alternations of Temperature.'—*Philos. Mag.*, August 1869.

'On the Mechanical Properties of Ice.'—*Philos. Mag.*, January 1870.

ALTHOUGH scientific men have been wrangling for nearly a century about the true causes of glacier motion, it is a disheartening fact that we are still unable to account for it upon rational mechanical principles. Since the brilliant contributions made to the controversy by Professor Tyndall ten years ago, the question appears to have gone to sleep, and to have been abandoned by physicists for more promising subjects of speculation. Now, however, that Canon Moseley has again entered the lists, and aroused us from our lethargy by the stirring articles he has recently published, it is a fitting opportunity to pass in review the part he has played in the discussion, to examine the present condition of the problem, and the direction in which we may look for a final solution.

Theories of glacier motion may be classified either according to the forces to which the motion is attributed, or according to the assumed physical properties of glacier ice upon which those forces are supposed to act. There are only two known forces capable of making a glacier move—gravitation and heat. If we take these forces as a primary, and the physical properties as a secondary basis of classification, glacier theories may be arranged as in the following table: —

MOTIVE POWER—GRAVITATION.

1. A glacier is a rigid mass of ice which slides along its channel in the same way as any other heavy body down an inclined plane.	Sliding theory. DeSaussure.*
2. A glacier is a collection of dislocated fragments of ice, with their under surfaces in a state of continual liquefaction. These fragments slide down the channel when the slope is less than that at which they would remain at rest if they were not deliquescent.	Sliding theory. Hopkins.†
3. A glacier is a mass of ice of a plastic nature, like mud or putty, the particles of which slide over one another without a solution of continuity.	Plastic theory. J.D. Forbes.‡
4. When two pieces of ice with deliquescent surfaces are brought into contact, they freeze together (Faraday). The pressures called into play at various points of a glacier produce fractures in the mass. The fractured portions move into new positions, and are reunited by regelation.	Fracture and regelation theory. Tyndall.§
5. The freezing point of water is lowered by pressure, and conversely, pressure when applied to ice converts part of it into water. The pressures produced at various points of a glacier liquefy portions of it, the water finds its way into new positions, in which the pressure is less, and where it is consequently reconverted into ice.	Pressure and liquefaction theory. James Thompson.‖

* De Saussure, 'Voyages dans les Alpes,' ch. vii. § 535. 'Une autre cause qui s'oppose avec beaucoup d'efficace à un accroissement excessif des neiges et des glaces, c'est leur pesanteur, qui les entraîne avec une rapidité plus ou moins grande dans les basses vallées, où la chaleur de l'été est assez forte pour les fondre.

'Presque tous les glaciers, tant du premier que du second genre, reposent sur des fonds inclinés; et tous ceux d'une grandeur un peu considérable ont au-dessous d'eux, même en hiver, des courans d'eau, qui coulent entre la glace et le fond qui la porte.

'On comprend donc, que ces masses glacées, entraînées par la pente du fond sur lequel elles reposent, dégagées par les eaux de la liaison qu'elles pourraient contracter avec ce même fond, soulevées même quelquefois par ces eaux, doivent peu à peu glisser et descendre en suivant la pente des vallées ou des croupes qu'elles couvrent.

'C'est ce glissement lent, mais continu, des glaces sur leurs bases inclinées qui les entraîne jusques dans les basses vallées.'

† William Hopkins, 'On the Motion of Glaciers.'—*Transactions of the Cambridge Philosophical Society*, vol. viii. p. 50.

‡ James D. Forbes, 'Travels in the Alps of Savoy'—*Occasional Papers on the Theory of Glaciers.*

§ John Tyndall, 'The Glaciers of the Alps.'—*Philosophical Transactions of the Royal Society.*

‖ James Thompson, in *Proceedings of the Royal Society*, May 1857.

MOTIVE POWER.—HEAT.

6. A glacier is a mass of ice traversed by a vast number of fissures, into which water is infiltrated. When the temperature of the air falls below the freezing point, the water in the fissures freezes, and, expanding at the same time, forces the glacier in the direction of least resistance, forming a new set of fissures in the process.	Dilatation theory. Charpentier, Agassiz.*
7. A glacier may be considered as a pile of sheets of ice, which contract and expand under variations of temperature, just as a sheet of lead would do. As they lie upon an inclined surface, each contraction and expansion is accompanied by a motion down that surface of the centre of gravity of the mass.	Contraction and expansion, or Crawling theory. Moseley.

I have quoted Saussure's theory as it is usually given, although he employs language much more general, and even consistent with the sliding theory, in the form it assumed in the hands of William Hopkins, whose contributions to the science of glacier motion we have next to consider.

This distinguished man, who has left the stamp of genius upon every subject that he handled, was the first to point out that the objection commonly urged against the sliding theory—that it must necessarily result in an accelerated motion—was not a valid one. It next occurred to him that the inability of a rigid body to move down a plane of small inclination by its own weight alone, in consequence of frictional resistance, might not hold if the lower surface of the body were in a state of disintegration. He therefore devised and carried out the following experiment, the description and results of which I shall give in his own words :—

'A slab of sandstone was so arranged that the inclination of its surface to the horizon could be slowly and continuously varied by the elevation of one edge. The surface was in the state in which it had been sent from the quarry, and in which such stones are sometimes laid down as paving stones, retaining the marks of the pick with which the quarryman has shaped them, without any subsequent process for rendering the surface smooth. The slab thus presented a grooved surface (the grooves running in very nearly parallel directions) having some resemblance to those over which existing glaciers move, but having little of the smoothness of *roches polies*. The best measure, however, of the degree of its roughness is

* Charpentier, 'Essai sur les Glaciers,' § 11. Agassiz, 'Études sur les Glaciers,' ch. xii.

this: when placed at an inclination of about 20°, a piece of polished marble would just rest upon it.

'The slab was so placed that the direction of the grooves coincided with that of greatest inclination. A frame of about 9 inches square and 6 inches in depth, without top or bottom, was then placed on the slab, and filled with lumps of ice from a neighbouring ice-house, in such a manner that the ice and not the frame (which merely served to keep the ice together as one mass) was in contact with the slab. In the experiments in which the following results were obtained, weights were placed on the ice, such that the pressure on the slab was at the rate of about 150 lbs. on the square foot.

Inclination of the slab.	Spaces in decimals of an inch through which the loaded ice descended in successive intervals of 10 minutes.	Mean space in inches for 1 hour.
3°	·08, ·05, ·07, ·03, ·04, ·05. ·07, ·06, ·04	·31
6°	·09, ·10, ·09, ·07, ·08	·52
9°	·14, ·12, ·17, ·14, ·19, ·20	·96
12°	·38, ·34, ·36, ·37	2 00
15°	·43, ·41	2·50
20°	The mass descended with an accelerated motion.	

'When the inclination was 9°, about two-thirds of the weight was removed; the velocity was diminished by nearly one-half.

'When the inclination of the slab did not exceed 1°, there was a small but very appreciable motion.

'On the surface of a slab of the same kind of stone, *smooth* but not *polished*, there was appreciable motion at an angle of 40′. Nor am I prepared to say that either in this or the preceding case the angle was the least at which sensible motion would take place.

'When the surface used was that of polished marble, there was sensible motion at the smallest possible inclination. The motion, in fact, afforded almost as sensitive a test of deviation from horizontality as the spirit-level itself.

'In all these experiments the ice melted continually but very slowly at its lower surface in immediate contact with the slab. During the night the temperature descended below that of freezing, and the motion entirely ceased.

'The angle at which the accelerated motion just begins to take place is that at which the ice would just rest upon the inclined

plane, if the temperature of the slab and of the air were at or below the freezing temperature, so that no disintegration of the ice should take place. This angle appears to be nearly the same in the case of ice, on the grooved slab I made use of, as for that in which polished marble was the sliding body, and is that whose tangent determines the coefficient of friction between the slab in question and *solid* ice. When the slab was of polished marble, this angle was very small.

'In the experiment above detailed we have these results:—

'1. For all angles less than that just mentioned the motion was *not an accelerated motion.* This result was verified in every experiment I made.

'2. For inclinations not exceeding 9° or 10°, the velocity, *cæteris paribus*, was approximately *proportional to the inclination.* This, I doubt not, would hold in all cases in which the inclinations should be sufficiently small compared with the angle of accelerated motion. It is manifestly equivalent to the assertion that the velocity is proportional to the moving force.

'3. The velocity of the mass was increased by an increase of weight.'

This simple experiment is one of the most important contributions ever made to the theory of glacier motion. It was an experiment of an eminently *crucial* character, and one in which no extraneous forces were called into play, the loading of the ice merely diminishing the disproportion between the weight upon the slab, and that of a glacier upon its bed. An actual glacier, in some portions of its course at any rate, is in a condition very similar to that of the ice upon the slab: it is broken into numerous fragments, and its under surface, even in winter, is in a state of continuous liquefaction; the chief difference consists in the friction of the fragments against the sides of the channel, which would tend to give them an angular motion, and to throw the greatest velocity of translation into the centre of the course.

We may regard it, then, as conclusively established by '*Hopkins's experiment*' that *sliding due to gravitation* is a real and very important element in glacier motion.

I have dwelt upon this experiment at so great length, not only on account of its intrinsic importance, but because it has been either ignored or misrepresented by subsequent inquirers, and also because I shall have to place it in sharp contrast to the speculations of Canon Moseley.

From Mr. Hopkins we pass to Principal Forbes, to whose researches on glaciers science is so deeply indebted. But with

respect to his theory, notwithstanding the ingenuity with which he has supported it, I fear a verdict of '*not proven*' is the only one that can be returned.

Professor Tyndall's brilliant discoveries are too fresh in our minds to need any description here. His is the second great contribution to the theory of glacier motion. He has proved that ice, when broken by pressure, can be moulded by the same agency through regelation into any assignable form, without a trace of the original surfaces of fracture. Ice, in fact, under pressure, is as plastic as wax. But—and here Tyndall differs from Forbes—fracture is a necessary element in the process.

The views of Professor James Thompson are not so much an independent theory as a physical explanation of regelation. The liquefaction of ice by pressure, discovered by this physicist, is an undoubted fact, but its precise significance as an element of the problem has yet to be determined.

Having briefly described the theories in which the motive power is attributed to gravitation, I turn to those which have their origin in heat. Here we may at once pass over the dilatation theory of Charpentier and Agassiz as amply disproven, without caring to encumber our pages with the arguments that have disposed of it. This brings me to the theory of Canon Moseley, which is the main subject of the present essay.

In the autumn of the year 1853, Mr. Moseley's attention was directed to the extraordinary behaviour of a sheet of lead upon the roof of Bristol Cathedral, which had been renewed in 1851, but not having been properly fastened to the ridge-beam, had descended bodily 18 inches into the gutter. I continue the description in Mr. Moseley's own words in his first communication to the Royal Society:—'The sheet of lead which had so descended measured from the ridge to the gutter 19 ft. 4 in., and along the ridge 60 ft. The descent had been continually going on from the time the lead had been laid down. An attempt made to stop it by driving nails through it into the rafters had failed. The force by which the lead had been made to descend, whatever it was, had been found sufficient to draw the nails. As the pitch of the roof was only $16\frac{1}{2}°$, it was sufficiently evident that the weight of the lead alone could not have caused it to descend. Sheet lead, whose surface is in the state of that used in roofing, will stand firmly upon a surface of planed deal when inclined at an angle of 30°, if no other force than its weight tends to cause it to descend.'

Mr. Moseley was not long in assigning a true physical

cause to this phenomenon. The roof had a southern aspect, and the lead in the daytime was powerfully heated by the sun, cooling again at night. It was consequently subjected to a continued succession of considerable variations of temperature. When it received an increment of temperature, the expansion due to that increment would be mainly in the direction of least resistance, so that the lower edge of the sheet would move down the roof farther than the upper edge would move up it. When, on the other hand, it sustained a decrement of temperature, the contraction due to that decrement would be mainly in the direction of least resistance, so that the upper edge of the sheet would move down the plane farther than the lower edge would move up it. The result in both cases would be a descent of the centre of gravity of the sheet, and therefore of the mass considered as a whole. Mr. Moseley investigated a mathematical formula applicable to the case, and substituting in it the known coefficient for the expansibility of lead, and the probable variation of temperature, deduced a result nearly corresponding with the facts that actually occurred. The motion of the sheet of lead, from its similarity to that of a worm, may be appropriately termed a *crawling* motion.

Mr. Moseley, not satisfied with having discovered the solution of an interesting problem, conceived that in the roof of Bristol Cathedral he had found a clue to the vexed question of glacier motion.

'Glaciers,' he urges, 'are, on an increased scale, sheets of ice placed upon the slopes of mountains;' 'ice has nearly twice the expansibility of lead;' the daily range of temperature in their vicinity is considerable; 'that they must from this cause descend into the valleys is therefore certain.' He applies to the Mer de Glace the same formula which had done such good service in the case of the sheet of lead, and deduces a rate of motion not very different from the reality. 'I have had,' he remarks, 'the less hesitation in offering this solution of the mechanical problem of the motion of glaciers, as those hitherto proposed are confessedly imperfect. That of De Saussure, which attributes the descent of the glacier simply to its weight, is contradicted by the fact that *isolated fragments of the glacier stand firmly on the slope on which the whole nevertheless descends*; it being obvious that, if the parts would remain at rest separately on the bed of the glacier, they would also remain at rest when united.'

It is curious to observe how the writer of this sentence deals with 'Hopkins's experiment.' 'The motion,' he says, 'which Mr. Hopkins attributed to the dissolution of the ice in

contact with the stone, would, I apprehend, have taken place if the mass had been lead instead of ice, and it would have been but about half as fast, because the linear expansion of lead is only about half that of ice.' Now, since the fragments of ice melted continuously at their lower surfaces, the temperature must have been above the freezing point, and could not have been instrumental in the slightest degree in causing the expansion of ice already at 0° Cent. The descent of the ice therefore could not have been due to its expansion, for no expansion took place. Moreover, Mr. Moseley's theory requires that the motion at night due to contraction from cold should be equal to that in the day due to expansion from heat; whereas Mr. Hopkins says, '*during the night the temperature descended below that of freezing, and the motion entirely ceased.*'

It is a pleasing feature of scientific controversies, to which the glacier question offers no exception, that whenever a novel speculation is ventured upon, someone immediately contradicts it. Not many weeks elapsed after the presentation to the Royal Society of the last-described paper, before Principal Forbes flung a lance at Canon Moseley. In a communication to the Royal Society read June 14, 1855, Principal Forbes pointed out that a glacier differs from the sheet of lead in having a much greater thickness in proportion to its area, and that the Canon had neglected to bring into proper relief a very important distinction between ice and lead, in their relation to heat. The former melts at 0° Cent., whereas the latter requires for its liquefaction a temperature more than three times that of boiling water. When solids expand under the influence of heat, their linear expansion is proportional to their increase in sensible temperature. This law, however, only holds *up to the point of liquefaction*; when that has been reached, each additional increment of heat is expended in changing the form of the substance to which it is communicated, not in expanding it or raising its temperature. Happily for the human race, the temperature of the air never rises high enough to change the form of lead, and therefore the range of air temperature may be assumed as approximately equal to that of a sheet of the metal subject to its influence. But with ice how great a difference! Variations of air temperature of wide extent may and do take place, without being the slightest guide to the range of temperature of ice; nay, without even affecting its temperature in the very smallest degree. It is in the hottest seasons of the year, when the disintegration of the ice is the most rapid, and when it often happens that the temperature of air in contact with the glacier never, for days together, falls below the freezing

point, that glacier motion is the swiftest. In calculating, by his formula, the motion of the Mer de Glace, Canon Moseley assigned to its whole mass a daily range of temperature equal to that observed by De Saussure upon the Col du Géant, where, during the whole of his stay there, the thermometer never fell below the freezing point; a range, therefore, as Forbes remarked, 'between limits absolutely incapable of affecting the expansion of the ice.'

Here Forbes left the question, but I think he ought fairly to have carried it one step farther. It is just conceivable, if we could be provided with a glacier having its lower strata many degrees below 0°, and capable of being infiltrated by surface water, that a warm atmosphere might be instrumental in raising the temperature of those lower strata. But before the crawling theory could be applied to such a glacier, its range of temperature must be determined by observations, not within the atmosphere, but in the substance of the ice itself. On the one hand we have the maximum of 0°; what have we on the other as the minimum temperature of glacier ice?

The only observations I am acquainted with bearing upon this question are those made upon the Aar Glacier in 1840 by Agassiz and his companions. This physicist bored holes in the glacier of depths ranging up to 25 ft., and buried minimum thermometers in them, with the following results:—

At depths of from 8 to 25 ft. the temperature of the ice was constantly $-\frac{1}{3}°$ Cent., whatever the surface temperature.

When the surface temperature was a little above 0°, the thermometers showed $-\frac{1}{3}°$ Cent. at 1 ft. deep.

When the surface temperature was several degrees above 0°, the thermometers showed 0° down to 7 ft. deep.*

It follows from these observations that oscillations of atmospheric temperature affect a glacier to a depth altogether insignificant compared with its total thickness; and this agrees perfectly well with the low conductivity of ice, and with its known opacity to radiant non-luminous heat.

In his second communication to the Royal Society, read April 11, 1861, Canon Moseley further developes his theory, considers some of the objections urged against it, and describes the following experiment:—Having prepared a deal board 9 ft. long and 5 in. broad, he fixed it at an inclination of $18\frac{1}{2}°$ against the wall of a house with a southern aspect, and placed

* Agassiz, 'Études sur les Glaciers,' ch. xv. Desor, 'Excursions et Séjours dans les Glaciers,' pp. 160, 184.

a sheet of lead upon it the eighth of an inch thick, weighing 28 lbs., with its edges turned over the edges of the board so as not to bind upon it. The lead was observed morning and evening from February 16 to June 28, 1858. The average daily descents of the lead were one-tenth of an inch for the month of February, and somewhat more than two-tenths for the months of May and June.

Mr. Moseley next describes the observations of Schumacher on the dilatation of ice, made upon a block 6 ft. 3 in. long and 6 by 6½ inches in section, exposed to temperatures varying from −2·3° to −22° R., from which the coefficient of expansion of ice for 1° R. was determined to be ·00006466. He urges that if this block had been placed upon the deal board, it would have descended twice as fast as the lead, if it had experienced '*the same variations of temperature*;' by which I understand him to mean variations equally great, *but all below the freezing point.* Such a block, he says, may be considered to be made up of thin plates parallel to its upper surface, each of which would descend by a motion proper to itself, and also by reason of the descents of those subjoined to it. If we had a block of varying instead of uniform thickness, it would descend with a differential motion, which on the scale of a glacier might become appreciable.

To bring Schumacher's block to the proportions of the Mer de Glace below the Tacul, Canon Moseley converts it into a slab 12 ft. long, 20 in. wide, and 2 in. thick. He then supposes it to be subject to *the same variations of temperature as the lead was*, reduces his slope to 5°, expands his slab to the actual dimensions of the Mer de Glace, and deduces a velocity twelve times as great as the reality; so that variations of temperature twelve times less than those of the lead might produce the actual motion.

In the act of descending, the slab of ice would be thrown into a state of compression in some parts and extension in others. The former might result in crushing, the latter in transverse crevasses. The rate of motion, by the reasoning above given, must diminish from the surface downwards. If the slab were thinner at the sides than in the middle, the surface motion of the middle would be faster than that of the sides, and the slab would crack obliquely to its axis. We thus have a physical explanation of the actual conditions of a glacier.

To the objections that had been urged against his theory, the Canon replies that the observations of Agassiz on the temperature of glacier ice are not to be relied upon; that ice, if

opaque to non-luminous, is transparent to luminous heat; that the latter cannot fail to dilate the ice to which it penetrates, and that it is precisely when the sun's action is the most powerful that the rate of motion is the greatest.

It is impossible not to accord to these arguments the merit of great ingenuity, but the following criticisms may, nevertheless, be fairly offered against them.

The whole superstructure of the crawling theory is founded on the hypothesis of the variation in temperature of the interior of glacier ice, and, until that hypothesis is verified by experiment, the theory cannot be translated from the region of speculation into that of reality. The observations of Agassiz point in the opposite direction: they may possibly be untrustworthy; if so, by all means let them be disproved; but until they are, they are entitled at least to a provisional acceptance.

The assertion that a glacier is permeable by luminous heat can scarcely be intended to apply to those portions of it which lie above the snow-line, nor even to the parts below that line which belong to the region of the névé. And yet these parts are not usually supposed to be exempt from the movement characteristic of the general mass. Even in the region of the glacier, properly so called, where its substance is actual ice, it may be doubted whether the sun's rays penetrate many inches below the surface. Every traveller has noticed the strong resemblance which that surface bears to the upturned edges of a pack of slates, and how opaque it becomes as it disintegrates under the influence of heat. Again, if the sun's rays be supposed capable of penetrating the substance of a glacier, they could not reach those parts of it which are covered by moraine and rubbish, except to the small extent which they might travel through them, when incident obliquely upon the unprotected ice. In all such parts—and in some glaciers they comprise a large area—the motion ought to be greatly diminished, if not entirely arrested.

Mr. Moseley identifies the motion of a glacier with that of a slab of solid ice, and the latter with the motion of a sheet of lead. If this identification be a true one, if the glacier expands as a whole under the influence of an increment of heat, at each such expansion there will be a point in its length where the motion will be nil. Above this point, if it be below the summit-level, the glacier will move up-hill, or be crushed in its attempt to do so; below it, each part will move with a velocity proportional to its distance from the point at rest. It is almost needless to remark how contrary to experience such a law of motion is, and how very greatly a real glacier differs

from the character assigned to it. If, on the other hand, it be compared not to a solid slab of ice, but to a cluster of dislocated fragments, each of these fragments will descend only with the motion proper to its own expansion, which will be inappreciable when compared with the actual march of a glacier.

Nearly eight years elapsed since the date of the memoir I have just been examining, before Canon Moseley again entered the arena, and, in the two next papers on our list, directed public attention to the unsatisfactory state of the glacier problem. In these papers, instead of attempting to strengthen his own position, he has adopted the controversial expedient of carrying the war into the enemy's country, and has endeavoured to demonstrate that the descent of glaciers by their weight alone is a mechanical impossibility. In the face of Hopkins's experiment, the enterprise must be pronounced a rash one. I proceed to describe the manner in which it is conducted.

If, says Mr. Moseley, a transverse section of a glacier were to be made, the ice would be found to be moving differently at every point of it. The velocity is greater at the surface than deeper down, and at the centre of the surface than at the edges. There is a constant displacement of the particles of ice over one another, and alongside one another, to which is opposed the resistance known as *shearing force*. By the property of ice called regelation, when a surface so sheared is brought into contact with a similar surface, it unites with it so as to form one continuous mass. Between the resistance to shearing and the force which tends to bring the glacier down there must be a mechanical relation, so that if the shearing resistance were greater the force would be insufficient to cause the descent.

Mr. Moseley supposes an imaginary glacier of unlimited length, lying on an even slope, and having an uniform rectangular channel to which it fits exactly, and which is of an uniform roughness sufficient to tear off the surface of the glacier as it advances. This mass of ice is conceived to be divided by an infinite number of equidistant vertical planes parallel to its central line or axis, and of other equidistant planes parallel to its bed, and thus to be cut into rectangular strips lying side by side and above one another. Each of these strips will move with the same velocity in every part, and will be continually shearing over two adjacent strips, and being sheared over by two others. If we consider the portion of one of these strips which moves in a day across any transverse section of the glacier, the work of its weight, for motion to be

possible, must be equal to the work of its shear plus the work of its friction.

Before the comparison can be made we must know the shearing force of ice, and to this important inquiry Canon Moseley next addresses himself.

In default of previous observations bearing upon this subject, he devised the following experiment:—

'Two pieces of hard wood, each three inches thick and of the same breadth, but of which one was considerably longer than the other, were placed together, the surfaces of contact being carefully smoothed, and a cylindrical hole 1½ inch in diameter was pierced through the two. The longer piece was then screwed down upon a frame which carried a pulley, over which a cord passed to the middle of the shorter piece, which rested on the longer. There were lateral guides to keep the shorter piece from deviating sideways when moved on the longer. The hole in the upper piece was brought so as accurately to coincide with that in the lower; small pieces of ice were thrown in, a few at a time, and driven home by sharp blows of a mallet on a wooden cylinder. By this means a solid cylinder of ice was constructed accurately fitting the hole. Weights were then suspended from the rope passing over the pulley until the cylinder of ice was sheared across. As by the melting of the ice during the experiment the diameter of the cylinder was slightly diminished, it was carefully measured with a pair of callipers.'

From the mean of two experiments, conducted in this manner, the shear of ice per square inch, or *unit of shear*, was determined to be 75 lbs.

By incorporating in the mathematical formula applicable to the imaginary glacier Professor Tyndall's measurements on the Mer de Glace, and assuming its inclination to be 4° 52′, Mr. Moseley has arrived at the conclusion that, for the Mer de Glace to descend by its own weight, at the rate at which Professor Tyndall observed it descending at the Tacul, the unit of shear could not have been greater than 1·3193 lbs., and that to produce the actual motion, with the real unit of shear, a force in aid of the weight and 34 times as great must be called into existence, and applied in the direction of motion. For such a force to be produced by the weight of the glacier alone, the density of ice would require to be increased more than 400 times.

Mr. Moseley has exhibited the incapacity of a glacier to descend in virtue of its weight in the following striking manner:—

'Let a strip of ice one square inch in section and one mile in length, in the middle of the surface of the imaginary glacier,

be conceived to be separated from the rest throughout its whole length except for the space of one inch, so that throughout its whole length, except for that one inch its descent is not retarded either by shear or friction. Let, moreover, this inch be conceived at the very end of the glacier, so that there is no glacier beyond it. Now it may easily be calculated that this strip of ice, one inch square and one mile long, lying on a slope of 4° 52′, without any resistance to its descent, except at its end, must press against its end, by reason of its weight, with a force of 194·42 lbs. But the cubical inch of solid ice at its extremity opposes, by the *shear* of its three surfaces, whose attachment to the adjacent ice is unbroken, a resistance of 3×75 lbs., or 225 lbs. That resistance stops, therefore, the descent of this strip of ice one mile long, having no other resistance than this opposed to its descent, by reason of its detachment from the rest. It is clear, therefore, that it could not have descended by its weight only when it *adhered* to the rest, and when its descent was opposed by the shear of its whole length; and the same may be proved of any number of miles of strip in prolongation of this. Also, with obvious modifications, it may be shown in the same way to be true of any other similar strip of ice in the glacier, whether on the surface or not, and therefore of the whole glacier.'

It is far from my intention to impugn the accuracy of Mr. Moseley's calculations. Professor Huxley has quaintly remarked that the flour produced by the mathematical mill depends mainly upon the grain that is put into it, and it is the quality of the latter that I shall take the liberty of examining. Mr. Moseley assumes that the resistance to the sliding of a glacier along its bed is equal to the shearing force of ice, and that *every* point of a glacier in the same transverse section moves with a differential velocity. We know from Hopkins's experiment that the former assumption is contrary to the fact; we have no evidence of the truth of the latter. With these two conditions reversed the nature of the problem may be entirely changed.

Moreover, a glacier, considered in its totality, is composed not of ice only, but partly of névé and partly of snow. Of the shearing force of the latter forms of aqueous matter we have as yet no information; that it must be greatly less than the shearing force of ice cannot for a moment be questioned. After all, Canon Moseley only presents us with a dilemma. Either the shearing of a glacier is not caused by gravitation, or the gravitation of a glacier does not produce a shearing motion. He has asked us to impale ourselves upon the one horn, but

what if we should prefer the other? Has it never occurred to him to examine the possibility of the latter alternative?

In asking this question I am not unaware that many distinguished physicists have made use of language involving an implicit recognition of the shearing hypothesis. Professor Tyndall, for instance, thus describes the origin of glacier motion:—'Even after it' (snow) 'has attained a compactness which would entitle it to be called ice, it is still capable of yielding more or less, as the snow yields, to pressure. When, therefore, a sufficient depth of the substance collects upon the earth's surface, the lower portions are squeezed out by the pressure of the upper ones, and if the snow rests upon a slope, it will yield principally in the direction of the slope and move downwards.'* Again, in that magnificent series of experiments in which he demonstrated the principle of fracture and regelation, the ice was sheared by crushing under the force of the hydraulic press, and reunited by the same agency. Admirable as these experiments were in a physical sense, one cannot help venturing the criticism that their mechanical significance has not yet been rendered fully apparent. Between the shearing forces and the hydraulic pressure which overcame them a certain relation existed. What proportion does that relation bear to the mechanical conditions of a glacier?

It is precisely because Canon Moseley is investigating the problem from this point of view that he is likely to contribute to its solution materials of great scientific value. And here I must finally call attention to his paper on the mechanical properties of ice, in the current number of the 'Philosophical Magazine.' The first part of the paper is occupied by a description of Schumacher's experiments on dilatation, which I have already sufficiently explained. The author then proceeds to give the details of a series of experiments upon the tenacity, crushing, and shearing of ice.

To determine its tenacity he subjected rods of ice to strains in the direction of their length, and increased the load until the rods were torn asunder. In eight experiments the tenacity was found to range from 74 lbs. to 116 lbs. per square inch.

To determine its crushing pressure he loaded vertical cylinders of ice 1½ inch in diameter, and ascertained that the pressure necessary to crush them was equivalent to 308·4 lbs. per square inch. Assuming ice to be of the same specific gravity as water, a strip one square inch in section and 710 feet high would have this weight. As no glacier is alleged to have

* 'Heat considered as a mode of Motion,' p. 183.

so great a depth, Mr. Moseley pertinently adds that this fact is an answer to the theory which attributes the descent of a glacier to the crushing of the ice at its base.

The experiments on shearing were made with an instrument somewhat similar to that described in his previous paper, but the results were remarkably different, the unit of shear, as determined by thirteen experiments, ranging from 98 to 119 lbs. per square inch. Mr. Moseley justly observes that *time* is an important element in the experiment, the shear which gave the smallest unit having required thirty-six minutes, the shear which gave the greatest unit having been effected in two. I am curious to know what weight would have sheared the ice if a *day* had been allowed for its operation, and what is the relation of time to crushing and tenacity.

It is characteristic of all these experiments that extraneous forces are brought to bear upon the substance submitted to our operations, and that conditions are thus introduced which may or may not obtain in the case of an actual glacier. It would throw great light upon our inquiry if we were to change this method of procedure, and simply to observe the deportment of masses of ice under the influence of no external forces but the gravitation of their own particles. As bearing upon this question, I venture to describe a simple experiment conducted during the last frost, in the prosecution of which I was indebted to the obliging assistance of my friend Mr. A. Follett Osler, F.R.S. A plank of ice 6 inches wide, and $2\frac{3}{8}$ inches in thickness, was sawn from the frozen surface of a pond, and supported at each end by bearers exactly 6 feet apart. The whole weight of the plank between the bearers could not have exceeded $37\frac{1}{2}$ lbs., and its cross section was nowhere less than 14 square inches. According to the views of Canon Moseley, shearing must surely have been impossible, yet what was the result? From the moment the plank was placed in position it began to sink, and continued to do so until it touched the surface over which it was supported. At the point of contact it appeared bent at a sharp angle, and was perfectly rigid in its altered form. The total deflection was 7 inches, which had been effected in about as many hours, under the influence of a thaw, during which the plank diminished very slightly in thickness. On observing the under-surface of the plank near the point of flexure, I noticed a number of very minute fissures extending a short distance into the ice, but they certainly were not sufficient to account for the flexure of the plank.

With this property of ice, viz. its power to change its form under strains produced by its own gravitation, combined with

the sliding movement demonstrated by Hopkins, we have, as it seems to me, adequate causes for glacier motion. What remains to be done is to connect the first of these causes with demonstrated principles of molecular physics, for which purpose many more experiments of the kind above mentioned will probably require to be undertaken.

Against the crawling theory, as an appreciable element in the problem, the verdict must, I think, be given, first, that it is highly improbable, secondly, that it is unnecessary. But whatever may be its fate, the services Canon Moseley has rendered to science by his contributions to the discussion, and by his investigation of the mechanical properties of ice, are beyond all question. To the recognition of those services I gladly unite the expression of my cordial thanks for the ready courtesy with which Canon Moseley has placed in the hands of an adverse critic all the materials for his essay.

The Lofoten Islands. By T. G. Bonney, M.A., F.G.S.

A paper without a single mountain ascent may seem out of place in the 'Alpine Journal.' That mine has this defect is due not so much to want of will as to want of time and favourable weather. I spent a week in the islands, and four out of the seven days were too cloudy and rainy to allow of distant excursions; the remaining period only sufficed for carrying into effect the purpose which had more especially led to my visit. Still, though without any stories of climbs, this paper may, I hope, be useful to future climbers; for in writing it, I shall do my best to give a clear idea of the topography of the islands, and to mention a few things that I should have been glad to know before my journey. Though the voyage along the western coast of Norway, which has been pleasantly described by Mr. Tyrwhitt in the third volume of this Journal, is annually made by many travellers, all of whom are enthusiastic in their praises of the Lofoten scenery, none, so far as I know, have attempted to describe its features at all minutely; so that in planning a trip to them one is sorely at a loss to know where one ought to stop and what is best worth seeing, or to get any very clear idea as to the general physical geography of the country.* The weird beauty of the scenery seems to have so

* Mr. Campbell's paper (vol. iv. p. 1) is even more brief than is wont on this one topic. I cannot mention this paper without expressing