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W. Hopkins Esq. M.A. F.R.S., &c

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ing a neutral acetate of lime at a temperature approaching that of the boiling-point by chalk alone, but that they require to add milk of lime in order to overcome the acid reaction. Blondlot has deduced the inference from his experiment, that the stomach owes its acid reaction to the presence of an acid phosphate of lime; but as the experiments now detailed do not coincide with those of the French physiologist, it is sufficiently obvious that they do not support him in his conclusions.

[To be continued.]

XLIX. *On the Mechanism of Glacial Motion. Fourth Letter.* *By* W. Hopkins, *Esq., M.A., F.R.S., &c.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

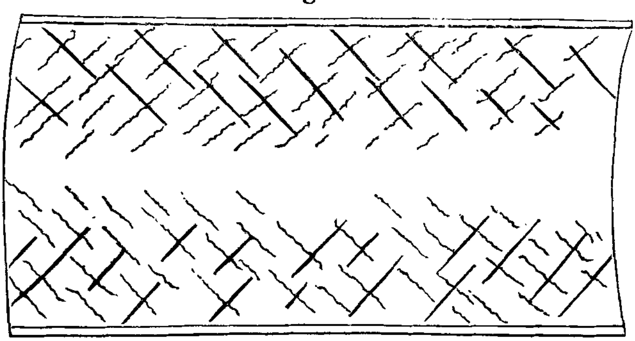
SINCE I addressed my last letter to you on the motion of glaciers, I have devised some experiments, the results of which I wish to communicate, as corroborative of the conclusions at which I have arrived by mathematical investigation.

The weight of a solid mass sufficiently small to experiment upon conveniently, is not sufficient to communicate a motion to it which, as in the case of a glacier, shall superinduce any considerable change of form and consequent fracture, the conditions under which the mass is placed being analogous to those of a glacier. When the width of a mass under such conditions is increased, the tendency of the forces arising from the weight of the mass to fracture it is also increased; and lateral obstacles, which would entirely arrest the motion of a mass of limited width, might produce scarcely a sensible effect on the central motion of a mass of large dimensions. When our object, however, is to determine the effects of an assigned motion and consequent change of form, as in the case before us, it is immaterial whether the motion be produced by the action of gravity, or any other cause. In my experiments the mass was about three feet in length and two in breadth, and two inches in depth. It consisted of fine mortar, which was allowed to assume different degrees of solidity, according to the particular object of each experiment.

The experiments were conducted as follows:—A trough of the length and width above-mentioned and five or six inches in depth, was prepared, open at both ends. Along the bottom of it was placed longitudinally a layer of straight rods parallel to each other, on which the mortar was poured, its consistency being about the same as that of mortar used in building. The quantity was such as to form a uniform layer of about two inches above the rods and to fill up the interspaces between them. The whole was then left to acquire the requisite degree of solidity. The motion was communicated by applying a force to the central rods alone, which, with the superincumbent mortar adhering to them, were thus made to move, while the lateral rods and mortar remained at rest. In this manner a straight transversal line drawn on the surface of the mortar became a *loop*, the form of which could be regulated at pleasure, and the mass was brought into a state of constraint exactly similar to that superinduced in a glacier by the excess of its central over its lateral motion. The results were different according as the mortar was comparatively soft or compact.

I. When the mortar was left for twenty-four hours or upwards, it acquired a considerable degree of solidity, so that, when such a motion had been communicated to its central portion as to give a very slight curvature to the transverse line above-mentioned, a system of parallel fissures began to be formed in each lateral portion, in directions making angles of 45° with the axis of the trough, as represented in fig. 1.

Fig. 1.



This is exactly accordant with my theoretical deduction for the case in which there is neither longitudinal nor transversal extension or compression (Second Letter, art. 20, p. 161), as was the case in the experiment; for, from the manner in which the motion was communicated, there could be no longitudinal extension or compression, and transversal compression could only arise from the relative motion of the centre, and could therefore only be of the second order of small quantities, the motion itself being considered a small quantity of the first order. The incipient fissures were not curved but straight, as they ought to be, according to theory, under the above conditions.

The experiment was also made somewhat differently, by placing the central rods so that they reached only half the length of the trough. When these rods were made to move as before, with the mass immediately superincumbent upon them, that portion along the middle of the trough under which the rods did not pass was extended, and the *curved* fissures across the whole surface were there produced in a manner exactly accordant with theory.

Another system of lines also resulted from the motion communicated to the mass. A great number of extremely small ridges forming discontinuous lines, as represented in the preceding diagram, appeared on the surface, their directions being at right angles to the lines of fissure. They were manifestly due to the compression in directions at right angles to them, these directions being in fact the directions of maximum pressure, as determined by theory. That such was the case was made further evident by continuing the motion of the central portion, and watching the gradual development and increased number of these lines. They were best developed when the surface of the mass had become slightly *crisped* before the motion was given to it.

To obtain more distinct evidence respecting the existence of the internal pressure in question, I cut the mass so as to form an artificial fissure of some width, along one of these lines of elevation, and therefore perpendicular to the directions of the original fissures first described. When the motion of the central part of the mass was continued, the artificial fissure was soon entirely closed up, while the width of the natural fissures continually increased. The evidence was perfectly conclusive.

According to theory, the directions of the natural fissures and that of the artificial one mentioned in the last paragraph, are those in which there is no tendency in the particles on one side of a vertical plane to slide past the contiguous particles on the opposite side. This was perfectly verified by experiment; for after the movement which opened the natural fissures and closed the artificial one intersecting them at right angles, the direction of the artificial fissure, as well as that of each natural one, remained unbroken at the point of intersection. There had not been the smallest sliding motion like that here contemplated, in the direction of either fissure, although there was nothing to resist the tendency to such motion, the cohesion being entirely destroyed along the lines of fissure. This conclusion, it will be recollected, is in direct opposition to the theory of Professor Forbes, according to which the direction of the artificial fissure above described is that in which the greatest sliding motion ought to take place.

The lines exhibited on Prof. Forbes's models of plaster of Paris, and which have been considered by him as representatives of the veined structure in glaciers, are to be referred, I have no doubt, to the same cause as the linear ridges described in my experiment. In fact, if it were possible to continue the central motion in my experiments, to the same extent as in the more fluid mass used in the experiments of Prof. Forbes, the two sets of lines in these cases respectively would assume exactly similar positions. I have shown experimentally that the lines in my experiments were not associated with any discontinuity of the mass, such as that which Prof. Forbes has considered to be indicated by the corresponding lines in his models. How such indication is afforded by the lines in question, I do not in any degree understand.

When the central motion was continued the fissures were necessarily brought more nearly to perpendicularity with the axis; but when their obliquity to their original position, *i. e.* to the line of maximum pressure, became considerable, they began to close. (Second Letter, art. 24, p. 163.). We thus account for the fact, that transverse fissures never remain open long enough to acquire a position in which their convexity would be turned towards the lower extremity of the glacier, a position which they must necessarily assume by the more rapid motion of the centre, unless there were some cause originating in that motion constantly tending to obliterate them.

When the central motion was continued long enough, the fissures along the flanks became more irregular and ran into each other, after which the central portion moved nearly as a continuous mass, sliding past the narrow lateral portions, from which it was severed on either side by the lateral fissures running into each other as just described.

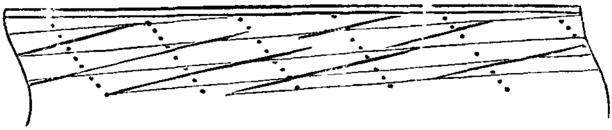
Another effect of the pressure superinduced by the motion of the central portion is not undeserving of notice. When the mass projected beyond the lower end of the trough, it was pushed outwards at the sides by the pressure acting transversely, so that by this action and the dislocation along the flank of the mass, the lateral portions turned round the extremities of the trough almost as completely as if the mass had been fluid. This explains the expansion of glaciers in expanding valleys without the hypothesis of fluidity. (Second Letter, art. 23, p. 163.)

II. When the experiment was made within an hour or two after the layer of mortar had been prepared, and when the mortar was comparatively soft, parallel lines of dislocation appeared on each side of the central portion, but in a position entirely different from those above-described. When first visible their common direction made an angle of 15° or 16° with the axis of the trough, whereas the systems of lines when the mass was more compact, were at an angle of 45° with the axis. To ascertain whether there was any tendency in the particles on opposite sides of one of these new lines of dislocation to slide past each other, I proceeded as in the former case, drawing a short line on the surface perpendicular to one of the dislocations, and continuing the central motion. The short line drawn on the surface immediately became a *broken* line at the point of intersection, proving the existence, in this case, of the sliding motion, of which it will be recollected there was no indication whatever in the experiment previously described, when the mass was more compact.

The lines now spoken of were not visible till the curvature of the transverse loop had become much greater than in the preceding case. On calculating the directions in which the tangential action was a maximum, by the construction previously given (Second Letter, art. 17), it appeared that those directions made an angle with the axis about half as great as that made by the lines of dislocation, when those lines first showed themselves. Now in supposing, as I have in my previous letters, that the lines of greatest tangential action are those along which *separation planes* would be formed, if formed at all, I have tacitly assumed the resistance afforded by the mass to their formation to be the same in all directions. It would seem highly probable, however, that, *cæteris paribus*, this resistance would be greatest in directions perpendicular to the lines of greatest pressure, in which case dislocations would take place in directions deviating *less* than the directions of greatest tangential action from the lines of maximum pressure. Such was the case in the experiments just described. The difference of position in the systems of dislocations, the mass being compact in one case and soft in the other, was owing, I conceive, to the mass yielding most easily in the former case to the direct *normal* tension, and in the latter case to the *tangential* action. It seems probable that such would be the case; and, in fact, in those experiments in which the mass was softest, I doubt whether it was ever brought into a state of *tension* at all, on account of its semifluidity, in which case there would be no tendency at all to form open fissures, as in the first set of experiments, while the formation of separation planes would be facilitated.

In the annexed diagram the stronger lines represent the lines of dislocation in my experiment, when the mass was soft,

Fig. 2.



or the lines along which the particles moved past each other; the finer lines those of greatest tangential action, and the dotted lines those of maximum tension, or those along which the *drag* takes place towards the middle of the glacier. Consequently these latter lines are those along which, according to Prof. Forbes's views, the sliding of one particle past another ought to have taken place. The lines on one longitudinal half only of the mass are represented in the diagram.

In experiments like those of Prof. Forbes, made with plaster of Paris in a semifluid state, the mass at the extreme upper section of the trough, partly from its adhesion to the trough and partly from its fluid condition, remains stationary, and consequently there is a tendency in the particles along each longitudinal line (more especially along the middle of the trough), to separate from each other. In this respect the motion differs from that in my experiments as above described; but with regard to the relative motions of the middle and sides, the cases are precisely similar. In the experiments above described with the soft mortar, the lines of dislocation were straight lines; but when the experiment was varied so as to give an elongation to the central portion (in the manner already described), the lines of dislocation were continued as curved lines across the central portion, forming *elongated loops*, with their convexity turned in a direction opposite to that in which the motion took place. These elongated loops exactly resembled those indicated by the coloured powder spread on the surface of the plaster of Paris in Prof. Forbes's experiments. I may also add, that in repeating the Professor's experiments, previously to making the others, I had observed that when discontinuity in the motion became sensible, as it did near the flanks if the inclination of the trough was made sufficiently great, it took place by a sliding of the particles past each other along the curved loop, as in my experiments. This was shown by a line, originally continuous across the loop, becoming a *broken* line at the point of intersection. The two systems of curves are undoubtedly identical.

It appears, then, that if a mass move in the same manner as a glacier in a canal-shaped valley (independently of local obstacles), two systems of dislocations may be formed, according as the mass is *semifluid*, or possesses at least a certain degree of *solidity*. The curves of dislocation have, in both cases, their convexity turned in the same direction, but in the former case the loop is much more elongated than in the latter. In the more elongated curves the tangent at a point near the side will be inclined to the axis at an angle of about 15°, whereas in the other curves the corresponding angle will be *at least* 45°. Now it is unquestionable that this last system of dislocations affords the accurate type of the transverse fissures of a glacier; but what glacial phænomena correspond to the other system? I challenge any one to point out in a canal-shaped glacier transverse fissures approximating to the elongated loops of that system; nor will the lines of structure suffice. There are, in fact, no phænomena hitherto observed which represent the loops in question, and hence I maintain that *glacial motion cannot be correctly represented by that of a semifluid mass.*

The experimental results I have now described are so clear and determinate, and appear to me to corroborate so distinctly the theoretical results I had previously obtained, that I trust I may be allowed to add this account of them to my previous communications, as the conclusion of the exposition of my views respecting the mechanism of glacial motion.

I am, Gentlemen,

Your obedient Servant,

Cambridge, March 19, 1845. W. Hopkins.

L. Mr. Hopkins's *Reply to* Dr. Whewell's *Remarks on Glacier Theories.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

HAVING completed the exposition of my own views respecting the motion of glaciers, I shall now beg permission to offer a few remarks in reply to the letters addressed to you by Dr. Whewell on the same subject.

With respect to the definitions which your correspondent has given of such terms as *solidity*, *flexibility*, &c., every one, I apprehend, will agree with him, and in so doing must allow the justice of the observation at the close of my first letter, which appears to have called forth these definitions, viz. that Prof. Forbes had used such terms too indiscriminately. He has spoken of having proved the *plasticity* of glacial ice by observations which only proved a small degree of *flexibility**,

* In the last paragraph of my First Letter, p. 16.