during our ascent, though we had read that on this mountain it refused to act. The aneroid simply confirmed the previously ascertained height of the peak.

The whole excursion was a most charming one, both from the magnificence of the panorama spread beneath us, and from the wonders and beauties of the scenery more immediately surrounding the Jökull. Though the actual height above the sea level is small compared with the Alps, yet the mountain rises direct from the sea, there is all the novelty of traversing new ground, and after mountaineering in any other district you have the pleasant change of tent life, and a *cuisine* entirely and amply supplied by your gun, if you care for sport. The people are most hospitable, and if tents, a cooking apparatus, and sea biscuits are taken from England, everything else that one can want is quite as well, and in many instances more cheaply procured at Reykjavik, where a steamer goes once a month.

Observations of Canon Moseley on Mr. Mathews's Account of his Theory of Glacier Motion.

THE phenomena of glacier motion belong, it seems to me, rather to mechanical philosophy than physics. It is therefore from the point of view of the former science that I have endeavoured to investigate them.

It is a well-known principle of mechanical philosophy, that to give motion to a body, or system of bodies, the aggregate *work* of the forces which tend to move it must exceed the aggregate *work* of the resistances opposed to its motion. If, therefore, a glacier descend by its weight only, the work of its weight in descending through any space must equal the aggregate work of the shearing, crushing (if any), and friction which its mass undergoes while descending through that space. It is of course impossible to represent these conditions mathematically in respect to an actual glacier having an irregular channel and a variable slope and direction; but in respect to an imaginary one having a constant direction and a uniform channel and slope, it is possible. I have so represented them,* and have calculated 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 its weight in a given time, and the aggregate work of the resistances which oppose themselves to its descent in the same time. And I have found that the work of the weight so determined, instead of being greater, is only about $\frac{1}{35}$th as great as

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* *Phil. Mag.*, May 1869.

the work of the resistances; so that it is impossible the glacier should descend by its weight only, with those resistances opposed to its descent.

The imaginary glacier to which this computation applies differs from all actual glaciers in these respects, that the actual glacier is not straight or of a uniform section or slope. In all these respects, the resistance to the descent of the actual glacier is greater than to the imaginary one. But, this being the case, since in the imaginary glacier the weight is found to be insufficient to cause it to descend, much more must it be so in the actual glacier. This conclusion, if it be valid, is fatal to every theory which attributes the descent of a glacier to its weight only. Mr. Mathews has quoted, with reference to it, a saying of Professor Huxley, to the effect that 'the flour produced by the mathematical mill depends mainly upon the grain put into it.' The mill, in reference to my argument, is, I suppose, the mathematical formula at which I have arrived, the accuracy of which has not (so far as I know) been impugned; the wheat represents the conditions of the problem or its data, which, if they be true, being substituted for the symbols of the formula, yield a true result, as good wheat yields good flour. Now the data of the problem are the velocities of different points in the surface ice, observed by Professor Tyndall at Les Ponts, and the velocities at different altitudes of the side ice observed by him near the Tacul. There can be no question as to the accuracy of the first set of observations, but the second were made under circumstances of considerable difficulty and some danger. It is most desirable that they should be repeated at more leisure than Professor Tyndall could command and with more precautions. It is, moreover, desirable that at whatever point the velocity of the side ice* is observed, that of the surface ice should also be observed. No conceivable error in these data can, however, account for the enormous disproportion which is shown to exist between the amount of the force of gravity which has hitherto been supposed to cause glaciers to descend, and the resistances that force has to overcome.

Among these resistances I have reckoned those of the sides and bottom of the channel to be as great as though the ice were frozen to them; and, considering what are the obstacles in the actual glacier from projecting rocks, bends in its direc-

* The velocity of the deep ice in the centre of the glacier might perhaps be found if a boring were made there and filled at different depths with stones of different kinds. If these stones could be found again afterwards, the distances they had travelled would show the motion of the glacier at the depth at which each was deposited in the interval.

tion, and frequent throttlings, the assumption of a resistance of the imaginary channel equal to that of the ice being frozen to it, is not perhaps unreasonable. The result is not, however, practically affected by throwing the resistances of the bottom and sides of the channel wholly out of the computation. If they be conceived to be perfectly smooth, and if the ice be assumed not to adhere to them at all, the work of the weight will be found* still to be only $\frac{1}{47}$th of that necessary to overcome the work of the resistances. The fact is, that the *differential* motion of the glacier is by far the greatest part of its motion ($\frac{13}{14}$ths according to Forbes†), and the aggregate work of the forces opposed to the differential motion is by far the greatest part of the work.

Neither will any possible error in my assumed value of the unit (75 lbs.) of shear in ice affect (as Mr. Mathews thinks possible) the general result at which I have arrived. If the unit of shear were only one-tenth that my experiments gave, the force of gravity would still be less than one-fifth of that necessary to bring the glacier down. My first experiments on shear were made in a high temperature of the air. I have since repeated them in a low temperature. I could detect no perceptible shearing of a prism of ice when subjected through the whole of a cold night to a pressure which in the heat of the day would readily have sheared it across. As the temperature of glacier ice is not above freezing, we may therefore conclude that its unit of shear is higher than that which I obtained by experiments on a hot day. Mr. Mathews seems to think it necessary to the descent of a glacier on my theory, that luminous heat should not only enter it to a greater or less depth, but penetrate to its very bottom. That is not, however, the case. In the same way as by injecting a stream into a quiescent body of water motion is communicated to the whole of it, so it is ascertained by the experiments of M. Tresca, that by putting in motion parts of certain solids (and ice among the number) differential motion is communicated to other parts more or less distant from them. It is sufficient, therefore, that the ice of a glacier should be put in motion for a certain depth (greater or less) below its surface, to communicate a differential motion to the whole of it. The luminous rays need only penetrate to that depth, whatever it may be. I must have imperfectly explained myself, or Mr. Mathews would not have arrived at the conclusion that on my theory the density of the ice of a glacier must be increased 400 times for it to descend by its weight only. It

* *Phil. Mag.*, May 1869. By making U_2 and U^3 in formula 9 each $=0$.

† 'Occasional Papers,' p. 74.

would, in point of fact, so descend by its weight alone, if its density were increased fifty-five times.*

Some experiments which I have made on the modulus of elasticity of rods of ice in a freezing temperature show it to be exceedingly elastic but exceedingly brittle, and to have (as Mr. Osler found it) a great tendency when deflected to take a *set*. But if (on what seems to be Mr. Mathews's hypothesis) we are to explain the differential motion of a glacier by this experiment, we must assume the bending of successive subjacent layers, parallel to the surface, to take place in the planes of those layers. That Mr. Osler's experiment might offer an analogy to the bending of these layers, his plank of ice should therefore have been placed on its edge, and not flatwise, between its supports. Indeed, looking at the proportion of the length to the width of a glacier, and considering that the bending (if there be any such bending) must, to make the analogy complete, be in the direction of its length, the plank of ice should rather have been placed vertically on its end than on its side edge, and its deflexion have taken place in the direction of its length, and exhibited itself in lines curved downwards like the dirt-bands in glaciers. And all these phenomena should have continued to exhibit themselves when the plank of ice resting on its end was inclined from a vertical position to an inclination of 4° 52′; and, lastly, it should be possible to be shown that the resistances overcome by the weight of the plank (which include with longitudinal resistances of shearing other transverse ones proper to bending) satisfy the inexorable condition that the aggregate of their work in deflecting is less than the work of the weight of the plank; which it is impossible they should, as the work of the shearing alone exceeds it.

In concluding these remarks on Mr. Mathews's paper, I am desirous to acknowledge very cordially the ability and courtesy with which he has placed my theory before your readers. There are some points, however, in my theory to which I attach much importance, which he has not yet stated. I enclose therefore a paper read at Christmas last before the Bristol Philosophical Society, in which I have endeavoured to state it under a popular form, and which contains incidentally my answers to other objections of Mr. Mathews than those to which in this paper I have adverted.

* By referring to my paper (*Phil. Mag.*, May 1869) it will be seen (Equation 10) that the unit of shear which would allow a glacier to descend is 1·3489 lb., whereas the actual unit of shear ice is 75 lbs. The density of ice whose weight would be sufficient to cause the descent is thus shown (Equation 8) to be $\frac{75}{1\cdot 3489}$, or 55 times that of ice.