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XIX. *On the Cause of the Motion of Glaciers.* *By* JAMES CROLL, *of the Geological Survey of Scotland**.

The generally accepted theory proved by the Rev. Canon Moseley *to be incorrect.*

SINCE the time that Professor Tyndall had shown that all the phenomena formerly attributed by Professor Forbes to plasticity could be explained upon the principle of regelation, discovered by Faraday, the viscous theory of glacier-motion has been pretty generally given up. The ice of a glacier is now almost universally believed to be, not a soft plastic substance, but a substance hard, brittle, and unyielding. The power that the glacier has of accommodating itself to the inequalities of its bed without losing its apparent continuity is referred to the property of regelation possessed by ice. All this is now plain; but what is it that impels the glacier forward is still a question under discussion. Various theories have been propounded regarding the cause of the descent of glaciers, all of which have been abandoned with the exception of that which attributes their descent to gravitation. But as the ice of the glacier descends with a differential motion, we have not only to explain what causes the glacier to slide on its bed, but also what displaces the particles of the ice over one another and alongside one another. What, then, is the force which *shears* the ice? The answer generally given is that gravitation alone is the force which does this; or, in other words, the mere weight of the ice is sufficient to overcome its cohesive force and to displace the particles over one another. The

* Communicated by the Author.

Rev. Canon Moseley has lately investigated this point, and has found that the amount of work performed on a glacier (assuming, of course, that the ice shears in the solid state) during its descent through a given space is enormously greater than the work of the weight of the glacier descending through that space. He has determined the amount of work performed by gravitation in the descent of a glacier, and the amount of internal work performed on the ice during the descent; and has found that, in respect to a glacier of the same uniform rectangular section and slope as the Mer de Glace at Les Ponts, and moving with the same uniform velocity, the aggregate work of the resistances which oppose themselves to its descent in a given time is about thirty-four times the work of the weight in the same time; consequently it is physically impossible that the mere weight alone of the glacier can be the cause of its descent.

The impression left on my mind after reading Canon Moseley's memoir in the Proceedings of the Royal Society for January 1869 was that, unless some very serious error could be pointed out in the mathematical part of his investigation, it would be hopeless to attempt to overturn his general conclusion as regards the received theory of the cause of the descent of glaciers, by searching for errors in the experimental data on which the conclusion rests. Had the result been that the actual shearing-force of ice is by twice, thrice, four times, or even five times too great to allow of a glacier shearing by its own weight, one might then hope that, by some more accurate method of determining the unit of shear than that adopted by Canon Moseley, his objection to the received theory of glacier-motion might be met; but when the unit of shear is found to be not simply by three times, four times, or even five times, but actually by thirty, forty, or fifty times too great, all our hopes of overturning his conclusion by searching for errors in this direction vanish, even although there are some points connected with his unit of shear that are not very satisfactory.

The ice of a glacier is in the hard, solid, and crystalline state. This is now generally admitted. Then, if the particles of the ice shear in this state, Canon Moseley's calculations show that the glacier cannot possibly descend by its weight only, as is generally supposed; and the generally received theory of glacier-motion must therefore be abandoned. I can perceive no way of escape from this conclusion.

I presume that few who have given much thought to the subject of glacier-motion have not had some slight misgivings in regard to the commonly received theory. There are some facts which I never could harmonize with this theory. For example, boulder-clay is a far looser substance than ice; its shearing-force must be very much less than that of ice; yet immense masses of boulder-clay will lie immoveable for ages on the slope of a hill so steep that one can hardly venture to climb it, while a glacier will come crawling down a valley which by the eye we could hardly detect to be actually off the level. Again, a glacier moves faster during the day than during the night, and about twice as fast during summer as during winter. Professor Forbes, for example, found that the Glacier des Bos near its lower extremity moved sometimes in December only 11·5 inches daily, while during the month of July its rate of motion sometimes reached 52·1 inches per day. Why such a difference in the rate of motion between day and night, summer and winter? The glacier is not heavier during the day than it is during the night, or during the summer than it is during the winter; neither is the shearing-force of the great mass of the ice of a glacier sensibly less during the day than during the night, or during the summer than during the winter; for the temperature of the great mass of the ice does not sensibly vary with the seasons. Then, if this is the case, gravitation ought to be as able to move a glacier during the night as during the day, or during the winter as during the summer. At any rate, if there should be any difference it ought to be but trifling. It is true that, owing to the melting of the ice, the crevices of the glacier are more gorged with water during summer than during winter; and this, as Professor Forbes maintains*, may tend to make the glacier move faster during the former season than during the latter. But the advocates of the regelation theory cannot conclude, with Professor Forbes, that the water favours the motion of the glacier by making the ice more soft and plastic. The melting of the ice, according to the regelation theory, cannot very materially aid the motion of the glacier.

The fact that the rate of motion of a glacier depends upon the amount of heat that the ice is receiving shows that heat in some way or other stands related as a cause to the motion of the glacier.

But the point under consideration is, If the ice of a glacier shears in the solid state, as is generally supposed, has Canon Moseley proved that a glacier cannot descend by its weight only? I have carefully read the interesting memoirs by Mr. Mathews and Mr. Ball in reply to Canon Moseley; and although I agree with the most of their remarks regarding the unsatisfactory nature of Mr. Moseley's own theory of glacier-motion, yet I am unable to perceive that any thing which they have advanced materially affects his general conclusion as regards the commonly received theory. If the ice of a glacier shears, nothing which I have yet

* Occasional Papers, pp. 166, 223.

seen advanced to the contrary can, so far as I perceive, overturn Mr. Moseley's conclusion, that the glacier cannot descend by its weight only. The interesting experiment described by Mr. Mathews*, of a plank of ice supported horizontally at each end being deflected in the middle without any weight being applied to the ice, does not appear to me to prove any thing either in favour of the generally received theory or against Canon Moseley's conclusion, —for this very simple reason, that whatever theory we may adopt as to the cause of the motion of glaciers, the deflection of the plank in the way described by Mr. Mathews *follows as a necessary consequence.* Although no weight was placed upon the plank, it does not necessarily follow that the deflection was caused by the weight of the ice alone; for, according to Canon Moseley's own theory of the motion of glaciers by heat, the plank ought to be deflected in the middle, just as it was in Mr. Mathews's experiment. A solid body, when exposed to variations of temperature, will expand and contract transversely as well as longitudinally. Ice, according to Canon Moseley's theory, expands and contracts by heat. Then if the plank expands transversely, the upper half of the plank must rise and the lower half descend. But the side which rises has to perform work against gravity, whereas the side which descends has work performed upon it by gravity; consequently more of the plank will descend than rise, and this will, of course, tend to lower or deflect the plank in the middle. Again, when the plank contracts, the lower half will rise and the upper half will descend; but as gravitation, in this case also, favours the descending part and opposes the rising part, more of the plank will descend than rise, and consequently the plank will be lowered in the middle by contraction as well as by expansion. Thus, as the plank changes its temperature, it must, according to Mr. Moseley's theory, descend or be deflected in the middle, step by step—and this not by gravitation alone, but chiefly by the motive power of heat. I do not, of course, mean to assert that the descent of the plank was thus actually caused by heat; but I assert that Mr. Mathews's experiment does not necessarily prove (and this is all that is required in the mean time) that gravitation alone was the cause of the deflection of the plank. Neither does this experiment prove that the ice was deflected without shearing; for although the weight of the plank was not sufficient to shear the ice, as Mr. Mathews, I presume, admits, yet Mr. Moseley would reply that the weight of the ice, assisted by the motive power of heat, was perfectly sufficient.

Had Mr. Mathews laid his plank horizontally across an inclined plane and fixed the two ends of the plank so as to prevent them

* Alpine Journal for February 1870; 'Nature' for March 24, 1870.

moving, everybody (whatever might be his theory as to the cause of the motion of glaciers) would at once admit that the middle of the plank (which, of course, was not fixed) would begin slowly to descend the incline in the manner that the ice of a glacier actually does, and that the plank, not being permitted to move at its ends, would become bent or deflected in the middle. Then, if everybody would admit that the plank would be deflected in the middle notwithstanding the friction of the ice on the inclined plane, and the diminished pressure of the weight of the ice in consequence of its resting on the slope, surely no one could conclude that, were the inclined plane removed and the plank suspended in the air by its two extremities, as in Mr. Mathews's experiment, it would not descend in the middle.

I shall now briefly refer to Mr. Ball's principal objections to Canon Moseley's proof that a glacier cannot shear by its weight alone. One of his chief objections is that Mr. Moseley has assumed the ice to be homogeneous in structure, and that pressures and tensions acting within it are not modified by the varying constitution of the mass. Although there is, no doubt, some force in this objection (for we have probably good reason to believe that ice will shear, for example, more easily along certain planes than along others), still I can hardly think that Canon Moseley's main conclusion can ever be materially affected by this objection. The main question is this, Can the ice of the glacier shear by its own weight in the way generally supposed? Now the shearing-force of ice, take it in whatever direction we may, so enormously exceeds that required by Mr. Moseley in order to allow a glacier to descend by its weight only, that it is a matter of indifference whether ice be regarded as homogeneous in structure or not. Mr. Ball objects also to Mr. Moseley's imaginary glacier lying on an even slope and in a uniform rectangular channel. Surely Mr. Ball does not suppose that a glacier would descend more easily in an irregular and broken channel having a variable slope and direction than it would do in a straight channel uniform in width and slope. And if he does not, why advance such an objection? Canon Moseley assumed, as he had a perfect right to do, that if the glacier could not descend by its weight in his imaginary channel, it could much less do so in its actual one.

That a relative displacement of the particles of the ice is involved in the motion of a glacier, is admitted, of course, by Mr. Ball; but he states that the amount of this displacement is but small, and that it is effected with extreme slowness. This may be the case; but if the weight of the ice be not able to overcome the mutual cohesion of the particles, then the weight of the ice cannot produce the required displacement, however small it may be. Mr. Ball then objects to Mr. Moseley's method of determining the unit of shear on this ground:—The shearing of the ice in a glacier is effected with extreme slowness; but the shearing in Canon Moseley's experiment was effected with rapidity; and although it required 75 lbs. to shear one square inch of surface in his experiment, it does not follow that 75 lbs. would be required to shear the ice if done in the slow manner in which it is effected in the glacier. "In short," says Mr. Ball, "to ascertain the resistance opposed to very slow changes in the relative positions of the particles, so slight as to be insensible at short distances, Mr. Moseley measures the resistance opposed to rapid disruption between contiguous portions of the same substance."

There is force in this objection; and here we arrive at a really weak point in Canon Moseley's reasoning. His experiments show that if we want to shear ice quickly a weight of nearly 120 lbs. is required; but if the thing is to be done more slowly, 75 lbs. will suffice*. In short, the number of pounds required to shear the ice depends to a large extent on the length of time that the weight is allowed to act; the longer it is allowed to act, the less will be the weight required to perform the work. "I am curious to know," says Mr. Mathews when referring to this point, "what weight would have sheared the ice if a *day* had been allowed for its operation." I do not know what would have been the weight required to shear the ice in Mr. Moseley's experiments had a day been allowed; but I feel pretty confident that, should the ice remain unmelted, and sufficient time be allowed, shearing would be produced without the application of any weight whatever. There are no weights placed upon a glacier to make it move, and yet the ice of the glacier shears. If the shearing is effected by weight, the only weight applied is the weight of the ice; and if the weight of the ice makes the ice shear in the glacier, why may it not do the same thing in the experiment? Whatever may be the cause which displaces the particles of the ice in a glacier, they, as a matter of fact, are displaced without any weight being applied beyond that of the ice itself; and if so, why may not the particles of the ice in the experiment be also displaced without the application of weights? Allow the ice of the glacier to take its own time and its own way, and the particles will move over each other without the aid of external weights, whatever may be the cause of this; well, then, allow the ice in the experiment to take its own time and its own way, and it will probably do the same thing. There is something here unsatisfactory. If, by the unit of shear, be meant the pressure in pounds that must be applied to the ice to break the connexion of one square inch of two surfaces frozen together and

* Philosophical Magazine for January 1870, p. 8; Proceedings of the Royal Society for January 1869.

cause the one to slip over the other, then the amount of pressure required to do this will depend upon the time you allow for the thing being done. If the thing is to be done rapidly, as in some of Mr. Moseley's experiments, it will take, as he has shown, a pressure of about 120 lbs.; but if the thing has to be done more slowly, as in some other of his experiments, 75 lbs. will suffice. And if sufficient time be allowed, as in the case of glaciers, the thing may be done without any weight whatever being applied to the ice, and, of course, Mr. Moseley's argument, that a glacier cannot descend by its weight alone, falls to the ground. But if, by the unit of shear, be meant not the *weight* or *pressure* necessary to shear the ice, but the amount of *work* required to shear a square inch of surface *in a given time or at a given rate,* then he might be able to show that in the case of a glacier (say the Mer de Glace) the work of all the resistances which are opposed to its descent at the *rate* at which it is descending is greater than the work of its weight, and that consequently there must be some cause, in addition to the weight, urging the glacier forward. But then he would have no right to affirm that the glacier would not descend by its weight only; all that he could affirm would simply be that it could not descend by its weight alone at the *rate* at which it is descending.

Mr. Moseley's unit of shear, however, is not the amount of *work* performed in shearing a square inch of ice in a given time, but the amount of *weight* or *pressure* requiring to be applied to the ice to shear a square inch. But this amount of pressure depends upon the length of time that the pressure is applied. Here lies the difficulty in determining what amount of pressure is to be taken as the real unit. And here also lies the radical defect in Canon Moseley's result. Time as well as pressure enters as an element into the process. The key to the explanation of this curious circumstance will, I think, be found in the fact to which reference has already been made, viz. that the rate at which a glacier descends depends in some way or other upon the amount of heat that the ice is receiving. This fact shows that heat has something to do in the shearing of the ice of the glacier. But in the communication of heat to the ice *time* necessarily enters as an element. There are two different ways in which heat may be conceived to aid in shearing the ice: (1) we may conceive that heat acts as a force along with gravitation in producing displacement of the particles of the ice; or (2) we may conceive that heat does not act as a force in pushing the particles over each other, but that it assists the shearing processes by diminishing the cohesion of the particles of the ice, and thus allowing gravitation to produce displacement. The former is the function attributed to heat in Canon Moseley's theory of glacier-motion; the latter is the function attributed to heat in the theory of glacier-motion which I ventured to advance some time ago*. It results, therefore, from Canon Moseley's own theory, that the longer the time that is allowed for the pressure to shear the ice, the less will be the pressure required; for, according to his theory, a very large proportion of the displacement is produced by the motive power of heat entering the ice; and, as it follows of course, other things being equal, the longer the time during which the heat is allowed to act, the greater will be the proportionate amount of displacement produced by the heat; consequently the less will require to be done by the weight applied. In the case of the glacier, Mr. Moseley concludes that at least thirty or forty times as much work is done by the motive power of heat in the way of shearing the ice as is done by mere pressure or weight. Then, if sufficient time be allowed, why may not far more be done by heat in shearing the ice in his experiment than by the weight applied? In this case how is he to know how much of the shearing is effected by the heat and how much by the weight. If the greater part of the shearing of the ice in the case of a glacier is produced, not by pressure, but by the heat which necessarily enters the ice, it would be inconceivable that in his experiments the heat entering the ice should not produce, at least to some extent, a similar effect. And if a portion of the displacement of the particles is produced by heat, then the weight which is applied cannot be regarded as the measure of the force employed in the displacement, any more than it could be inferred that the weight of the glacier is the measure of the force employed in the shearing of it. If the weight is not the entire force employed in shearing, but only a part of the force, then the weight cannot, as in Mr. Moseley's experiment, be taken as the measure of the force.

How, then, are we to determine what is the amount of force required to shear ice? in other words, how is the unit of shear to be determined? If we are to measure the unit of shear by the weight required to produce displacement of the particles of the ice, we must make sure that the displacement is wholly effected by the weight. We must be certain that heat does not enter as an element in the process. But if time be allowed to elapse during the experiment, we can never be certain that heat has not been at work. It is impossible to prevent heat entering the ice. We may keep the ice at a constant temperature, but this would not prevent heat from entering the ice and producing molecular work. True that, according to Moseley's theory of glacier-motion, if the temperature of the ice be not permitted to *vary*, then no displacement of the particles can take place from

* Philosophical Magazine for March 1869.

the influence of heat; but according to the molecular theory of glacier-motion which I have adopted, heat will aid the displacement of the particles whether the temperature be kept constant or not. In short, it is absolutely impossible in our experiments to be certain that heat is not in some way or other concerned in the displacement of the particles of the ice. But we can shorten the time, and thus make sure that the amount of heat entering the ice during the experiments is too small to affect materially the result. We cannot in this case say that all the displacement has been effected by the weight applied to the ice, but we can say that so little has been done by heat that, practically, we may regard it as all done by the weight.

This consideration, I trust, shows that the unit of shear adopted by Canon Moseley in his calculations is not too large. For if in half an hour, after all the work that may have been done by heat, a pressure of 75 lbs. is still required to displace the particles of one square inch, it is perfectly evident that if no work had been done by heat during that time, the force required to produce the displacement could not have been less than 75 lbs. It might have been more than that; but it could not have been less. Be this, however, as it may, in determining the unit of shear we cannot be permitted to prolong the experiment for any considerable length of time, because the weight under which the ice might then shear could not be taken as the measure of the force which is required to shear ice. By prolonging the experiment we might possibly get a unit smaller than that required by Canon Moseley for a glacier to descend by its own weight. But it would be just as much begging the whole question at issue, to assume that, because the ice sheared under such a weight, a glacier might descend by its weight alone, as it would be to assume that, because a glacier shears without a weight being placed upon it, the glacier descends by its weight alone.

But why not determine the unit of shear of ice in the same way as we would the unit of shear of any other solid substance, such as iron, stone, or wood? If the shearing-force of ice be determined in this manner, it will be found to be by far too great to allow of the ice shearing by its weight alone. We shall be obliged to admit either that the ice of the glacier does not shear (in the ordinary sense of the term), or if it does shear, that there must, as Canon Moseley concludes, be some other force in addition to the weight of the ice urging the glacier forward.

Physical objections to the Rev. Canon Moseley's *own theory.*

Although Canon Moseley has thus so ably and so successfully shown the insufficiency of the generally received theory of the cause of the descent of glaciers, he has, however, I venture to think, not been so fortunate in his attempt to establish a theory of his own. And I cannot help thinking that the influence which his remarkable communication to the Royal Society, on the impossibility of the descent of glaciers by their weight alone, would have had on the minds of physicists, has been much impaired by the prominence which he has since been giving to a theory which few, I fear, will ever be able to accept. Whatever may be the fate which awaits the generally accepted theory of the cause of glacier-motion, his own theory seems to be beset by difficulties of a physical nature which will require to be removed before he can expect that it will be received by physicists in general.

Most of these difficulties have already been noticed and discussed by Professor Forbes, Mr. Mathews, Mr. Ball, and others. I shall therefore only briefly allude to a few of those that more particularly bear on some points which have not already been sufficiently discussed.

Canon Moseley has shown that the mere weight of the ice is wholly insufficient to overcome the cohesion of the crystalline particles, so as to break their connexion and cause them to be displaced one over the other. This point I regard as fully established. It is implied in the generally received theory, that, in the descent of a glacier, owing to differential motion the cohesion of the particles of the ice is broken, and that these solid particles are forced over one another and alongside one another. Mr. Moseley then concludes that it follows, as a necessary consequence, that there must be some other force, in addition to the weight of the ice, pushing the glacier forward. Here lies the fundamental error. He has not proved that in the descent of the glacier the connexion of the solid particles of the ice has to be broken. True, the ice moves with a differential motion, and, as a necessary consequence, the particles are displaced over each other. Two particles separate, and the one moves past the other; but the point to be determined is this:—were the two particles at the moment when separation took place both in the hard crystalline and solid state? Canon Moseley does not prove this; he merely assumes it to be the case; but it must be proved to be the case, not assumed to be so, before he can conclude that it *necessarily* follows that in the descent of the glacier some force in addition to the weight of the ice is required to push the glacier forward. Certainly he is warranted in concluding that it necessarily follows that the generally received theory is incorrect, because in this theory it is assumed that the particles shear in the solid state. He would be warranted in saying to those who believe in the generally received theory, "You assume with me that in the descent of a glacier the cohesion of the solid particles of the ice has to be overcome and the one particle forced past the other. Then you must be wrong when you assert that the glacier descends by its weight only; for, as I have demonstrated, the mere weight of the glacier alone is not sufficient to do this." Canon Moseley has not, however, proved that the glacier cannot absolutely descend by its weight alone; he has only proved that *if* the glacier shears in the way that it is generally supposed to do, it cannot descend by its weight alone. Had it been established that the ice of the glacier shears in the way that it is generally supposed to do, Mr. Moseley's results would leave us no other alternative than to conclude that there must actually be some other cause in addition to the weight of the glacier impelling it forward; and we should be obliged to seek in heat or in something else for this additional impelling power.

I presume that Canon Moseley has not duly considered this point, and that consequently he has been led to the conclusion that, if his late remarkable results be received (which no doubt they will ere long), we shall then be obliged to adopt his own theory of glacier-motion, or some other similar theory which calls in the aid of forces more powerful than that of gravitation to impel the glacier downwards. That he supposes that we are forced to this alternative is, I think, apparent from the way in which he has lately introduced his theory. "The ice of a glacier," he says, "behaves itself in its descent exactly as the lead did in my experiment. The Mer de Glace moves faster by day than by night. Its mean daily motion is twice as great during the six summer as during the six winter months. The connexion between its rate of motion and the external temperature is most remarkable. It has been carefully observed, and the results, as recorded by Professor Forbes, leave no doubt of the fact, that no change of external mean temperature is unaccompanied by a corresponding change of glacier-motion. From this it follows that the two are either dependent on some common cause, or that the one set of changes stands in the relation of a cause to the other. That both sets of phenomena (the changes of the sun's heat and the changes of glacier-motion) should be due to some common independent cause seems impossible. *We are forced, therefore, on the conclusion that one is caused by the other.* And as the changes in the glacier-motion cannot cause the changes of solar heat, it *must* be the changes of solar heat which cause the changes of glacier-motion"*.

It is certainly true that the fact that the glacier moves more rapidly during the day than during the night, and during summer than during winter, proves that there must be some physical connexion between the heat of the sun and the motion of the

* Proceedings of the Bristol Naturalists' Society, vol. iv. p. 38 (new series).

glacier. It is also true that the changes of the sun's heat and the changes of glacier-motion cannot be due to a common cause. And it is admitted that the changes in the glacier-motion must in some way or other be dependent upon the changes in the sun's heat. Further, it is admitted that the changes in the sun's heat are the cause of the *changes* in glacier-motion; but it entirely depends upon the meaning which we attach to the term "*cause*" whether it will be admitted that the sun's heat is the cause of the motion of the glacier. If by cause of the motion of the glacier be meant every thing without which the glacier would not descend, then it is admitted that heat is a cause of the motion of the glacier. But if by cause of the motion of the glacier be meant the energy or power that impels the glacier forward (and this is the meaning which Mr. Moseley seems to attach to the term), then we are not compelled logically to admit that heat is the *cause* of the motion of the glacier; for it may only be a necessary *condition* to the operation of the cause, whatever that cause may be, which impels the glacier forward. The absence of a *necessary condition* will as effectually prevent the occurrence of an effect as the absence of the *cause* itself. It does not follow that, because a glacier will not move without heat, heat is necessarily the cause of its motion. Gravitation may be the cause, and heat only a condition.

The fundamental condition in Mr. Moseley's theory of the descent of solid bodies on an incline is, not that heat should maintain these bodies at a high temperature, but that the temperature should vary. The rate of descent is proportionate, not simply to the amount of heat received, but to the extent and frequency of the variations of temperature. As a proof that glaciers are subjected to great variations of temperature, he adduces the following:—"All alpine travellers," he says, "from De Saussure to Forbes and Tyndall, have borne testimony to the intensity of the solar radiation on the surfaces of glaciers. 'I scarcely ever,' says Forbes, 'remember to have found the sun more piercing than at the Jardin.' This heat passes abruptly into a state of intense cold when any part of the glacier falls into shadow by an alteration of the position of the sun, or even by the passing over it of a cloud" *.

Mr. Moseley is here narrating simply what the traveller feels, and not what the glacier experiences. The traveller is subjected to great variations of temperature; but there is no proof from this that the glacier experiences any changes of temperature. It is rather because the temperature of the glacier is not affected by the sun's heat that the traveller is so much chilled when the

* Proceedings of the Bristol Naturalists' Society, vol. iv. p. 37 (new series).

sun's rays are cut off. The sun shines down with piercing rays and the traveller is scorched; the glacier melts on the surface, but it still remains "cold as ice." The sun passes behind a cloud or disappears behind a neighbouring hill; the scorching rays are then withdrawn, and the traveller is now subjected to radiation on every side from surfaces at the freezing-point.

It is also a necessary condition in Mr. Moseley's theory that the heat should pass easily into and out of the glacier; for unless this were the case sudden changes of temperature could produce little or no effect on the great mass of the glacier. How, then, is it possible that during the heat of summer the temperature of the glacier could vary much? During that season, in the lower valleys at least, every thing, with the exception of the glacier, is above the freezing-point; consequently when the glacier goes into the shade there is nothing to lower the ice below the freezing-point; and as the sun's rays do not raise the temperature of the ice above the freezing-point, the temperature of the glacier must therefore remain unaltered during that season. It therefore follows that, instead of a glacier moving more rapidly during the middle of summer than during the middle of winter, it should, according to Moseley's theory, have no motion whatever during summer.

The following, written fifteen years ago by Professor Forbes on this very point, is most conclusive:—"But how stands the fact? Mr. Moseley quotes from De Saussure the following *daily ranges* of the temperature of the air in the month of July at the Col du Géant and at Chamouni, between which points the glacier lies:

At the Col du Géant . .	$\overset{\circ}{4}$·257 Reaumur.
At Chamouni	10·092 „

And he assumes 'the same mean daily variation of temperature to obtain throughout the length' [and depth ?] 'of the Glacier du Géant which De Saussure observed in July at the Col du Géant.' But between what limits does the temperature of the air oscillate? We find, by referring to the third volume of De Saussure's Travels, that the mean temperature of the coldest hour (4 A.M.) during his stay at the Col du Géant was 33°·03 Fahrenheit, and of the warmest (2 P.M.) 42°·61 F. So that even upon that exposed ridge, between 2000 and 3000 feet above where the glacier can be properly said to commence, the air does not, on an average of the month of July, reach the freezing-point at any hour of the night. Consequently the *range of temperature attributed to the glacier is between limits absolutely incapable of effecting the expansion of the ice in the smallest degree*"*.

* Phil. Mag. S. 4. vol. x. p. 303.

Again, during winter, as Mr. Ball remarks, the glacier is completely covered with snow and thus protected both from the influence of cold and of heat, so that there can be nothing either to raise the temperature of the ice above the freezing-point, or to bring it below that point; and consequently the glacier ought to remain immoveable during that season also.

"There can be no doubt, therefore," Mr. Moseley states, "that the rays of the sun, which in those alpine regions are of such remarkable intensity, find their way into the depths of the glacier. They are a *power*, and there is no such thing as the loss of power. The mechanical work which is their equivalent, and into which they are converted when received into the substance of a solid body, accumulates and stores itself up in the ice under the form of what we call elastic force or tendency to dilate, until it becomes sufficient to produce actual dilatation of the ice in the direction in which the resistance is weakest, and by its withdrawal to produce contraction. From this expansion and contraction follows of necessity the descent of the glacier"*. When the temperature of the ice is below the freezing-point, the rays which are absorbed will, no doubt, produce dilatation; but during summer, when the ice is not below the freezing-point, no dilatation can possibly take place. All physicists, so far as I am aware, agree that the rays that are then absorbed go to melt the ice and not to expand it. But to this Mr. Moseley replies as follows:—"To this there is the obvious answer that radiant heat does find its way into ice as a matter of common observation, and that it does not melt it except at its surface. Blocks of ice may be seen in the windows of ice-shops with the sun shining full upon them, and melting nowhere but on their surfaces. And the experiment of the ice-lens shows that heat may stream through ice in abundance (of which a portion is necessarily stopped in the passage) without melting it, except on its surface." But what evidence has Mr. Moseley to conclude that if there is no melting of the ice in the interior of the lens there is a portion of the rays "necessarily stopped" in the interior? It will not do to assume a point so much opposed to all that we know of the physical properties of ice as this really is. Has Mr. Moseley, after accurately determining the amount of work performed in melting the ice of his lens during any given time, found it to fall short of the amount of work which ought to have been performed by the heat absorbed during that given time? If he has done this in a manner that can be relied upon, then he has some warrant to conclude that there is a portion of the rays stopped which goes to perform work different from that of melting

* Proceedings of the Bristol Naturalists' Society, vol. iv. p. 39 (new series).

the ice, and that this work in all probability is the expansion of the ice. Or has he determined directly that his lens, after reaching the temperature which is considered to be the melting-point of ice, actually continued to expand as the rays passed into it? It is absolutely essential to Mr. Moseley's theory of the motion of glaciers, during summer at least, that ice should continue to expand after it reaches the melting-point; and it is therefore incumbent upon him to afford us some evidence that such is the case; or he need not wonder that we cannot accept his theory, because it demands of us the adoption of a conclusion so contrary to all our previous conceptions. But, as a matter of fact, it is not strictly true that when rays pass through a piece of ice there is no melting of the ice in the interior. Experiments made by Professor Tyndall show the contrary*.

There is, however, one fortunate circumstance connected with Canon Moseley's theory. It is this; its truth can be easily tested by direct experiment. The ice, according to this theory, descends not simply in virtue of heat, but in virtue of *change of temperature*. Try, then, Hopkins's famous experiment, but keep the ice at a *constant temperature*; then, according to Moseley's theory, the ice will not descend. Or try Mr. Mathews's experiment, but keep the ice-plank at a constant temperature, and the plank ought not to sink in the middle. But let it be observed that although the ice under this condition should descend (as there is little doubt but it would), it would show that Mr. Moseley's theory of the descent of glaciers is incorrect, but it would not in the least degree affect the conclusions which he has lately arrived at in regard to the generally received theory of glacier-motion. It would not prove that the ice sheared, in the way generally supposed, by its weight only. It might be the heat, after all, entering the ice, which accounted for its descent, although gravitation (the weight of the ice) might be the impelling cause.

The present state of the question.

The condition which the perplexing question of the cause of the descent of glaciers has now reached seems to be something like the following. The ice of a glacier is not in a soft and plastic state, but is solid, hard, brittle, and unyielding. It nevertheless behaves in some respects in a manner very like what a soft and plastic substance would do if placed in similar circumstances, inasmuch as it accommodates itself to all the inequalities of the channel in which it moves. The ice of the glacier, though hard and solid, moves with a differential motion; the particles of the ice are displaced over each other, or, in other words, the ice shears as it descends. It had been concluded that

* See Philosophical Transactions, December 1857.

the mere weight of the glacier was sufficient to shear the ice. Canon Moseley has investigated this point, and shown that it is not. He has found that for a glacier to shear in the way that it is supposed to do, it would require a force some thirty or forty times as great as the weight of the glacier. Consequently, for the glacier to descend, a force in addition to that of gravitation is required. What, then, is this force? It is found that the rate at which the glacier descends depends upon the amount of heat which it is receiving. This shows that the motion of the glacier is in some way or other dependent upon heat. Is heat, then, the force we are in search of? The answer to this, of course, is, since heat is a force necessarily required, we have no right to assume any other till we see whether or not heat will suffice. In what way, then, does heat aid gravitation in the descent of the glacier? In what way does heat assist gravitation in the shearing of the ice? There are two ways whereby we may conceive the thing to be done: the heat may assist gravitation to shear, by pressing the ice forward, or it may assist gravitation by diminishing the cohesion of the particles, and thus allowing gravitation to produce motion which it otherwise could not produce. Every attempt which has yet been made to explain how heat can act as a force in pushing the ice forward, has failed. The fact that heat cannot expand the ice of the glacier may be regarded as a sufficient proof that it does not act as a force impelling the glacier forward; and we are thus obliged to turn our attention to the other conception, viz. that heat assists gravitation to shear the ice, not by direct pressure, but by diminishing the cohesive force of the particles, so as to enable gravitation to push the one past the other. But how is this done? Does heat diminish the cohesion by acting as an expansive force in separating the particles? Heat cannot do this, because it cannot expand the ice of a glacier; and besides, were it to do this, it would destroy the solid and firm character of the ice, and the ice of the glacier would not then, as a mass, possess the great amount of shearing-force which observation and experiment show that it does. In short it is because the particles of the ice are so firmly fixed together at the time that the glacier is descending, that we are obliged to call in the aid of some other force in addition to the weight of the glacier to shear the ice. Heat does not cause displacement of the particles by making the ice soft and plastic; for we know that the ice of the glacier is not soft and plastic, but hard and brittle. The shearing-force of the ice of the moving glacier is found to be by at least from thirty to forty times too great to permit of the ice being sheared by the mere force of gravitation; how, then, is it that gravitation, without the direct assistance of any other force, can manage to shear the ice? Or to put the question under another form: heat does not reduce the shearing-force of the ice of a glacier to something like 1·3193 lb. per square inch of surface, the unit required by Mr. Moseley to enable a glacier to shear by its weight; the shearing-force of the ice, notwithstanding all the heat received, still remains at about 75 lbs.; how, then, can the glacier shear without any other force than its own weight pushing it forward? This is the fundamental question; and the true answer to it must reveal the mystery of glacier-motion. We are compelled in the present state of the problem to admit that glaciers do descend with a differential motion without any other force than their own weight pushing them forward; and yet the shearing-force of the ice is actually found to be thirty or forty times the maximum that would permit of the glacier shearing by its weight only. The explanation of this apparent paradox will remove all our difficulties in reference to the cause of the descent of glaciers.

There seems to be but one explanation (and it is a very obvious one), viz. that the motion of the glacier is *molecular*. The ice descends molecule by molecule. The ice of a glacier is in the hard crystalline state, but it does not descend in this state. Gravitation is a constantly acting force; if a particle of the ice lose its shearing-force, though but for the moment, it will descend by its weight alone. But a particle of the ice will lose its shearing-force for a moment if the particle loses its crystalline state for the moment. The passage of heat through ice, whether by conduction or by radiation, in all probability is a molecular process; that is, the form of energy termed heat is transmitted from molecule to molecule of the ice. A particle takes the energy from its neighbour A on the one side and hands it over to its neighbour B on the opposite side. But the particle must be in a different state at the moment it is in possession of the energy from what it was before it received it from A, and from what it will be after it has handed it over to B. Before it became possessed of the energy, it was in the crystalline state—it was ice; and after it loses possession of the energy it will be ice; but at the moment that it is in possession of the passing energy is it in the crystalline or icy state? If we assume that it is not, but that in becoming possessed of the energy, it loses its crystalline form and for the moment becomes water, all our difficulties regarding the cause of the motion of glaciers are removed*. We know that the ice of a glacier in the mass cannot become possessed of energy in the form of heat without becoming fluid; may not the same thing hold true of the ice particle?

* See Phil. Mag. for March 1869, p. 201.

The alleged limit to the thickness of a glacier.

In his memoir "On the Mechanical Properties of Ice," published in the Philosophical Magazine for January 1870, Canon Moseley arrives at a conclusion in regard to the crushing of ice to which I am unable, without some qualifications, to agree. In his experiments ice was crushed under a pressure of 308·4 lbs. on the square inch, and he concludes that if a glacier is over 710 feet in thickness the ice at the under surface must be crushed by the incumbent weight. Professor Phillips also made some experiments on the crushing of ice, and he came to the conclusion that the height of a crushing column of ice is between 1000 and 1500 feet, and concluded also that if a glacier were to exceed this in thickness the ice would lose its solidity*. Whether the height of a crushing column of ice be 710, or 1000, or 1500 feet is of no consequence whatever as regards the possible thickness of a glacier. No doubt a piece of ice solidified not under pressure would be crushed to powder were it placed under a glacier 1000 feet in thickness or so; but after being crushed it would resolidify, and would then probably be able to sustain a pressure of 2000 feet of ice. This follows as a necessary consequence from the property of regelation. There is as yet, so far as I am aware, no known limit to the amount of pressure which ice may sustain. There probably is a limit; but what that limit is has not yet been determined. Canon Moseley says that "there is no glacier alleged to have so great a depth as 710 feet." The Humboldt glacier in North Greenland, according to Dr. Kane, has a depth of more than three times 710 feet. And Dr. Heyes found in Baffin's Bay icebergs (which are just pieces broken off the ends of glaciers) aground in about half a mile of water. And on the antarctic continent we have reasons for believing that the ice is in some places over a mile in thickness†.

XX. *On the Molecular Movements and Magnetic Changes in Iron &c. at different Temperatures. By* G. Gore, *F.R.S.*‡

R. W. FOX§ has shown that cast iron in the melted state produces little or no magnetic effect upon a delicately poised magnetic needle placed near it during its cooling, solidification, and subsequent further cooling, until the solid metal acquires "a cherry-red colour;" it then suddenly attracts the needle with great energy. Gilbert had also many years before

* Paper on Glacial Striation read before the Geological Section of the British Association, 1865.

† Geological Magazine for June 1870, p. 276.

‡ Communicated by the Author.

§ Philosophical Magazine, vol. vii. (1835) p. 388.