XIV. *On the Physical Phenomena of Glaciers.*—Part I. *Observations on the Mer de Glace.*

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§ 1.

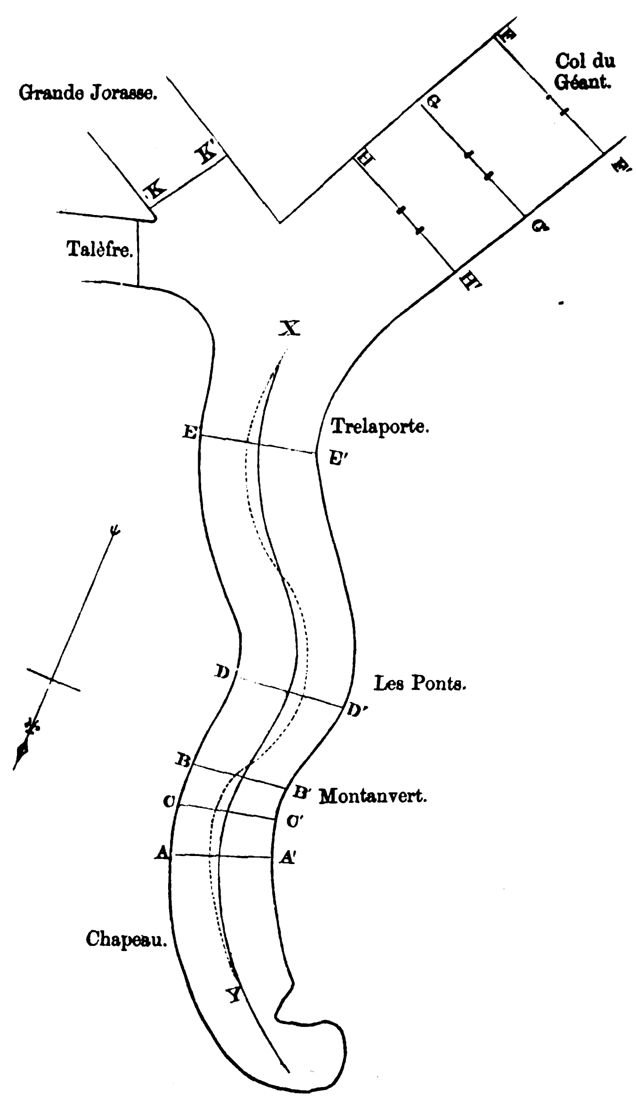
THE Philosophical Transactions for 1857 contain a paper by Mr. HUXLEY and myself upon the Structure and Motion of Glaciers. The observations on which that paper was founded extended over a very brief period, and hence arose the desire, on my part, to make a second expedition to the Alps, in which I regret to say my friend was unable fully to join. The phenomena of the Mer de Glace being those on which the most important theoretic views of the constitution and motion of glaciers are based, I wished especially to make myself acquainted by personal observation with these phenomena. Six weeks of the summer of 1857 were accordingly devoted to the examination of this glacier. For the purpose of observing its motion, bearings and inclinations, and also of determining its width at various points, I took with me an excellent 5-inch theodolite, and a surveyor's chain; for both of which I am indebted to the kindness of the Director-General of the Geological Survey, and to Professor RAMSAY. I propose to divide the investigation into two parts, the first of which forms the subject of the following paper, while the second will be the subject of a future communication. It gives me great pleasure here to record my grateful sense of the able and unremitting assistance rendered me throughout the entire period of the observations, by my friend Mr. T. A. HIRST, whose name indeed, had he permitted it, I should gladly have seen associated with my own at the head of this paper.

§ 2. *On the Motion of the Mer de Glace.*

Our first observation of the motion of the Mer de Glace was made on the 14th of July. On the steep terminal incline of the Glacier de Bois we singled out a tall pinnacle of ice, the front edge of which was perfectly vertical. In coincidence with this edge I fixed the vertical wire of our theodolite, and after three hours found that the ice cliff had moved downwards, the cross hairs being now projected against the face of the cliff several inches above its edge.

Our first line across the glacier was set out upon the 17th of July. The mode of proceeding in all such cases was this:—the theodolite was placed beside the glacier, quite clear of the ice, and usually at a sufficient height above it to command an uninterrupted view across the glacier. The plummet of the instrument being suspended, a stake was driven into the ground, or a fixed stone was carefully marked exactly under the point of the plummet. The direction of a line perpendicular to the axis of the glacier from this point being ascertained, a well-defined object was sought in the production of this line, at the opposite side of the valley—the sharp edge of a cliff, a projecting corner of rock, or a well-defined mark on the surface of the rock. This mark, and the objects surrounding it, were carefully sketched, so that in coming subsequently to the place the line was immediately recognized. The cross hairs being fixed upon the mark, the object-end of the telescope was lowered until the cross hairs cut the point at which a stake was to be placed. The positions of the stakes were found by means of an ordinary traveller's *baton*, which was set erect upon the ice and moved up or down in accordance with the signals from the observer at the theodolite, till the exact point was hit upon. Here the ice was pierced to the depth of about 18 inches, and a wooden stake was firmly driven into it. The position of each individual stake was secured by taking the angle of depression down to it, a precaution which was found very useful when subsequent reference to any particular stake was necessary. The exact time at which each stake was driven in was noted; and the time at which the displacements were measured being also observed, the motion was afterwards reduced, by calculation, to its diurnal rate.

Fig. 1.



The station from which our first line started was at some distance below the Montanvert Hotel, and about eighteen yards, in an ascending direction, from the station marked D on the Map of Professor FORBES*. The line is that marked AA′ on the sketch-map, fig. 1†.

* We found this station marked by a chisel on a block of granite, and painted red.

† The side of the glacier opposite to the Montanvert is much crevassed, and while fixing a stake upon one

On the 18th of July we set out a second line above the Montanvert Hotel, and we afterwards measured the displacements of the stakes along the line AA′. The result led to the establishment of a hitherto unobserved law of glacier motion, which the discussion of the observations will gradually render manifest. Reduced to twenty-four hours, the motion of the stakes along our first line was as follows:—

First Line (AA′).—Mean Daily Motion.

No. of stake.	Motion in inches.	No. of stake.	Motion in inches.
West 1	$12\frac{1}{4}$	6	
2	$16\frac{3}{4}$	7	$26\frac{1}{4}$
3	$22\frac{1}{2}$	8	
4	$25\frac{1}{2}$	9	$28\frac{3}{4}$
5	$24\frac{1}{2}$	East 10	$35\frac{1}{2}$

Stake No. 7 of this series was about midway between the bounding sides of the Mer de Glace; No. 1 was near the lateral moraine at the Montanvert side, and the retarding influence of this side is very manifest. With slight breaches of regularity, the rate of motion increases gradually from the first stake towards the centre of the glacier.

But it will be observed that stake No. 7 by no means moves the fastest. Stake No. 10 stood far beyond the centre, and upon the portion of the glacier derived from the Léchaud and Talèfre. This portion is distinguishable at a glance by the quantity of dirt upon its surface, the portion derived from the Glacier du Géant remaining comparatively clean throughout the entire length of the Mer de Glace. Professor Forbes accounts for the excessive crevassing of the eastern side of the glacier by assuming that the Glacier du Géant, having by far the greater mass, moves most swiftly, drags its more sluggish companions after it, and thus tears them asunder. The foregoing observations show that this assumption is untenable. The difference here observed cannot be referred to the slip to which reference has already been made in the note at the foot of this page, for the slip did not amount to more than 4 inches at the utmost. Further, the displacements were measured a second time on the following day, when the maximum movement of the Glacier du Géant portion was found to be $27\frac{1}{2}$ inches, and that of the Léchaud and Telèfre side $32\frac{1}{2}$.

Our second line, marked BB′ upon the sketch-map, had its terminal station on the ancient moraine a little higher up the glacier than the Montanvert Hotel. Along this line thirty-one stakes were driven on the 18th of July, and their displacements measured the day following. The results reduced to twenty-four hours are as follows:—

of the ice ridges here, the whole mass slid suddenly some inches forward. Were special attention directed to the crevassed portions of a glacier, the same phenomenon might, I doubt not, be frequently observed.

Second Line (BB′).—Mean Daily Motion.

No. of stake.	Motion in inches.
West 1	$7\frac{1}{2}$
2	$10\frac{3}{4}$
3	$12\frac{1}{4}$
4	$14\frac{1}{2}$
5	$14\frac{1}{2}$
6	16
7	$16\frac{3}{4}$
8	$17\frac{1}{2}$
9	19
10	$19\frac{1}{2}$
11	$19\frac{1}{2}$
12	21
13	21
14	21
15	$22\frac{1}{2}$
16	$22\frac{1}{2}$
17	$22\frac{1}{2}$
18	21
19	$22\frac{1}{2}$
20	$20\frac{1}{2}$
21	×
22	×
23	$24\frac{1}{2}$
24	×
25	$21\frac{3}{4}$
26	×
27	×
28	$22\frac{1}{4}$
29	$22\frac{3}{4}$
30	$25\frac{1}{4}$
East 31	$25\frac{3}{4}$

The stakes marked thus × were fixed by the eye, their positions being such that they could not be seen by the theodolite. Some of them were placed in deep glacial hollows, where, without an instrument, it was difficult to keep them in the same vertical plane. The slight uncertainty thus arising induced me finally to reject them. The gradual augmentation of velocity from the side towards the centre is very manifest; but it will be observed that stake 31, which stood upon the Talèfre side of the glacier, moved quickest of all. The difference in favour of the latter side is, however, much less than it was lower down.

The reason why in the two cases just considered the terminal stake towards the eastern side of the glacier shows no retardation, is, that the state of the ice, and the position of the theodolite, were not such as to enable us to continue the line of stakes completely across the glacier to the eastern side, and hence the observations could not show the retarding influence of that side. In setting out the third line CC′, therefore, Mr. Hirst took up a position on the Chapeau side of the valley, from which the vision across the glacier was quite uninterrupted by ridges or other obstacles, while the crevasses were not impracticable. One of the fixed termini of this line was the corner of a window of the Montanvert Hotel. There were twelve stakes planted along the line, and the motion of these during twenty-four hours, from the 20th to the 21st of July, was as follows:—

Third Line (CC').—Mean Daily Motion.

	East.										West.
No. of stakes.	1	2	3	4	5	6	7	8	9	10	11	12
Motion....	$19\frac{1}{2}$	$22\frac{3}{4}$	$28\frac{3}{4}$	$30\frac{1}{4}$	$33\frac{3}{4}$	$28\frac{1}{4}$	$24\frac{1}{2}$	25	25	18	×	$8\frac{1}{2}$

Stake No. 1 was fixed in the ice, close to the eastern side of the glacier, and the retarding influence of this side is quite manifest from the measurements. A glance, however, reveals a fact confirmative of the former measurements; the daily motion of the extreme eastern stake is $14\frac{1}{2}$ inches behind the maximum, while the motion of the extreme western stake is $25\frac{1}{2}$ inches behind it. The stake No. 5, which moved at the maximum rate, was also much nearer to the eastern than to the western side of the ice-stream; the observation therefore corroborates those already made as regards the position of the point of maximum motion.

How then is the fact to be accounted for, that the point of maximum motion of the Mer de Glace is thus thrown towards its eastern boundary? Reflection suggested to me that the effect might be due to the curvature of the valley through which the Mer de Glace moves. At the place where the foregoing observations were made the glacier bends, turning its concave side to the Montanvert, and its convexity towards the Chapeau. M. RENDU insists on the complete analogy of the phenomena of a river and those of a glacier; and the idea has been to a great extent corroborated by the measurements of Professor FORBES and M. AGASSIZ; but let us make a bolder application of the analogy than any of them contemplated, confining our view to the influence of curvature merely. The point of maximum motion of a river moving through a channel similar to that occupied by the Mer de Glace, would lie on that side of the centre of the channel towards which the river turns its convex curvature. Can this be the case with the ice? If so, the place of maximum motion ought to be different where the glacier bends in the opposite direction. Fortunately the Mer de Glace itself enables us to bring this idea to a test.

Higher up the valley, and opposite to the passages called "Les Ponts," such a band occurs. Here the convexity is turned towards the Montanvert or western side of the valley. A line was set out across this portion of the glacier on the 25th of July, and its measurement upon the 26th gave the following results:—

Fourth Line (DD').—Mean Daily Motion.

	East.														West.	
No. of Stakes.	1	2	3	4	5	6	9	10	11	12	13	14	15	16	17
Motion..	$6\frac{1}{2}$	8	$12\frac{1}{2}$	$15\frac{1}{4}$	$15\frac{1}{2}$	$18\frac{3}{4}$	$19\frac{1}{2}$	21	$20\frac{1}{2}$	$23\frac{1}{4}$	$23\frac{1}{4}$	21	$22\frac{1}{4}$	$17\frac{1}{4}$	15

After the setting out of this line, its length was measured by Mr. HIRST; and found to be 39 chains 25 links, which, as each chain is equal to 22 yards, gives 863 yards as the width of the Mer de Glace opposite the first "*Pont.*" A mark on the rock crossed by this *pont* constituted indeed one of the fixed termini of the line.

For the sake of stricter discussion, a copy of the notes of this measurement faces the next page.

The stakes along the line are marked thus, ⊙. The fixing of them commenced at the Echellets or eastern side of the valley, and they were numbered *from* this side: the measurement, on the contrary, commenced at the "Pont." Hence it is that the 17th stake was the first encountered in the measurement. This stake stood at a distance of 326 links, or nearly 72 yards from the edge of the glacier. Stake No. 1 at the other end of the line stood close beside the lateral moraine at the eastern side of the glacier.

Referring to the notes, it will be seen that the place of maximum movement occurs between the stakes 12 and 13, the former at a distance of 1544 links, and the latter at a distance of 1250 links from the western side of the glacier. The mean of these is 1397 links; consequently, as the entire width is 3925 links, the point of maximum motion is here 1131 links nearer to the western than to the eastern side of the Mer de Glace. The dirt also which marks the junction of the portion of the ice derived from the Col du Géant, with that derived from the other tributaries, is crossed at the distance 2251; hence the place of maximum motion occurs at a point 854 links *west of the dirt*, while on the lines set out lower down the point of maximum motion was far in upon the dirt, eastward from the junction. The position of the point of maximum motion changes, therefore, in exact accordance with the explanation given above.

But the question is capable of still closer examination. The notes enable us to compare a number of points at the eastern side of the glacier with others, situated at the same respective distances from the western side. Let us call every pair of points, one of which is situated as far from the eastern boundary as the other is from the western, *corresponding points*. The corresponding points along our fourth line may then be ranged as follows:—

	S. V.	S. V.	S. V.	S. V.	S. V.	
West....	17—15 ;	16—$17\frac{1}{4}$;	15—$22\frac{1}{4}$;	13—$23\frac{3}{4}$;	12—$23\frac{1}{4}$	...(A)
East	3—$12\frac{1}{2}$;	4—$15\frac{1}{4}$;	5—$15\frac{1}{2}$;	7—$18\frac{1}{4}$;	9—$19\frac{1}{2}$	

The numbers under the letter S are those of the stakes, those under V are the corresponding velocities. It will be seen that in each case the point on the western portion of the glacier moves quicker than the corresponding point on the eastern side. As a whole, therefore, the western side moves more speedily than the eastern, which is the reverse of what was observed lower down, but quite demonstrative of the explanation which refers the effect to the curvature of the valley.

An inspection of the notes also shows, that at the place where the fourth line crossed the glacier, the crevasses are found chiefly upon the portion derived from the Glacier du Géant. The dirt which announces the position of the other tributaries of the Mer de Glace is crossed at the distance 2251; and after this distance we find the remark "crevasse nearly closed," "closed crevasse;" so that not only is the eastern side of the glacier here less crevassed than the western, but crevasses previously formed are partially, or wholly closed up. The shifting of the place of strain consequent on the change of curvature, carried naturally along with it the shifting of the crevasses. It may be inferred from the notes that the measurement of such a line is not without its difficulties.

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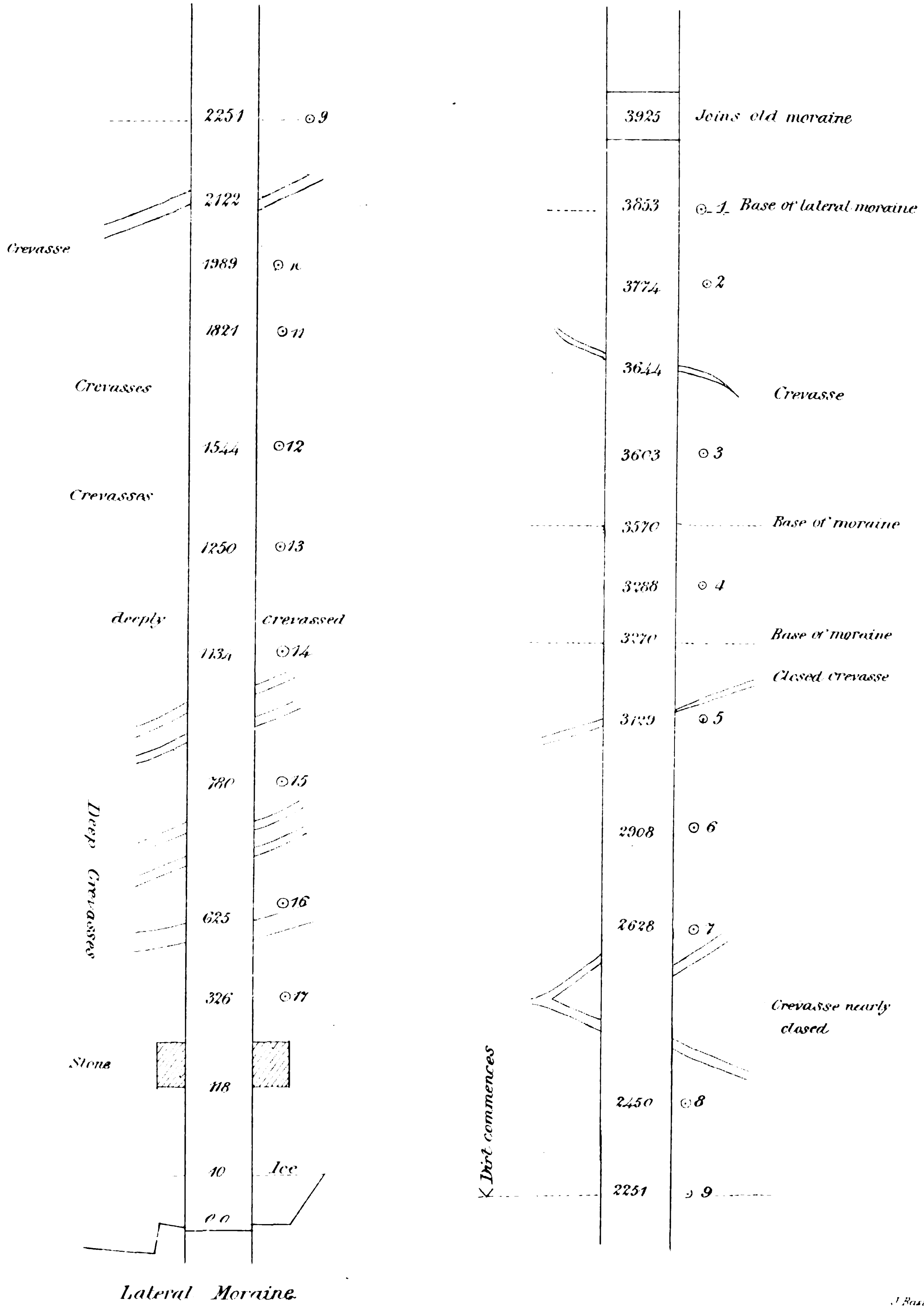
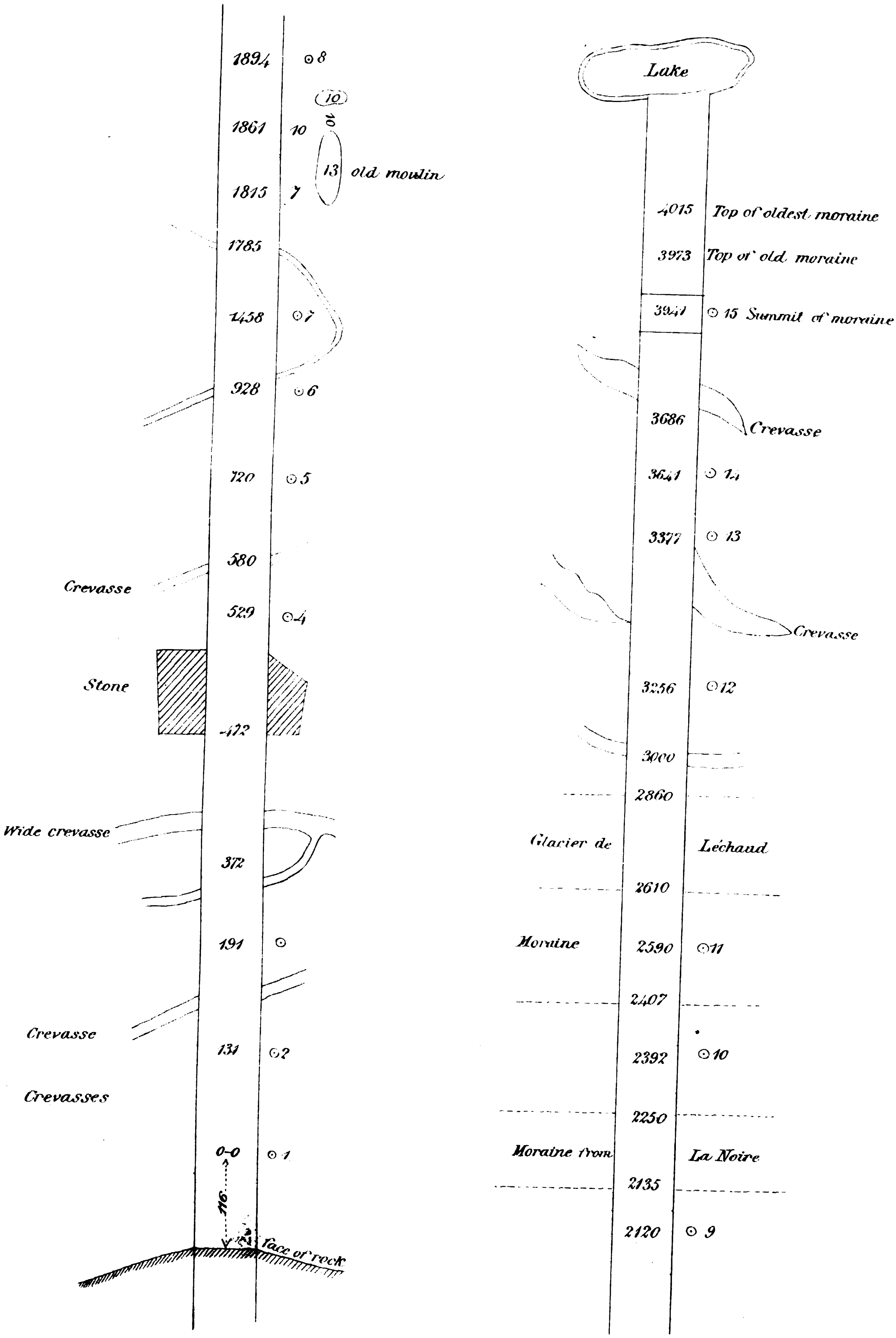


J. Basire lith.



J. Basire, lith.

Our next line (EE′) stretched across the glacier from the promontory of Trelaporte to the base of the Aiguille du Moine. The instrument being placed upon a grassy slope above the promontory, the line was set out on the 28th of July. The Trelaporte end of this line was immediately under the station marked G* on the Map of Professor Forbes; the displacements of the stakes were measured on the 31st of July, and were found to be as follows:—

Fifth Line (EE′).—Mean Daily Motion.

	West.														East.
No. of Stakes.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
Motion......	$11\frac{1}{4}$	$13\frac{1}{2}$	$12\frac{3}{4}$	15	$15\frac{1}{4}$	16	$17\frac{1}{4}$	$19\frac{1}{4}$	$19\frac{3}{4}$	19	$19\frac{1}{2}$	$17\frac{1}{2}$	16	$14\frac{3}{4}$	10

The first of these stakes was about 80 feet distant from the face of the rock at Trelaporte; the 15th was on the lateral moraine, which moved along with the ice at the opposite side of the valley. The retarding influence of both sides is very clearly shown, the motion of the central stakes being nearly twice that of the extreme ones. As a whole, the rate of motion is slower here than at the "Ponts" or at the Montanvert.

This line was also chained by Mr. Hirst; a copy of his notes, showing the distances along the line at which the stakes were set, faces this page.

The chaining commenced at a point 116 links distant from the face of the rock at Trelaporte. Adding these 116 to the distance 3941, we have 4057 links, or 893 yards for the width of this portion of the Mer de Glace. The point of maximum motion occurs at stake No. 9, which is 2236 links distant from the rock at Trelaporte, or more than one-half the distance across; that is to say, the point of maximum motion is here nearer to the Talèfre side than to the Géant side of the glacier. Here, again, we have a result different from that obtained with our fourth line; and if we look to the sketch-map we shall see the reason. Between the fourth and fifth lines the Mer de Glace has passed a point of contrary flexure; and here at Trelaporte the convex side of the glacier is turned towards the base of the Aiguille du Moine.

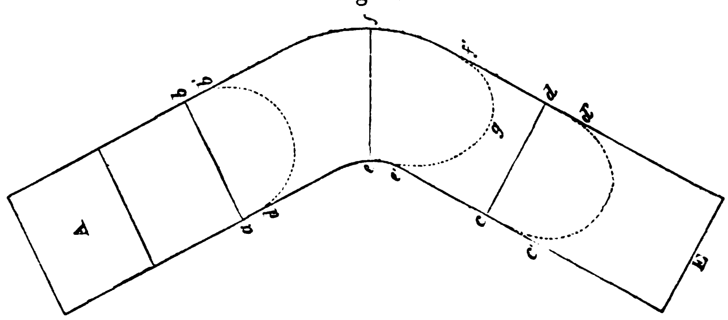
Taking the 116 links at the commencement into account, the following pairs of stakes may be regarded as corresponding points:—3 and 14; 4 and 12; 7 and 10; the small numbers referring to stakes at the western, and the large numbers to stakes at the eastern side of the glacier. The relative motions of these points are as follows:—

West......	3—$12\frac{3}{4}$;	4—15;	7—$17\frac{1}{4}$.
East	14—$14\frac{3}{4}$;	12—$17\frac{1}{2}$;	10—19.

Comparing this Table with Table A, we observe a reverse result; in the latter case the western stakes moved most swiftly; here the eastern ones do so; the deportment of the ice is the same as at the places intersected by our three first lines, and the curvature of the valley is also similar.

From the foregoing observations the following law of glacier motion is derived:—*When a glacier moves through a sinuous valley, the locus of the point of maximum motion does not coincide with a line drawn along the centre of the glacier, but always lies on the* *convex side of the central line. It is therefore a curve more deeply sinuous than the valley itself, and crosses the axis of the glacier at each point of contrary flexure**.

Fig. 2.



The law may be illustrated by the following experiment:—A, fig. 2, is a box filled with fine mud, which by raising a sluice in front flowed into the curved trough AE. A line *ab* was drawn upon the mud above the bend, a second line, *cd*, below the bend, and a third, *ef*, at the bend. The distortions of these three lines by the motion of the mud downward will reveal the position of the point of maximum motion at the particular places where they are drawn. The line *ab* was distorted to *a'b'*, the summit of the curve being exactly in the centre of the trough, thus proving that the centre was the place of maximum motion. The same was true of the line *cd*, which was distorted to *c'd'*. The line *ef* was distorted to *e'f'*, the summit *g* of the curve being nearer to the side *bfd* of the trough, this proving the point of maximum motion to lie towards that side.

I scarcely know a case more calculated to impress the mind both with the yielding power of ice to pressure, and the magnitude of the forces brought into play in the motion of glaciers, than the crushing of the three tributaries of the Mer de Glace through the throat of the valley between Trelaporte and the base of the Aiguille du Moine. Not wishing to trust the eyes in the estimation of distances here, each of the three confluent branches was measured. The width of the Glacier du Géant, a short distance above the Tacul, was found to be 5155 links, or 1134 yards. The width of the Glacier de Léchaud, just before its junction with the Talèfre, was found to be 3725 links, or 825 yards. That of the Talèfre, before it is influenced by the pressure of the Léchaud, that is, across the ice-cascade, was found, approximately, to be 2900 links, or 638 yards. Adding all together, we find the sum of the widths of the three branch glaciers to be 2597 yards. At Trelaporte these three branches *are forced through a gorge* 893 *yards wide*; and our measurements show that it passes through with a velocity of nearly 20 inches a day!

Limiting our view to one of the glaciers thus compressed, the facts appear still more astonishing. Previous to its junction with the Talèfre, the Glacier de Léchaud has a width of thirty-seven chains and a half. In passing through the jaws of the granite vice at Trelaporte, this broad ice river is squeezed to a driblet *less than four chains in width*! This fact illustrates the relation of the size and power of a glacier to the quan-

* If the defined line between X and Y on the sketch map represents a line drawn along the centre of the glacier, the dotted line will represent the locus of the point of maximum motion.

tity of snow drainage which supplies it. The Talèfre has its basin, and the Géant has its vast plateau, from which the respective glaciers derive nutrition; but the Léchaud is fed by two or three couloirs merely, which descend principally from the Mont Mallet and Les Jorasses. The Géant, in the struggle for place at Trelaporte, takes up more than half the valley, and the others come in the order of the drainage which supplies them.

The velocity of the Mer de Glace at Trelaporte being about 20 inches, it seemed probable that the velocity of the Glacier du Géant above the Tacul, and also of the Léchaud above its junction with the Talèfre, would be considerably less, in consequence of the greater width at these places. This proved to be the case. On the 29th of July a line was set out across the Glacier du Géant, a little above the Tacul. There were ten stakes in this line, and their motions reduced to twenty-four hours were as follows:—

Sixth Line (HH′).—Mean Daily Motion.

No. of Stakes.	1	2	3	4	5	6	7	8	9	10
Motion......	11	10	12	13	12	$12\frac{3}{4}$	$10\frac{1}{2}$	10	9	5

The velocity here is considerably under that of the Mer de Glace at Trelaporte.

On the 1st of August we set out a line across the Glacier de Léchaud immediately above where it is joined by the Talèfre. The line commenced at the side of the glacier beneath the block of stone called the Pierre de Béranger, and ran perpendicular to the axis of the glacier to the other side. The displacements were measured on the 3rd of August: reduced to twenty-four hours, they are as follows:—

Seventh Line (KK′).—Mean Daily Motion.

No. of Stakes.	1	2	3	4	5	6	7	8	9	10
Motion......	$4\frac{1}{2}$	$8\frac{1}{4}$	$9\frac{1}{2}$	9	$8\frac{1}{2}$	$7\frac{1}{2}$	$6\frac{1}{4}$	$8\frac{1}{2}$	7	$5\frac{1}{2}$

The stakes 8 and 9 were at opposite sides of a "moulin," which was found to share the general motion of the glacier. A new crevasse crossed our line above 8 and below 9, and the greater advance of stake No. 8 was probably owing to the yielding which this crevasse permitted. The rates of motion, it will be observed, are still less than those upon the Glacier du Géant.

Were the Glacier de Léchaud subjected to no waste during its descent, and did no accumulation take place at any point, equal quantities of ice would pass through all its cross sections in the same time. The compression which takes place at Trelaporte is not a change of *volume* but of *form*. The mass is squeezed laterally, and no doubt expands vertically. Comparing the velocities and widths at Trelaporte and opposite the Pierre de Béranger, we should be led to the result that the depth of the Glacier de Léchaud at the former place would, if no waste had taken place, be at least four and a half times its depth at the latter. The loss of ice by superficial and subglacial melting must materially modify this result; but some interesting observations might be made in connexion with the point, and I think one result of such observations would be the establishment of the comparative shallowness of the Glacier de Léchaud.

There is another characteristic of glacier motion which was predicted by Professor FORBES, before any observations had been made upon the point, and afterwards confirmed both by his own measurements and those of M. MARTINS,—I allude to the fact that the glacier is not only retarded by its sides, but by its bottom, the superficial ice thus moving more quickly than that in contact with the bed of the glacier.

Objections have been made to both the measurements alluded to, and I was therefore desirous to submit the question to a new test. The experiments which I have to record were made upon the face of an ice precipice, which offered a rare opportunity for an observation of the kind. The face formed the eastern boundary of the Glacier du Géant near the Tacul, was about 140 feet in height, and nearly vertical. I requested Mr. HIRST to place two stakes, one at the top and the other at the bottom of this precipice. This was done on the 3rd of August; and on the 5th it was found that the stake at the top had moved through $12\frac{1}{2}$ inches, while that at the bottom showed an advance of 6 inches only. There was some uncertainty regarding this latter result, on account of the danger incurred by the assistant, from the stones which fell incessantly from the top of the precipice, and which compelled him to retreat several times before the measurement could be effected.

I was reluctant, however, to leave an observation of the kind with a shade of uncertainty attached to it. On the 11th of August, therefore, I fixed myself two stakes, one at the top and the other at the bottom of the precipice, and feeling strongly impressed with the importance of ascertaining the motion of a point midway between top and bottom, I cut steps in the ice, climbed the face of the precipice, pierced the ice with an auger, and drove a stake firmly into it. Until Monday the 17th of August I was unable to reach the place again. On this day I penetrated through dense fog and snow to the Tacul, and found the highest of the three stakes standing, but the two lower ones were buried in a heap of snow which lay at the base of the precipice. On the following day the perilous process above described had to be repeated; and on Tuesday the 20th of August the displacements were measured. Reduced to twenty-four hours, the motion of the three stakes was found to be as follows:—

	inches.
Top stake	6·00
Middle stake	4·59
Bottom stake	2·56

The distance from the top of the ice-wall to its base was found, by measurement with a rope, to be 140·58 feet, but it was not quite perpendicular at its upper portion; the height of the middle stake from the ground was 35 feet, and of the bottom one 4 feet. It is therefore proved by these measurements that the bottom of the ice-wall at the Tacul moves with less than half the velocity of the summit; while the deportment of the intermediate stake shows how the velocity increases from the bottom upwards.

§ 3. *On the Cause of Glacier Motion.*

The various theories which have been advanced to account for the progression of glaciers are too well known to need detailed discussion here. SAUSSURE, and some before him, thought that the glacier slid along its bed*. CHARPENTIER thought that the motion was due to the freezing of water in capillary fissures, and the consequent swelling of the contents of these fissures. Other hypotheses have been advanced without producing any deep impression. It has been objected to SAUSSURE'S theory, that were it true, glaciers must slide down with an accelerated motion; but reflection alone would deprive this objection of weight, and an experiment of Mr. HOPKINS completely refutes it†. When incessantly checked by the surface over which they slide, even avalanches may, and do, sometimes descend with a uniform motion. The motion of a man in walking down stairs is on the whole uniform, but it is actually made up of an aggregate of small motions, each of which is accelerated. It is easy to conceive that ice moving over an uneven bed, will, when it is released from one opposing obstacle, be checked by another, and its motion thus be rendered sensibly uniform. So many obstacles exist along the bed of a glacier, that sudden slipping forwards of the mass through any considerable distance is not to be expected. But the real weak point of SAUSSURE'S theory, though partly true, is its inability to account for many facts observed since his time. The theory of CHARPENTIER, though not always fairly represented, has been shown to be untenable.

The facts submitted to our consideration are briefly as follows:—We see the glacier winding through a valley, squeezing itself through a gorge, and widening where it has room. We see that the centre moves more quickly than the sides, and the top more quickly than the bottom; and the next demand of the mind is for a general principle which shall unite these facts, and from which they shall follow as physical corollaries. Professor FORBES seeks this principle in the *viscosity* of the ice. Ice, according to him, is a substance resembling treacle, honey, or tar, and the observed phenomena are a consequence of this property. In this assumption consists what is called the *viscous theory*‡.

* I hardly think, however, that SAUSSURE would have subscribed to some of the interpretations of his theory now extant.

† See HOPKINS in Philosophical Magazine, vol. xxvi. p. 4. Were it not that this objection is thoughtlessly repeated in every work upon glaciers, I would not dwell upon it here. The objection drawn from the deportment of secondary glaciers lying on steep slopes is also very commonly dwelt upon, but it is equally without weight; and applies with at least as much force to the viscous theory as to the theory of SAUSSURE.

‡ The name of M. RENDU will always be honourably associated with the theory of glacier motion. He first drew attention to the power of the glacier to move through a sinuous valley, to narrow and widen and behave like lava or like "a soft paste." He conjectured also that the centre would move more quickly than the sides. In fact he appears to have had a correct conception of almost all that the subsequent observations of Professor FORBES established. I regret to say that I have not been able to obtain M. RENDU'S original memoir.

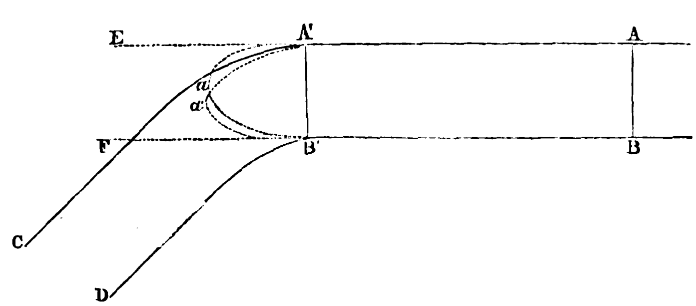
August 1859.—Thanks to my Zürich friends, I have recently had the pleasure of reading M. RENDU'S paper, the perusal of which has confirmed my estimate of his sagacity. Had this gentleman been a philosopher instead of an ecclesiastic, we should doubtless have heard more about his claims than we have hitherto done.

Before entering upon the examination of this theory, I would ask permission to make the following remarks:—I am aware that the paper published by Mr. HUXLEY and myself has produced considerable diversity of opinion among scientific men. Some, whose opinions are entitled to every respect, regard the views there advocated, and the experiments there described, as consistent with and explanatory of the viscous theory; while others, of equal eminence, believe that if the views referred to be sound, the viscous theory can no longer be maintained. Under these circumstances it behoves me to state distinctly the point of view from which I intend to examine the theory, submitting myself completely to the public sense as to whether this point of view be the correct one or not. Both the terms and the illustrations made use of by Professor FORBES have diffused ideas regarding the physical qualities of ice which render a strict examination of the subject essential. Let me here briefly state what I understand by viscosity, and what I, and other more competent persons, at one time believed to be a demonstrated property of ice*.

By viscosity, I understand that property of a semifluid body which permits of its being drawn out when subjected to a force of *tension*, the particles of the substance taking up new positions of equilibrium, so that when relieved from the strain the substance has no distortion to recover from. A capacity to change the form under crushing *pressure* is not, I think, a test of viscosity; for this power is possessed by substances, to which we should never think of applying the term viscous.

In examining whether glaciers possess the power of yielding to tension like viscous bodies, I would refer:—1. To the shifting of the place of strain by the curvature of the valley, to which I have already referred. Let ABCD, fig. 3, embrace a curved portion of a glacial valley, and let AB be a linear element of the glacier transverse to its axis.

Fig. 3.



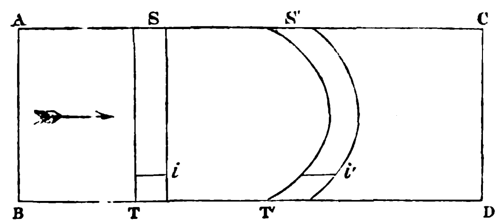
As the ice descends AB becomes curved in consequence of the quicker movement of its centre. Did the valley continue straight in the direction of E and F, the point of maximum velocity would, after a certain time, be found at a, midway between the lines AE, BF; but the curving of the valley throws the point a to a', and thus increases the strain upon the branch a'A' of the curve, while it diminishes the strain upon a'B'. The conse-

* "*Gluey tenacity*" is the quality which I have heard ascribed to ice by intelligent and cultivated persons.

quence of this difference of action upon the two branches, is that the side of the glacier which is subjected to the augmented tension does not yield to the strain as a viscous body would do, but *breaks*. In the words of Professor FORBES, the glacier at this place becomes "*excessively crevassed.*" This fact, therefore, as far as it goes, is opposed to the idea of viscosity as above defined.

2. The fact that the centre of a glacier moves more quickly than the sides, is that on which the viscous theory is chiefly based: let us examine the circumstances connected with this motion, availing ourselves while doing so both of the figure and the reasoning of Mr. HOPKINS. Let ABCD, fig. 4, be a sloping canal, into which is poured a

Fig. 4.



quantity of treacle, honey, tar, or melted caoutchouc, all of which have been referred to as illustrative of the character of ice; and let the mass move down the slope in the direction of the arrow. Let ST be a narrow segment of the viscous substance; this segment, as it moves downwards, will take the form S'T'. Supposing T*i* to be a square element of the mass, it will be distorted lower down into the lozenge T'*i*', and the line T*i* will become T'*i*'. Now the analogy between such a substance and ice fails in this respect; in the viscous mass the short diagonal of the square *stretches* to the long one of the lozenge, but, in the glacier, the ice breaks at right angles to the tension, and *marginal crevasses* are formed. It was by means of the simple diagram here sketched that Mr. HOPKINS showed why the marginal crevasses of a glacier are inclined towards its source*. This fact, therefore, so far as it goes, is also opposed to the idea of viscosity.

But it is known that in the case of a substance confessedly viscous, a sudden shock or strain may produce fracture. Professor FORBES justly urges, "that sealing wax at moderate atmospheric temperatures, will mould itself (with time) to the most delicate inequalities of the surface on which it rests . . . . . but may, at the same time, be shivered to atoms by a blow with a hammer†." Hence, in order to estimate the weight of the objection, that the glacier breaks when subjected to strain, we must know the conditions under which the force is applied.

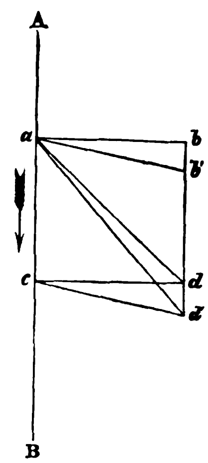
The fifteenth station on the line (EE') at Trelaporte stands on the lateral moraine of the glacier; between it and the fourteenth, a distance of 300 links, or 190 feet, intervenes, and within this distance the glacier suffers its maximum strain. Let AB (fig. 5) be the

* Philosophical Magazine, vol. xxvi. page 160.

† Philosophical Magazine, Fourth Series, vol. x. p. 201. Proceedings of the Royal Society, June 14, 1855.

side of the glacier, and let the direction of motion be that indicated by the arrow. Let *abcd* be a square element of the glacier with a side of 190 feet. The whole square moves downwards with the glacier, but the side *bd* moves quickest. The point *a* moves 10 inches, the point *b*, 14·75 inches in twenty-four hours, the differential motion thus amounting to an inch in five hours. Let *ab'cd'* be the shape of the figure after five hours' motion, the distance $bb'=dd'$ being 1 inch; then the line *ab* would be extended to *ab'*, and the line *cd* to *cd'*.

Fig. 5.



But the extension of *these* lines does not mark the *maximum strain* to which the ice is subjected. Mr. Hopkins has shown this strain to take place along the line *ad*, which encloses an angle of 45° with the side of the glacier. In five hours, then, this line, if capable of yielding, would be stretched to *ad'*.

In the right-angled triangle *abd'* we have $ab=2280$ inches, $bd'=2281$, and hence we find *ad'* to be 3225·1 inches; the diagonal *ad* is 3224·4 inches; and the amount of yielding required from the ice is that the latter line shall be extended by five hours' gradual strain to the length of the former.

This is the utmost demand made upon the presumed viscosity of the ice, but the substance is unable to respond to it: instead of stretching, it breaks, and copious fissures are the consequence. It must not be forgotten that the evidence here adduced merely proves what ice *cannot* do; what it *can* do in the way of viscous yielding we do not know. There is no experiment on record, with masses great or small, to show that the substance possesses, in any measurable degree, that power of being drawn out which is the very essence of viscosity.

Further, the case here referred to is not solitary, but typical. I dare say every single glacier of the first order would furnish proofs of the absence of viscosity equally cogent with that here brought forward. The marginal crevasses of glaciers usually result from an incapacity on the part of the ice to respond to a demand upon its viscosity, not greater than that just cited *.

When a person unaccustomed to glacier life observes, from a safe distance, the profound fissures by which the ice is intersected, the question sometimes arises, "what if one of these chasms should suddenly open beneath the traveller's feet?" There is, however, no fear of this. The crevasses, when first formed, are exceedingly narrow, and they

* It may, however, be urged that I do not know how much the ice observed in the locality referred to had been stretched before it arrived there. Extend an elastic string to the point of breaking, and a small additional force would break it; but this latter small extension would be no measure of the extensibility of the string. To this I reply, that it is the very essence of a viscous mass to accommodate itself to the forces which act upon it, so that in each new position the texture of the substance shall be in a state of equilibrium. If such a mass be broken it will have no distortion to recover from. The idea that a glacier is typified by such a string as that referred to, has been expressly rejected by the ablest advocates of the viscous theory; in proof of which I would refer to the lucid note of Dr. Whewell, in the 26th volume of the Philosophical Magazine, page 172. Cases may occur where the lateral yielding produced by the *pressure* along *bc*, fig. 5, may satisfy the *strain* along *ad*; in such a case no marginal crevasses would be formed.

open with extreme slowness. While standing one evening, in company with Mr. Hirst, on the Glacier du Géant, both of us were startled by a sound like a heavy explosion in the body of the glacier, underneath the place where we stood. This was instantly followed by a succession of loud cracks, accompanied by a low singing noise. The ice continued cracking for an hour; but notwithstanding the manifest breaking of the glacier, which was to some extent awe-inspiring, we could not, for a long time, detect any trace of rupture. The escape of air-bubbles from the surface first informed us of the position and direction of the incipient crevasse, for such it was. It was so narrow that the thinnest blade of my penknife would not enter it.

On another occasion, our guide, while engaged in setting out one of our lines, observed the ice to break beneath his feet, and a rent to propagate itself suddenly, with loud cracking, to a distance of 50 or 60 yards across the glacier. These fissures are produced by tension, and the velocity with which they widen is a measure of the amount of relief demanded by the glacier. The crevasse last alluded to required several days to attain a width of 3 inches, and the opening of the one on the Glacier du Géant was far slower than this. This is their general character. They form *suddenly* and open *slowly*, and both facts are demonstrative of the non-viscosity of the ice. *For were the substance capable of stretching, even at the small rate at which they widen, there would be no necessity for their formation* *.

There is another point of view from which the question of viscosity may be examined; but as the observations which bear upon it possess a general value, I will devote a special section to them; choosing afterwards those which more particularly apply to the case now under consideration.

§ 4. *On the Inclinations of the Mer de Glace.*

By calculation from heights and distances, Professor Forbes obtained approximately the inclinations of some portions of the Mer de Glace †, but no direct observations on the subject have been hitherto made. On the 4th of August we transported our theodolite to the Jardin, for the purpose of ascertaining its inclination, and that of the Glacier du Talèfre. From the green space on which visitors to the place usually repose, the angle of elevation to the top of the Jardin is 24° 7′, and from the same place downwards to the bottom of the Jardin the inclination is 30°. From the bottom of the Jardin, for some distance along its medial moraine, the ice is nearly level, its inclination being only 21′. A succession of slopes then follows, enclosing with the horizon the following angles of depression:—3° 5′; 4° 25′; 6° 50′; 8° 5′ and 9° 40′, which last brings us to the brow of the ice cascade. The inclination of the fall is 25°,—producing a line drawn along the centre of the cascade until it cuts the moraine between the Talèfre and Léchaud: the inclination along this line, from the base of the cascade downwards, is 7° 30′.

* For an interesting account of the formation of a number of new crevasses, see Agassiz, 'Système Glaciaire,' p. 310.

† Travels, p. 117.

The descent of the ice through this gorge from the basin of the Talèfre, is adduced by Professor FORBES as an illustration "which will appear to the impartial reader almost a demonstration" of the principle of viscosity. "The ice is compact," he urges, "and almost without fissures. . . . . The open crevasses which commence a little above AB are turned towards the basin*." The line AB here referred to is actually in the jaws of the gorge, and apparently at a considerable distance below where the ice enters it. The description certainly would not apply to the ice of the year 1857. Long before reaching the summit of the fall the most skilful iceman would find himself in difficulties. We proceeded as far as we dared amid the pits and chasms into which the glacier is torn, and which followed each other so speedily, that the ridges between the fissures were often reduced to mere plates and wedges, which were in many instances bent and broken by the lateral pressure. At some places vortical forces seemed to have acted upon the mass, and turned huge pyramids so far round as to place the structural veins at right angles to their normal position. Looking downwards towards the summit of the cascade, the ice was frightfully riven. The glacier descends the cascade itself in wedges, pyramids, and columns, which latter often fall with a sound like thunder, and crush to pieces the ice crags below them. After this description I do not think that the case is likely to be accepted as a demonstration of the viscosity of ice.

I now pass on to the inclinations of the Glacier du Géant. For some distance below the base of the so-called *Seracs* the irregularities of the glacier render an estimate of its general inclination somewhat difficult, but I should judge it to be about 13°. From the end of this steeper portion, two slopes, one of 4° 37′, and the other of 3°, bring us to the Tacul, and from this point to the bottom of the ice valley at Trelaporte we have the following series of inclinations:—2° 15′; 3° 15′; 5° and 9°; thence to the Grand Moulin the slope is 3° 30′, and afterwards, down the glacier to a point nearly opposite to the Grande Cheminée below l'Angle, the inclinations are 3° 10′; 5°; 6° 25′, and 4°. The glacier then descends a slope of 9°, and afterwards passes the Montanvert at an inclination of 4° 45′. Below the Montanvert it falls steeply for some distance, the inclination being 16°. Between the base of this slope and the brow which marks the termination of the Mer de Glace and the commencement of the Glacier des Bois, the slope is 5° 10′. The ice afterwards descends an incline of 22° 20′ in a state of great dislocation. From the base of this incline the general inclination of the lower portion of the glacier is 10°.

A brief reference to the Glacier de Léchaud will complete this portion of our subject. The upper portion of the glacier, to the base of the steep snow slopes which rear themselves against the Grande Jorasse, has an inclination of 4° 29′. Opposite to the icefall of the Talèfre, the inclination, for a short distance, is 3° 17′, and afterwards down to the Tacul, where the Léchaud and Géant join, the slope is 5° 22′.

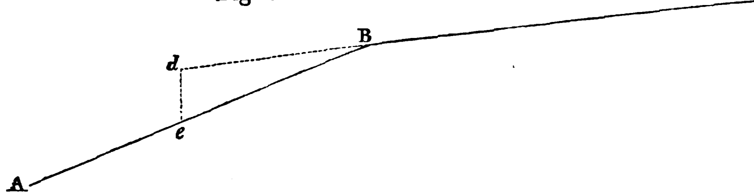
I will now endeavour to show the theoretic significance of the observations above recorded, referring in the first place to the great terminal slope of the Glacier des Bois,

* "Reply to HOPKINS," Philosophical Magazine, 1845, vol. xxvi. p. 415.

down which the ice is shot in crags, pinnacles, wedges, and castellated masses, all tossed together in the utmost confusion. Regarding this portion of the glacier, Professor FORBES writes as follows:—"Escaping from the rocky defile between the promontory of the Montanvert and the base of the Aiguille de Dru, it pours in a cascade of icy fragments, assuming the most fantastic forms, into the valley beneath." Above the fall the ice is compact: Professor FORBES compares it to the dark unruffled swell of swift water rushing to precipitate itself in a mass of foam over a precipice.

In fig. 6 I have protracted the inclination of the fall and of the glacier above it, one

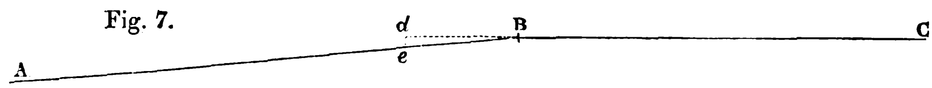
Fig. 6.



of them, BC, making an angle of 5° 10′, and the other, BA, an angle of 22° 20′ with the horizon. Supposing the ice to pursue the direction which it had previous to reaching the fall, it would, at the end of a certain time, reach the point *d* *; but the ice is not rigid enough to do this, and the mass descends to *e*. Now if it be the viscosity of the substance which has carried it in a certain time from B to *d*, that same property ought, one would think, to enable it to drop down the vertical *de* without breaking. But so far from its being able to do this, the glacier descends the slope BA as "a cascade of icy fragments." The fact, therefore, adds its evidence to that already adduced against the viscosity of the substance.

But the case will appear much stronger when we revert to other slopes upon the Mer de Glace. For example; the inclination of the glacier above l'Angle is 4°: it subsequently descends a slope of 9° 25′, and in doing so is so much fissured as to be absolutely impassable. The chasms cut the glacier from side to side, and present clear vertical faces of great depth †. Subtracting the smaller of the above angles from the larger, the difference, 5° 25′, gives the *change* of slope which produces the chasms. In fig. 7 the two adjacent slopes are protracted to a proper scale. Now the velocity of the

Fig. 7.



glacier here, in the direction of its length, is to the vertical velocity with which it would have to sink to reach its bed, as B*d* : *de*, or as the cosine of 5° 25′ is to its sine, or as 996 : 94, or, in round numbers, as 10 : 1. Hence if it be viscosity which enables the mass to move from B to *d* in a certain time, the same property ought, one would think, to permit it to *sink* through the space *de*, which is only one-tenth of B*d*, in the same

* I here assume that the general inclination of the surface of the glacier changes in accordance with that of its bed, which will hardly be questioned.

† I once found myself alone upon this portion of the glacier towards the close of a day's work, and experienced great difficulty in escaping from the entanglement of chasms in which I had involved myself.

time. But this is not the case. In accommodating itself to the change of inclination, the glacier breaks and is fissured in the manner described.

The change of inclination last mentioned, so far from marking the limit at which transverse crevasses begin to be formed, is sufficient to produce chasms of great magnitude, and in most inconvenient numbers. Higher up the glacier, transverse crevasses are produced by a change of inclination from 3° 10′ to 5°. If this change be accurately protracted, the mere inspection of it will illustrate more forcibly than words can do the absence of the power of viscous yielding on the part of the ice.

Looked at broadly, then, two classes of facts address themselves to the attention of the glacier investigator; one entirely in accordance with the idea of viscosity, and the other as entirely opposed to it. The affirmers and deniers of the viscous theory have perhaps been influenced too exclusively by one or the other of these classes of phenomena. The analysis of the facts gives the result, that where *pressure* comes into play we have the evidences of apparent viscosity*, but where *tension* is active we have evidences of an opposite kind. One of these classes of effects is as undeniable as the other, and hence the true theory of glaciers must render an account of both.

When the mountain snow is first moistened, it becomes more coarsely granular; these granules abut against each other, and hold air and water in their interstices. But as successive layers press upon the mass, the granules are squeezed more closely together; rupture and liquefaction, succeeded by regelation, take place at the points of abutment; water and air are expressed by the process, and the mass becomes more and more consolidated. But although powerfully squeezed, each portion of the deeper ice is surrounded on all sides by a resistent mass; it is thus compelled to yield very gradually to the pressure and moves slowly through into the valley of *écoulement*. As far as external appearances go, there is, of course, almost a perfect similarity between such an action and one due to viscosity.

But when a force of tension is applied, the case is wholly different. That intestine mobility which characterizes a truly viscous body, and enables one molecule to move round another while clinging to it, or one particle to advance while another slides in laterally to supply its place, being absent, the only way in which such a body can meet the requirements of a strain is by breaking, the fissures widening as the strain continues.

Thus, I think, we take account of all the facts adduced in proof of viscosity, and also furnish a satisfactory explanation of the other set of facts on which the opponents of the viscous theory have hitherto based their arguments.

Royal Institution, May 1858.

* The ingenious experiment of Mr. CHRISTIE with a bomb-shell filled with water and submitted to a freezing temperature, belongs, of course, to this class of effects.

XV. *On the Veined Structure of Glaciers; with observations upon White Ice-seams, Air-bubbles and Dirt-bands, and remarks upon Glacier Theories.* *By* JOHN TYNDALL, *F.R.S., Professor of Natural Philosophy, Royal Institution.*

Received February 24,—Read February 24, 1859.

§ 1. *Introduction.*

ON the 20th of May, 1858, I communicated a paper to the Royal Society, containing an account of observations made upon the Mer de Glace of Chamouni. In addition to the questions there discussed, another of great importance occupied my attention during my sojourn at the Montanvert, and that was the *veined structure* of the ice. To obtain information on this head, I visited almost every portion of the Mer de Glace and its tributaries; I examined the Talèfre and Léchaud glaciers, and spent several days amid the *seracs* of the Glacier du Géant. To investigate the connexion, if any, between the structure of the glacier and the stratification of its névé I ascended the Col du Géant, and afterwards inspected the magnificent ice-sections exhibited in the dislocations of the Grand Plateau and other portions of Mont Blanc.

During this investigation my convictions were by no means fixed; cases strongly suggestive of the influence of pressure, in producing the structure, came before me, and again other cases appeared which suggested, with almost equal force, the influence of stratification. The result, however, of the observations on the Mer de Glace was a strong opinion that *pressure* was the true cause of the phenomenon.

But I could not help feeling that the facts and arguments which I was in a position to bring forward would still leave the question an open one. They might influence the opinions of others, as they had influenced mine; but I had nothing to advance on which the mind could rest with perfect certainty. In short, neither the Mer de Glace nor its tributaries furnished facts capable of completely deciding the question. The subject being one on which a great deal had been written and retracted, I was unwilling to swell the bulk of the literature connected with it, while a possibility remained that what I had to say upon the subject might also require withdrawal. I therefore thought it better to wait another year; to extend the range of my observations, to visit glaciers in which the mechanical conditions of strain and pressure were different from those of the Mer de Glace. Thus by varying the circumstances, and observing Nature at work under different conditions, I hoped to confer upon the investigation the character and precision of an *experimental* inquiry.

The course of the inquiry in 1858 was as follows:—I first examined the glaciers of Grindelwald; crossing the Strahleck, I ascended the lower glacier of the Aar to the

Grimsel, thence to the glacier of the Rhone, thence to the great Aletsch glacier, in the neighbourhood of which I remained eight days. I afterwards spent eleven days at the Riffelberg, and explored the entire system of glaciers between the Monte Rosa and Mont Cervin. I thence proceeded to the Matmark Alp, and remained for five days in the vicinity of the Allalein glacier; I afterwards visited the Fée glacier, and completed the expedition by a visit to the Mer de Glace and its tributaries, and a second ascent to the summit of Mont Blanc.

The present paper contains the evidence derived from the sources thus opened to me; and I shall take these sources in the order in which they come before me; the evidence is therefore necessarily of a varied character, and it will I think be found conclusive. Besides those sections which are immediately devoted to the subject of structure, the paper contains others on the cause of the *flattening* of the air-bubbles in glacier ice, on the problem of glacier motion, and on the origin and cause of the Dirt-bands of the Mer de Glace.

§ 2. *General Aspect of the Veined Structure.*

The general appearance of the veined structure is well known. The ice of glaciers, especially midway between their mountain sources and their extremities, is of a whitish hue, owing to the number of small air-bubbles enclosed in the mass—the residue, doubtless, of that air which was originally entangled in the snow of which the ice is composed. Through the general whitish mass, however, at some places, blue veins are drawn, so numerous indeed, in some cases, as to cause the blue ice to predominate over the white. A laminated appearance is thus conferred upon the ice, the cause of the blueness being, that for some reason or other, the bubbles distributed throughout the general mass do not exist in the veins, or exist there in much smaller numbers.

In different glaciers, and in different portions of the same glacier, these veins exist in different stages of perfection. On the clean walls of some crevasses, and in the channels worked in the ice by glacial streams, they present a most beautiful appearance. They are not to be regarded as a partial phenomenon, or as affecting the constitution of glaciers to a small extent only. Vast masses of some glaciers are thus affected: by far the greater part of the Mer de Glace, and its tributaries, is composed of this laminated ice. The lower portion of the glacier of the Rhone, from the base of the ice cascade downwards, is entirely composed of it, and numerous similar cases might be cited.

To observe the structure of a glacier it is not even necessary to see the blue veins. Those who have ascended Snowdon, or wandered among the hills of Cumberland, or even walked in the environs of Leeds or other towns in Yorkshire and Lancashire, where the stratified sandstone of the district is used for architectural purposes, will have observed the exposed edges of the slate rocks, and of the stratified sandstone, to be grooved and furrowed by the action of the weather. In fact some portions of such rocks withstand the action of the atmosphere better than others, and these more resisting portions stand out in ridges while the softer portions between them are worn away. An effect exactly similar is observed upon the surface of the glacier. The laminated ice, exposed to the sun, and to wasting atmospheric influences, melts in a manner similar to the wasting of the rocks; little grooves and little ridges are formed upon the surface of the glacier, the latter being due to the more resisting ice, while the grooves are produced by the melting of the less resisting mass between them.

The consequence of this is, that the light dirt scattered by winds and avalanches over the surface of the glacier is gradually washed into the little grooves, thus forming fine lines, which to the practised eye are an infallible indication of the structure of the ice underneath. Visitors to the Jardin have ample occasion to observe these striæ, for they are finely shown upon the surface of the Mer de Glace between the Augle and Trelaporte. When they are followed until they are intersected by a fissure or a stream, it is seen that the superficial groovings always mark the direction of the veined structure within the glacier.

§ 3. *Structure and Stratification:—Marginal Structure.*

Opinions at present are very diverse as to the origin of these veins. Professor Forbes first regarded them as being caused by the freezing of water which filled fissures in the ice, but he now discards the notion of freezing, and supposes the "incipient fissures" to be closed by "time and cohesion." M. Agassiz despairs of rendering an account of them, but calls them "bands of infiltration." The Messrs. Schlagintweit have also treated the question, but with no greater success. In the paper published by Mr. Huxley and myself, pressure is referred to as the probable cause of the phenomenon, but we were unable at the time to furnish proofs of this. Apart from those who give public expression to their views upon the subject, I know that there are many who reject the pressure theory, and adopt instead of it the explanation that the blue veins of the glacier are merely the continuation of the strata of the névé; a view which has recently been upheld by Mr. John Ball in the Philosophical Magazine. The matter indeed so stands, that in a recent *résumé* of glacier investigations, Professor Mousson of Zürich omitted the subject of structure altogether, for the express reason that the question is still in complete obscurity.

I will not take up the time of the Society in discussing the vague, involved, and often absurd explanations which have been given of the blue veins of glaciers, but state broadly that the question now rests between the pressure theory, and that of stratification. Taking the parallel geological phenomena, the question then is, Does the veined structure of glaciers correspond to the *stratification* or the *cleavage* of rocks?

In reply to this question, I will remark, in the first place, that the veins are not always, nor even generally, such as we should expect from stratification. The latter ought to furnish us with distinct planes extending parallel to each other for great distances through the glacier; this is by no means the general character of the veins. We observe blue streaks, some a few inches, some a foot, and some several feet in length upon the walls of the same crevasse, and varying from a fraction of an inch to several inches in thickness. In many cases the blue spaces are definitely bounded, giving rise to the lenticular structure described by Mr. Huxley and myself, but more usually they lose themselves as pale washy streaks in the general mass of the white ice. In fig. 1, I have endeavoured to give an idea of a very common aspect of the veined structure. Such a structure is not that which we should expect from bedding.

Fig. 1.

Again, taking the Glacier du Géant as a representative case, we have first of all the slopes of the Col du Géant, the collectors of the snow by which the glacier is formed. The fissures on these slopes exhibit beautifully, to a certain depth, the horizontal stratification. The lines of bedding may be seen as far down as the summit of the great ice-fall between the Rognon and the Aiguille Noire; and on the castellated masses at the summit of this fall, to which the name *seracs* has been applied, the lines of stratification may be distinctly seen. Escaped from the confusion of the fall, the glacier flows gently through a long valley towards its junction with the Léchaud and Talèfre at the Tacul. Throughout the entire length of this glacier the planes of the structure are *vertical* or nearly so; sometimes they dip a little forward, but at other places they dip an equal quantity *backward*. Now let the mind figure, if it can, an agency which, as the mass descends the fall, shall turn up the horizontal strata of the Col du Géant and set them vertical, without a single break, throughout *the entire length* of the Glacier du Géant, and I imagine the effort to conceive of such an agency will be followed by the conviction that the change indicated is inconceivable.

Further, we often find, in the central portions of a glacier, the structure feeble, or scarcely developed at all, while at the sides it is well developed. This is often the case where the glacier moves through a valley of tolerably uniform inclination, and where no medial moraines occur to complicate the phenomenon. But if the veins mark the bedding, there seems to be no reason why we should not find them as clearly defined at the centre as at the sides; the fact, however, certainly is that we do *not* so find them.

Let me here show the true significance of this fact. If a plastic substance, such as mud, flow down a sloping canal, the central portions will flow more quickly than the lateral ones which are held back by friction. Now the flow may be so regulated that a circle stamped upon the central portion of such a mud-stream shall move downwards *without sensible distortion*, thus proving that the central mud is neither compressed nor stretched longitudinally; for if the former, the circle would be *squeezed* to an ellipse with its major axis transverse to the axis of the stream; and if the latter, it would be *drawn out* to an ellipse with its major axis parallel to the line of flow. A similar absence of longitudinal compression exists in many glaciers, and *in such ice-streams there is no transverse central structure developed.*

But let a circle be stamped upon the mud-stream near to its side; owing to the speedier flow of the centre, this circle must be distorted to an ellipse, because the part of the circle furthest from the side moves more quickly than the part nearest the side. Hence we shall have an ellipse formed with its major axis inclined downwards, indicating that the mud is compressed in one direction and expanded in another. An exactly similar state of things occurs in many glaciers; the ice near the sides is subjected to a pressure and tension like that here indicated, and we have marginal crevasses as the result of the tension, while the veined structure is, at all events, found associated with the pressure.

Fig. 2.

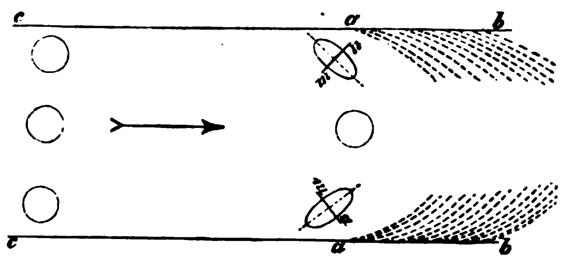


Fig. 2 will perhaps render my meaning more intelligible, in which *cb*, *cb* represents the sides of a glacier moving in the direction of the arrow. Here, while the central circle retains its shape, the side ones are squeezed and drawn out to ellipses. Marginal crevasses occur parallel to the lines *m n*, or perpendicular to the tension, while the dotted lines mark the direction of the blue veins which are at right angles, or nearly so, to the crevasses. I have dotted the line marking the direction of the structure along the margins *ab*, *ab*. In connexion with this point, I would refer to the instructive papers of Mr. HOPKINS*, who has shown that in glaciers which move through valleys of uniform width, the directions of maximum pressure and tension are at right angles to each other, each of them enclosing an angle of 45 degrees with the side of the glacier.

I have simply said that the structure in the case described is "associated with the pressure;" thus confining myself within the strict limits of the facts. But what has been said shows that the pressure theory affords, at all events, a possible solution of a difficulty, which, without violence to fact, is inexplicable upon the hypothesis of stratification; the difficulty, namely, that a finely developed structure often exists along the margin of a glacier, while it is excessively feeble, or entirely absent, in the central portions.

§ 4. *Transverse Structure.—Glaciers of Grindelwald, the Rhone, &c.*

In many cases, however, the structure is not thus limited to the margins, but sweeps across the glacier from side to side, without interruption, being as well developed at the centre as at the margins. The stratification theory is wholly incompetent to account for this; the pressure theory requires that to produce this transverse structure the glacier must, at some portion of its route, have been forcibly compressed longitudinally. It was not till after my return from the Mer de Glace in 1857, that the full mechanical significance of *a change of inclination* in the glacier occurred to me.

Bend a prism of glass, we have compression on one side and extension on the other,

* Philosophical Magazine, 1845, xxvi. p. 148. See also Proceedings of the Royal Institution, vol. ii. p. 324.

with a neutral axis between, the mechanical conditions of the mass being shown by its action on polarized light. The same is true of any other substance,—the concave surface of the bent prism is compressed. Now at the bases of steep glacier slopes, where the inclination suddenly changes, we have a case of this bending, and along with it a thrust of the mass behind. The concave surface is turned towards us, and that surface is thrown into a state of compression corresponding to the thrust, and to the change of inclination. Hence it occurred to me, that the bases of the ice-falls, where the requisite change of inclination occurs, were likely to be the manufactories of the transverse structure. The experience of 1858 completely verified this idea.

In illustration of my position I will take a representative case; and to render my observations capable of being easily checked, I will choose one of the most accessible glaciers in the Alps,—the lower glacier of Grindelwald. One portion of this glacier descends from the Viescherhörner; but there is another branch which descends from the Schreckhorn, Finsteraarhorn and Strahleck, and it is to this latter branch that I now wish to direct attention.

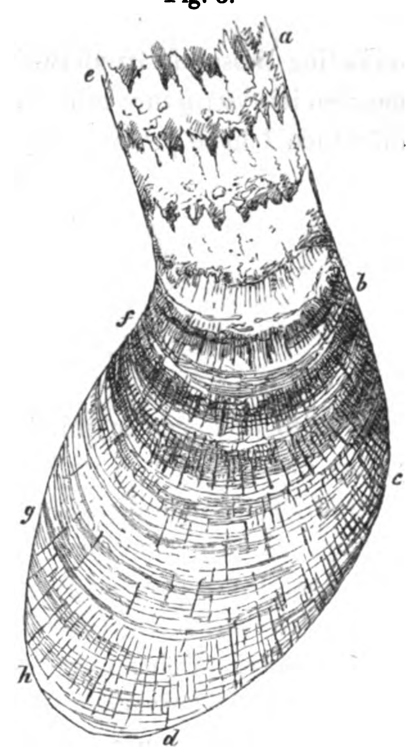
Walking up this glacier from its place of junction with the tributary from the Viescherhörner, we come at length to the base of an ice-fall which forbids further advance upon the ice. Let the glacier be here forsaken, and let the flanking mountain side, either right or left, be ascended, until a position is attained which affords a complete view of the fall and of the glacier stretching downwards from the base of the fall. The view from such a position will furnish a key to the development of the transverse structure.

It is, in point of fact, a grand *experiment* which Nature here submits to our inspection. The glacier, descending from its névé, reaches the summit of the fall and is broken transversely as it crosses the brow. It descends the fall as a succession of broken cliffy ridges, with transverse hollows between them. In these latter the ice débris and the dirt collect, partially choking up the fissures formed in the first instance. Carrying the eye downwards along the fall, we see, as we approach the base, these sharp ridges toned down, and a little below the base they dwindle into rounded protuberances which sweep, in curves, across the glacier. At the centre of the fall there is not a trace of the true structure to be observed. At the base of the fall it *begins* to appear,—at first feebly, but soon becomes more pronounced; until finally, at a short distance below the fall, the eye can follow the structural groovings right across the surface of the glacier, while the mass underneath has become correspondingly laminated in the most beautiful manner.

It is difficult to convey, by writing, the force of the evidence which the actual observation of this great experiment places before the mind. The ice at the base of the fall has to bear the powerful thrust of the descending mass; but more than this, the sudden change of inclination which it suffers throws its upper portion into a state of violent longitudinal compression. The protuberances are squeezed more closely together, the hollows between them wrinkle up in submission to the pressure; the whole aspect of the glacier here gives evidence of the powerful exertion of the latter force; and exactly at the place where *it* is exerted, the structure makes its appearance, and being once manufactured, is sent onwards, giving a character to other portions of the ice-stream which have no share in its production.

An illustration, perhaps equally good and equally accessible, is furnished by the glacier of the Rhone. Above the great icefall which the traveller descending from the Furca has to his right, the horizontal bedding is exhibited in a more or less perfect manner, to a certain depth, upon the walls of the huge and numerous crevasses here existing. I have also examined this fall from both sides, and an ordinary mountaineer will find no difficulty in reaching a spot nearly opposite the centre of the fall, from which both the fall itself and the glacier below it are distinctly visible. Here a similar state of things to that already presented to his view reveals itself. The fall is *structureless*; the cliffy ridges are separated from each other by transverse hollows, following each other in succession down the slope; those ridges are toned down to protuberances at the base of the fall, becoming more and more subdued, until low down the glacier the transverse swellings disappear. As in the case of the Grindelwald glacier, the squeezing of the protuberances and of the spaces between them is visibly manifested. *Where this squeezing commences the transverse structure also commences*, and in a very short distance reaches perfection. All the ice that forms the lower portion of the glacier has to pass through this *structure mill* at the base of the fall, and the consequence is that *it is all laminated.*

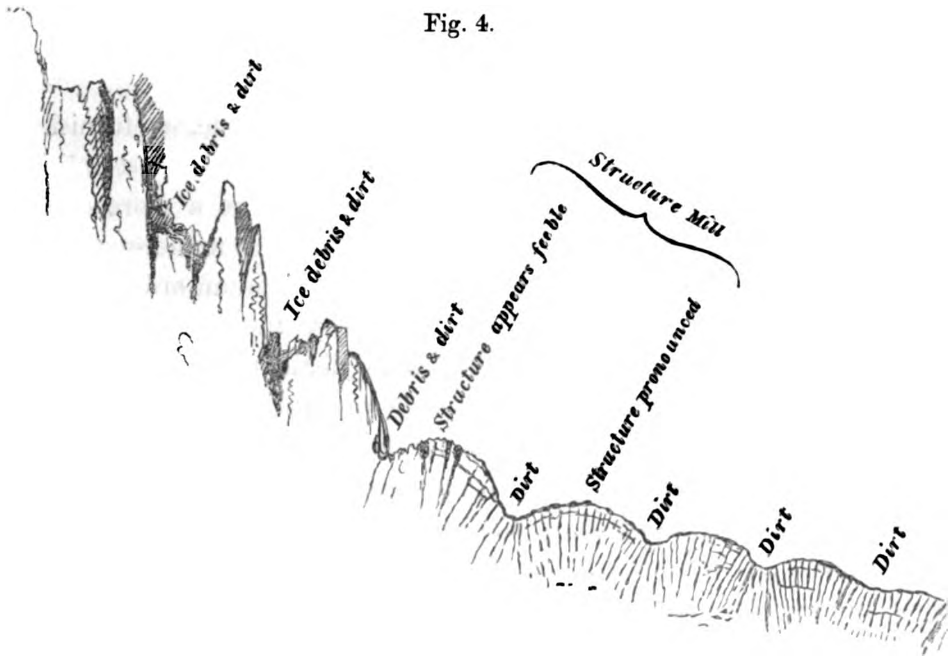
Fig. 3.



The case will be better appreciated by reference to figs. 3 and 4, the former being a sketch, in plan, and the latter a sketch in section of a part of the ice-fall and of the lower portion of the glacier of the Rhone. *a e b f* is the gorge of the fall, and *f b* its base. The transverse ridges are shown crossing the fall, being subdued at the base to protuberances, which gradually disappear further down the glacier. The "structure" sweeps across the glacier in the direction of the fine curved lines. On the plan I have also endeavoured to show the radial crevasses of the glacier; they are at right angles, or nearly so, to the structure. As would be inferred by those acquainted with what I have already written upon the influence of curvature, the side *b c d* of the glacier is much more violently crevassed than the side *f g h*.

Fig. 4 shows the cliffy ridges of the fall, and of the rounded protuberances below it, in section. The shading lines below denote the structure. The protuberances are so powerfully squeezed in some cases that they scale off at their surfaces. Fig. 5 is a representation of such scaling off which I have observed at the bases of several cascades,

Fig. 4.



including those of Grindelwald, the Rhone, the Rognon, and the Talèfre, each of which has also its "structure mill" at the base of its cascade. Fig. 5*a* is an example of scaling off which I have produced by artificial pressure.

Fig. 5.

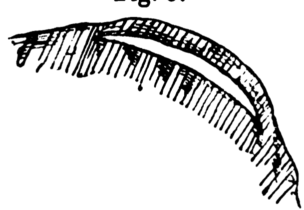
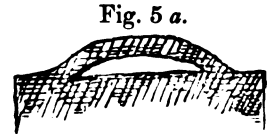


Fig. 5 *a*.



It is to be borne in mind that the structure once formed prolongs itself into places which have no part in its formation; it would therefore be hasty to infer the relationship of structure and pressure from an observation of them at a particular portion of the glacier. I have sometimes seen the veined structure parallel to the crevasses for a short distance: there are some transverse crevasses on the Glacier du Géant a little above Trelaporte which illustrate this; but it would be altogether erroneous to infer from this that the law which makes the structure perpendicular to the pressure, and hence, as a general rule, transverse to the crevasses, finds an exception here. It is perfectly manifest that the structure which is brought into this unusual relationship to the crevasses has been developed far higher up; that the change of conditions from longitudinal pressure to longitudinal strain is too weak and transitory to obliterate it. To effect obliteration, a force commensurate with that which produced the structure must be brought into play, and at the place now referred to no such force exists.

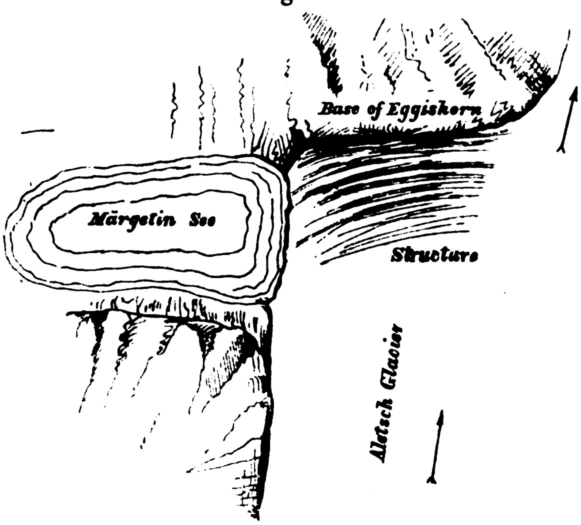
§ 5. *The Aletsch Glacier.*

Having made the foregoing observations upon the glacier of the Rhone, I proceeded to the Aletsch glacier, and during a residence of eight days at the hotel upon the slope of the Æggischhorn, made frequent excursions upon the ice. I had never previously seen this grand ice-stream, and my interest in it, at the time of my visit, was greatly augmented by the arguments which Mr. JOHN BALL had founded upon its deportment against the pressure theory of the veined structure. I shall here limit myself to a few brief remarks upon this subject.

I have already stated, and this must be particularly remembered, that the veined structure often appears in places which have no share in its production. The longitudinal structure in the centre of the stream of the Aletsch, for four miles above the base of the Æggischhorn, is not due to the lateral pressure endured by the glacier during these four miles. It is due, as Mr. BALL himself suggests, to the mutual thrust of the branch glaciers, which unite to form the trunk stream; and, once formed by this thrust, it perpetuates itself throughout a great portion of the trunk stream.

But it is urged against this view, that pressure exerted in new directions—the longitudinal pressure, for example, endured by the stream in its descent, and acting through long periods, ought,—if pressure has the power ascribed to it, to obliterate the first structure. Now here, again, it must be remembered that it is the portions of the ice near the bed of the glacier that yield, and that the upper portions of the ice, in many cases, are simply *floated* upon the moving under portions. Were the uniform "long reach" referred to by Mr. BALL strictly examined, it would, in all probability, be found that the ice near the surface is no more compressed than a log of timber would be if placed upon the glacier, and permitted to share its motion downwards.

Fig. 6.



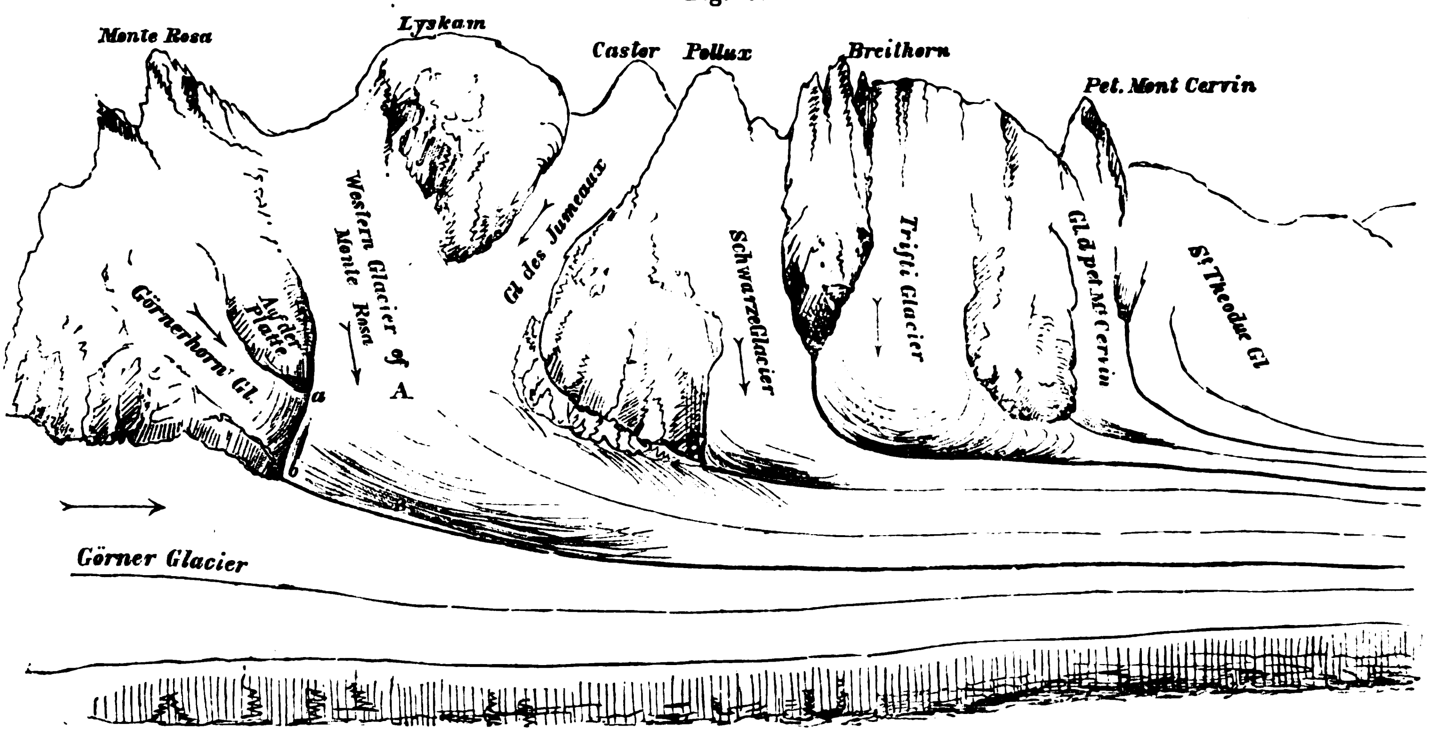
I may sum up by saying that a close examination of the glacier satisfied me, not only that it presented no phenomena which were at variance with the pressure theory, but also exhibited some which, as far as I could see, were perfectly fatal to the theory of stratification. The state of the ice at the base of the Æggischhorn, as shown in fig. 6, is certainly quite in harmony with the pressure theory; another fact observed upon the glacier shall be referred to at a future page.

§ 6. *Glaciers of Monte Rosa.*

I will next endeavour to describe the phenomena of structure exhibited in the system of glaciers in the neighbourhood of Monte Rosa. The general mechanical conditions of these glaciers will be evident to an observer stationed upon the Görner Grat, a point of view well known to travellers, and famous for the magnificence of the panorama which it commands.

As the observer stands here, facing Monte Rosa, the great Görner glacier, coming down from the heights of the old Weissthor at his left, flows beneath him. It is joined, in its course, by a series of glaciers from the sides of the opposite range of mountains. First of all comes the western glacier of Monte Rosa, which really ought to give its name to the trunk stream, as it is the most considerable of its tributaries. Into the glacier of Monte Rosa, and before the latter reaches the trunk valley, a glacier from the Twins, Castor and Pollux, pours its contents. Afterwards we have the Schwarze glacier, which lies between the Twins and the Breithorn; then the Trifti glacier, which lies upon the flank of the Breithorn, and afterwards the glaciers of the little Mont Cervin and of St. Theodule. The accompanying sketch (fig. 7) will render intelligible what I have to say regarding these glaciers.

Fig. 7.



The small Görnerhorn glacier, which comes down the sides of Monte Rosa, is a very singular one. In comparison with the western glacier of Monte Rosa its mass is insignificant, and it is abruptly cut off by the latter along the line *a b*, a moraine occurring here, which may be regarded as forming, at once, the *terminal* moraine of the one glacier and the *lateral* moraine of the other. Thus the smaller glacier coming down the mountain side abuts against its more powerful neighbour, and we should infer from the inspection of the glacier that its terminus is subjected to great pressure.

Let the observer now suppose himself transported to the Görnerhorn glacier, at some distance above the terminal moraine *a b*; he will find there the transverse structure, if at all developed, excessively feeble and defective; let him now walk downwards towards the moraine *a b*: every step he takes brings him to a place where the ice is subjected to a greater pressure, and every step also brings him to a better structure: both phenomena go hand in hand. At the end of the glacier, alongside the terminal moraine and under it, the structure is finely developed. If the observer now cross the glacier and ascend the rocks called *Auf der Platte*, from which he can command a near view of the Görnerhorn glacier, and embrace a large portion of it, he will be able to observe the gradual perfecting of the structure as the region of pressure is approached. Towards the extremity of the glacier the surface becomes wrinkled, the groovings denoting the structure become more and more pronounced, the dirt striæ being more closely squeezed together; and from these external aspects he may infer, with certainty, the gradual perfecting of structure within the glacier.

The western glacier of Monte Rosa next commands our attention. This great stream occupies the valley between Monte Rosa and the Lyskamm, receiving the snows of the opposite sides of both. The branch of the Görner glacier coming down from the Weissthor throws itself across the flow of its powerful neighbour, and deflects the latter, both of them afterwards moving together down the trunk valley, with a moraine, as usual, between them.

Before quitting the "Platte," we will suppose that the observer has endeavoured to form some idea of the mechanical conditions of the Monte Rosa glacier. He would see the mass arrested in its descent by the Görner glacier, and compelled to accompany the latter. A certain component of the weight of the glacier is borne by the ice where it comes into contact with the Görner glacier. The observer would infer, from mere inspection, that if the structure be due to pressure, it ought to be most fully developed near the moraine which separates the Monte Rosa from the Görner glacier.

If he now pass from the "Platte" to the ice, and cross to the centre of the Monte Rosa glacier to A, he will find the structure there excessively feeble, if at all developed. Let him now walk straight down the glacier towards B, where the pressure is most intense. Every step he takes downwards brings him to more perfectly veined ice; and I am not acquainted with a more splendid example of laminated structure than that exhibited by this glacier along the moraine, and for some distance from it, at its southern side.

The system of glaciers which next come under review are exceedingly instructive. In no place in the whole range of the Alps are the effects of pressure and the phenomena of structure more strikingly exhibited. I have endeavoured, in the sketch, to render the aspect of these glaciers intelligible. The Schwarze glacier moves down a steep mountain slope, and welds itself to the Monte Rosa glacier at the bottom. But the great mass of this latter enables it to pursue its way without being compelled to swerve sensibly by its feebler neighbour. The latter is forced to bend abruptly, and from a wide irregularly-shaped field of névé, it is squeezed between the Trifti and the Monte Rosa glaciers to the narrow band represented in the figure, and moves thus downwards.

The Trifti glacier itself is perhaps a still more striking illustration of the power of ice to yield when subjected to pressure for a long period. The aspect of the real glacier is much the same as that shown upon the sketch. *It* also is compelled to change its direction, and to flow as a narrow stripe along the trunk valley, being hemmed in between the strip of the Schwarze glacier and that of the glacier of the little Mont Cervin. A beautiful system of bands is to be seen at the lower portion of this glacier.

The inspection of the sketch will show better than words the modifications of shape which the lower portion of the glaciers undergo by the pressure of their higher portions, and the resistance of the trunk stream. They are turned aside, firmly welded together, and form a series of parallel narrow bands, separated from each other by moraines. They are all well seen either from the Görnergrat or the summit of the Riffelhorn.

I have examined each of these glaciers, and find the same to be true of all of them. High up the structure is feeble; as we descend it becomes more pronounced, and at the places where the tributaries join the trunk, and the ice has to bear the full thrust of the mass behind it, we have a finely developed structure.

§ 7. *Coexistence of Structure and Stratification.—The Furgge Glacier.*

The evidence of the association of pressure and glacier lamination which I have thus far laid before the Society, will, I think, be admitted to be very strong. I have no hesitation in saying that the stratification theory has nothing to urge at all to be compared with it in point of cogency. Still I cannot help feeling how a critical and well-informed mind might weaken the force of what I have adduced. Difficult as the conception is, it might be urged that the structure, so fully developed near the margins of glaciers, may be due to a turning up of the strata edgeways, in consequence of a wide névé being squeezed into a narrow channel,—just as a sheet of paper, if forced through a groove less than itself in width, would turn up at its edges. It might also be urged that the structure developed alongside and under the medial moraines, is due to the placing side by side of these folded-up strata; the perfect welding of both and the clearer development of the structure being conceded as possible consequences of the mutual pressure. This indeed is Mr. BALL's view of the subject; and M. AGASSIZ assumes such a folding-up of the strata of the Unteraar glacier. With regard to the transverse structure also, it might be said that we do not know how the interior of the mass is affected in descending the ice-falls. The mind, it is true, finds great difficulty in conceiving of any agency which could set the strata which were horizontal above the fall, vertical below it; still this difficulty may be due to our ignorance of the mechanical conditions of the mass during its descent. In this way it would be quite possible to fritter away conviction to a mere opinion, and hence arose a strong desire on my part, either to confirm these surmises, or to place the pressure theory, once for all, beyond the power of such attacks.

One conclusive observation is still wanting to establish the analogy between glacier lamination and the cleavage of slate rocks. In the latter case the arrangement of the strata has been traced by their organic remains; and, indeed, stratification has often been visibly exhibited coexistent with cleavage, both crossing each other at a high angle. If a similar state of things could be detected upon a glacier, it would at once lay the axe to the root of all the scruples above referred to, and place the pressure theory upon an unassailable basis. The consciousness of this was sufficient to stimulate me in the search of such evidence.

I had visited all the glaciers hitherto mentioned, and others not mentioned, without obtaining more than one clear case of the kind: this case I observed upon the Aletsch glacier on the 6th of August. Not far from the junction of the Middle Aletsch glacier with the trunk stream, a crevasse exposed a wall of ice 50 or 60 feet in height, upon which the stratification was exposed, and cutting the stratification at a high angle were the groovings which marked the true veined structure. The association was distinct; my friend Professor RAMSAY was with me at the time; I drew his attention to the fact, and to him the case was perfectly conclusive. Thus the Aletsch glacier, which had been referred to by Mr. BALL as furnishing evidence against the pressure theory, gave us a fact, which, as far as I could see, was perfectly fatal to the theory of stratification.

But the case was solitary, and although inspiriting at the moment, its effect upon the mind became feeble as time passed, and no repetition of the observation occurred. I had remained at the Riffel from the 9th to the 18th of August, exploring all the adjacent glaciers, and adding each day to my stock of knowledge; but I met no case in which the structure and the bedding were so clearly and independently exhibited, as to leave an adherent of the stratification theory no room for doubt. Wednesday the 18th of August was to be my last day at the Riffel, and it was devoted to the examination of the Furgge glacier, which occupies the space between the pass of St. Theodule and the Matterhorn.

Crossing the valley of the Görner glacier, I climbed the opposite mountain slope, and passing the Schwarze See, soon came upon the glacier referred to. I walked up it until I found myself in a kind of *cul de sac*, flanked by precipitous ice-slopes, and opposed in front by a cascade composed of four high terraces of ice. The highest terrace was composed principally of broken cliffs and peaks of ice, and it had let some of its frozen boulders fall upon the platform of the second terrace, where they stood like rocking-stones on the point of falling. The whole space at the foot of the fall was covered with quantities of crushed ice, while some coherent masses, upwards of 200 cubic feet in volume, were cast to a considerable distance down the glacier.

Upon the face of the terraces the stratification of the névé was beautifully shown. Above the fall the névé extends as a frozen plain, quite undisturbed, so that the bedding took place with great regularity; and being broken through for the first time at the summit of the fall, the lines of stratification were peculiarly well defined and beautiful.

Towards the right of the fall, looking upwards, this was particularly the case; for here no pressure had been exerted upon the beds sufficient to contort them or to rupture their continuity.

The figure of a vast lake pouring its waters over a rocky barrier, which curves convexly upwards, thus causing the water to rush down it, not only longitudinally over the vertex of the curve, but also laterally over its two arms, will convey to the mind a tolerably correct conception of the appearance of the fall. Towards the centre the ice was powerfully squeezed; the beds were *bent*, and their continuity often ruptured, so as to exhibit faults; but they were as plain, and as easily traced, as in any other portion of the fall. I thought I saw structural groovings running at a high angle to the stratification. Had the question been an undisputed one I should have felt *sure* of this, for the groovings were such as always mark the structure. The place being dangerous, I first observed it from a little distance through my opera-glass; but at length, resigning the instrument to my guide, and leaving him to watch the tottering blocks overhead, and to give me warning in case of their giving way, I went forward to the base of the fall, peeled the grooved surface away with my axe, and *found the true veined structure underneath*, running, in this case, nearly at right angles to the stratification.

The superficial groovings were not uniformly distributed over the whole face of the terrace, but occurred here and there where the ice had yielded most to the pressure. I examined several of these places, and in each instance found the superficial grooving to be the exponent of the true veined structure underneath, the structure being in general nearly *vertical*, while the lines of bedding were *horizontal*. The coarse bands which marked the division of the beds were also seen underneath, when the surface of the ice was removed. Having perfectly, and with deliberation, satisfied myself of these facts, I made a speedy retreat; for the ice blocks were most threatening, and the time of day that at which they fall most frequently.

We now resolved to try the ascent of the glacier to the right; it was much riven, but perfectly practicable to a good iceman. To me it was also perfectly delightful; in fact, as regards the relationship of structure and stratification, this glacier taught me more than all the others I had visited taken together. Our way lay through fissures which exposed magnificent sections, and every step forward added further demonstration to what I had already observed at the base of the fall. The bedding was perfectly distinct, and the structure equally so, the one being at a high angle—sometimes at a right angle—to the other. Among these crevasses the pressure was in some cases greater than on the fall, and the structure proportionally more pronounced. The crumpling of the beds demonstrated the exercise of the pressure, and the structure went straight through such crumplings, thus furnishing me with numerous parallels to the case observed by Professor SEDGWICK, Mr. SORBY, and others, of the passage of slaty cleavage through contorted beds. Indeed I question whether the phenomena of cleavage and bedding, in the case of slate rocks, were ever exhibited, side by side, with a distinctness equal to that of the stratification and "structure" of ice in the present instance.

Fig. 9 represents a crumpled portion of the ice, with the lines of lamination passing through those of bedding at a high angle. Fig. 10 represents a case where a fault

Fig. 9.

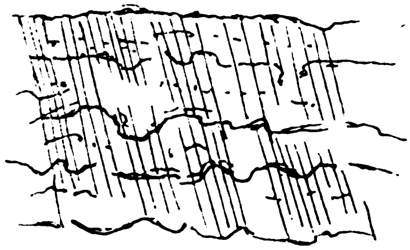
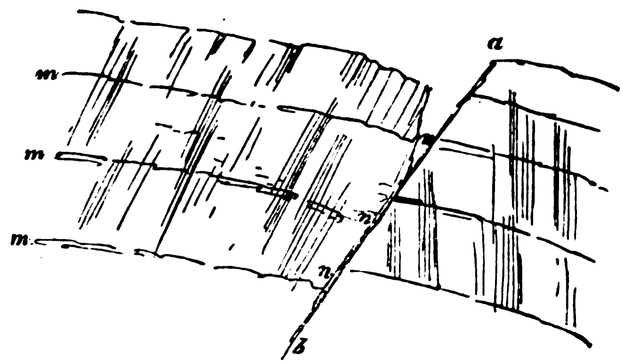


Fig. 10.

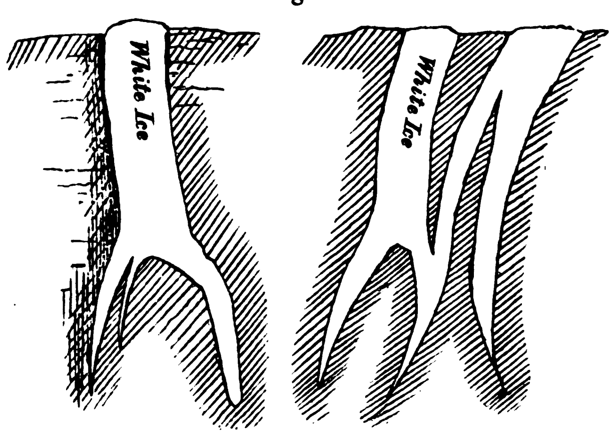


occurred, the veins at both sides of the line of dislocation *ab* being inclined towards each other. The lines *mn*, *mn* represent of course the lines of bedding, and the lines crossing them the structure. These observations are conclusive as regard the claims of the rival theories of structure and stratification*.

§ 8. *On the White Ice-seams of the Glacier du Géant, and their relation to the Veined Structure.*

From an elevated point at Trelaporte I observed a remarkable system of white bands sweeping across the Glacier du Géant in the direction of the structure. From one of the moraines near the junction of the three tributary glaciers, the same system of bands present a very striking appearance. They consist of a hard white ice, more resistant than the general mass of the glacier, and in some cases rising to a height of three or four feet above the surface. On close examination I found that they penetrated the

Fig. 11.



glacier only to a limited depth. In fig. 11 I have given the sections of two of these

* While correcting this proof, I find a case of this kind figured by M. Agassiz in the atlas to his 'Système Glaciaire,' pl. 8. fig. 3.

veins, about 15 feet deep, which were exposed on the walls of a crevasse high up the glacier. They constituted a kind of *inverted glacier trap*, and I was led to a knowledge of their origin in the following way.

In one of my earliest visits to the base of the ice-fall of the Talèfre, I observed a curious disposition of the veined structure on the walls of some of the crevasses: fig. 12 represents one case of the kind, and fig. 13 another, and numerous similar ones find a place

Fig. 12.

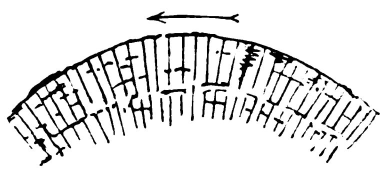
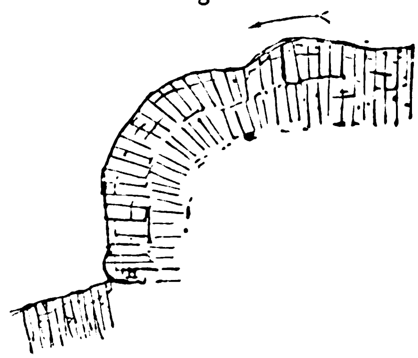


Fig. 13.



in my note-book. In the former case the veins fell *backward* as well as forward, being vertical through the central portion of the curve. In fig. 13 the position of the veins varies in a very short distance from the vertical to the horizontal.

I found that the portions of ice which showed the phenomena, formed, when seen from a point of view sufficiently commanding, a part of a system of crumples or protuberances which swept round the base of the fall, between the moraine which descends along it from the Jardin, and its highest lateral moraine. I have already referred to the protuberances which sweep across the Strahleck branch of the Lower Grindelwald glacier, and of those of the glacier of the Rhone: those to which I now refer were of the same character.

Right and left from the position where the crumples were most pronounced they gradually became subdued, shading off to a mere undulating surface; the sides of a crevasse intersecting this surface longitudinally presented the structural arrangement shown in fig. 14. It will be observed that the directions of the veins change in accordance with the undulations of the surface.

Supposing the squeezing of the mass to become so violent that the gentle undulations shall become steep crumples, the deviation of the structure from parallelism with itself would, of course, be augmented. This prepares us to understand the exact phenomena observed at the base of the Talèfre cascade. Fig. 15 represents a series of crumples following each other in succession at the place referred to; at the base of each I found a vein of *white* ice, *a*, *a*, wedged into the mass. This interrupted the continuity of the structure; the abrupt change in its direction at opposite sides of the white band being, as shown in the figure, in every case observed.

I found that the width of the seams was exceedingly irregular, varying, at different portions of the same seam, between 6 inches and 3 or 4 feet. I also found that a seam sometimes became forked so as to form two branches, which thinned gradually off until

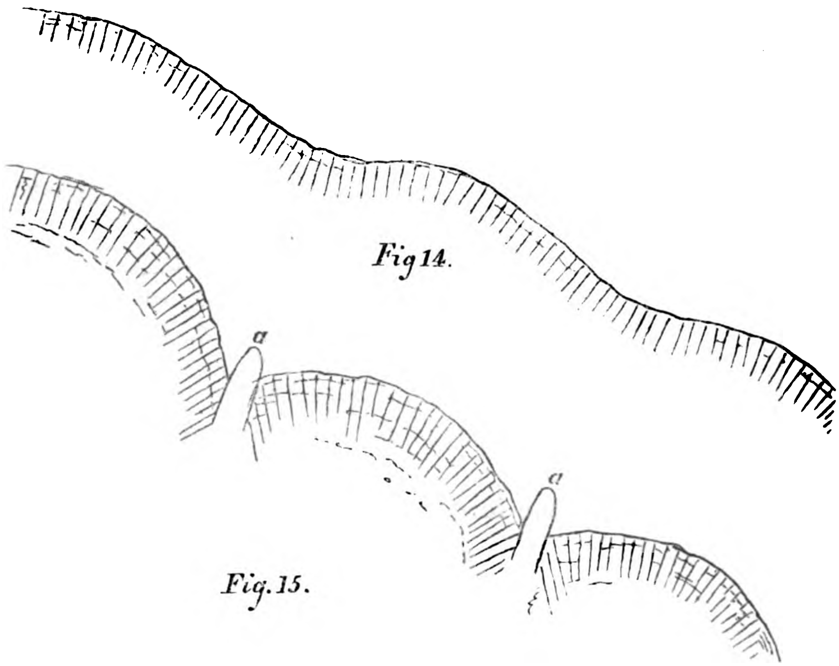
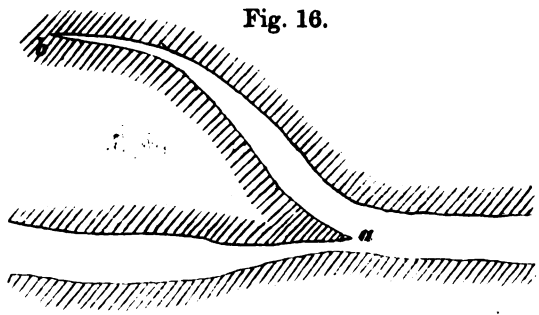


Fig 14.

Fig. 15.

they finally vanished. Fig. 16 is an example of this kind: the seam was divided at the point *a*; one of its branches *ran up the face of the crumple*, thinned off and disappeared at *b*; the other widened considerably, but finally thinned off and also vanished.

Fig. 16.

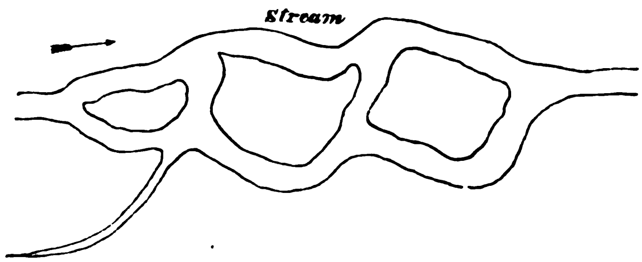


Along the bases of the crumples the fillets of water which poured down their faces were collected and flowed. The streams thus formed ran in many cases alongside the existing veins of white ice, and had worn for themselves deep channels in the glacier. The thought soon suggested itself, that the seams themselves were formed by the gorging up of those channels by snow in winter, and the subsequent consolidation of this snow during the descent of the glacier. Indeed the channels of the streams seemed the exact matrices of the seams of white ice*.

* The fact of one branch of a vein running up the face of a crumple, seems to prove that the ice, which at one time constitutes the *base* of a crumple, does not always remain so; the bases of the crumples are sometimes *lifted up* by the squeezing. The *horizontal* structure at the fronts of many of the crumples seems due to a local forcing forward of one protuberance over that next below it. Were the matter tested by strict measurement, I think it would be found that different portions of the crumples move downwards with different velocities. According to this view, upon the general motion of the glacier there are *local* motions superposed.

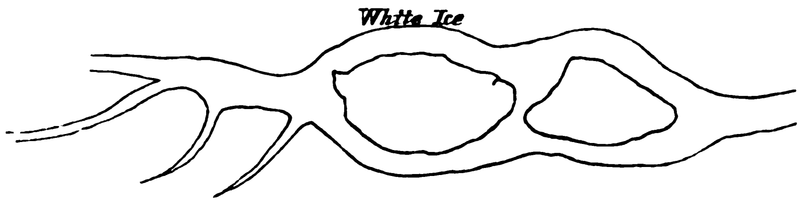
I afterwards traced the seams of white ice of the Glacier du Géant to their origin amid the ridges and hollows at the base of the great ice-fall of Le Rognon. In some cases the seams opened out into two branches, which, after remaining for some distance separate, would unite again so as to enclose a little glacial island; at other places lateral branches were thrown off from the principal seam, presenting the form of a glacier stream which had been fed by tributary branches. Fig. 17 is the plan of an actual

Fig. 17.



stream observed at the base of the ice-fall; fig. 18 is the plan of a seam of white ice

Fig. 18.



observed the same day lower down the glacier; their relationship is evident. I may remark that I have observed other seams produced by the gorging of *crevasses* with snow, and the subsequent closure of the fissures.

Considering the place where they are formed, these channels cannot escape compression; but let me remove all uncertainty on this point, by proving that not only at the base of the seracs, but throughout almost its entire length, the Glacier du Géant is in a state of longitudinal compression.

The first proof I have to offer is that the transverse undulations of the glacier, to which reference has been so often made, become gradually *shorter* as they descend. A series of three of them, measured along the axis of the glacier on the 6th of August 1857, gave the following respective lengths—955, 855, and 770 links, the shortest undulation being the furthest down the glacier. Now these undulations, as I shall subsequently show, are due to a regularly recurrent action, and are doubtless originally of the same length; that the lower ones are shorter than the higher must therefore be due to compression.

The following observation is, however, more conclusive. About three-quarters of a mile above the Tacul, and to the left as we ascend, there is a green patch upon the craggy mountain side. From this spot, as a station, I set out with a theodolite a line (No. 1) transverse to the axis of the glacier.

From a station lower down, chosen in a couloir along which the stones are discharged from the end of a secondary glacier which hangs upon the slope of Mont Tacul, I set out a second line (No. 2) transverse to the axis of the glacier.

A third line (No. 3) was set out across the glacier about a quarter of a mile still lower*.

The mean daily motion of the centres of these three lines is given in the annexed Table, and also their distances apart.

Mean daily motion of three points upon the axis of the Glacier du Géant.

	inches.	Distances apart.
No. 1	20·55	
		. 2477 links.
No. 2	15·43	
		. 2215 links.
No. 3	12·75	

The advance of the hinder lines upon these in front is most strikingly shown by these measurements; and the proof that the Glacier du Géant is in a state of longitudinal compression is thus complete.

Here then we have a vast ice press, and here we have the pure snow filling the transverse channels of the streams. We are thus furnished with an experimental test on a grand scale of the pressure theory of the veined structure. In 1857 I examined a great number of these seams of white ice, and found in many of them *a finely developed lenticular structure.* In 1858 I also examined the seams, and found some of them "ribboned" in the most exquisite manner by the blue veins; indeed I had never seen the veins more sharply and beautifully developed.

This structure was observed in portions of the seams at and near the centre of the glacier, where the differential motion observed at the sides does not exist. This fact, I think, throws grave difficulties in the way of any theory which makes the veined structure *dependent on differential motion*, and more especially a theory which requires "*a very considerable amount of this differential motion* to produce any sensible degree of stratification in the vesicles."

§ 9. *On the flattening of Air-bubbles in Glacier Ice, and its relation to the Veined Structure.*

Those who have given their attention to the subject, know that the bubbles contained in glacier ice are, in general, not spherical, but *flattened*; and that from their shape conclusions of the greatest import have been drawn regarding the internal pressures of glaciers.

M. Agassiz draws attention to this subject in the following words:—"The air-bubbles undergo no less curious modifications; in the neighbourhood of the névé, where they are most numerous, those which one sees on the surface are all spherical or ovoid, but by degrees they begin to be flattened, and near the end of the glacier there are some that are so flat *that they might be taken for fissures when seen in profile.* The drawing,

* These three lines are drawn upon the sketch map at page 268 of Part I. of these researches (FF′, GG′, HH′).

fig. 10, represents a piece of ice detached from the gallery of infiltration; all the bubbles are greatly flattened. But what is most extraordinary is, that far from being uniform, *the flattening is different in each fragment*; so that the bubbles, according to the face which they offer, appear either very broad or very thin. I know of no more significant fact than this, *since it demonstrates that each fragment of ice is capable of undergoing in the interior of the glacier a proper displacement independently of the movement of the whole.*

"The same flattening of the bubbles," continues M. AGASSIZ, "is found at a greater depth. While engaged in my boring experiments, I observed attentively the fragments of ice brought up by the borer. I found in them almost flat bubbles, perfectly similar to those of the fragment figured above, at all depths from 10 to 65 metres. It follows *hence* that *a strong pressure is exercised on the interior of the glacier.*"

The description of the "flattening" here given is correct: all observers agree in corroborating it, and every observer with whom I am acquainted draws substantially the same conclusion from the phenomenon that M. AGASSIZ does. Professor THOMSON'S speculation upon the subject is particularly refined and ingenious.

Mr. JOHN BALL converts the flattening of the bubbles into evidence against the pressure theory of the structure in the following way:—"As AGASSIZ has pointed out," writes Mr. BALL, "and I have frequently verified his observations upon this point, though the air-cavities show traces of compression reducing them to the form of flattened lenses, the directions in which they are flattened are most various, *and show no constant relationship to the planes of the veined structure.* Here then we have direct evidence that separate portions of the ice have been acted upon by pressure sufficient in amount to modify their internal arrangement, but that these pressures have not acted in the same, or nearly the same direction."

Granting the inference that the observed flattening "furnishes direct evidence" of pressure, the foregoing argument would, I confess, be a very formidable one. If the bubbles are thus flattened by pressure, and if the veined structure, as I contend, be the result of pressure, and approximately at right angles to the direction of the force, we ought to have the bubbles squeezed out in planes parallel to the structure. The fact that the bubbles are not so squeezed out, would then afford a strong presumption that the structure is not produced by pressure. I expect, however, to be able to prove that the shape of the bubbles is *not* a "direct evidence" of pressure, as hitherto assumed; and I think, as I do so, it will be seen how necessary it is to associate experiment with an inquiry of this kind, if we would read aright our observations.

In a paper in the Philosophical Transactions on the Physical Properties of Ice, I have shown that when a sunbeam traverses a mass of ice, the latter melts at innumerable points in the track of the beam, and that each portion melted assumes the form, not of a globule, but of a flower of six petals. The planes in which these flowers are formed are independent of the shape of the mass and of the direction of the beam through it; they are always formed *parallel to the surface of freezing.*

This is a natural consequence of the manner in which the particles of ice are set together by the crystallizing force. By the slow abstraction of heat from water its particles build themselves into these little stars, and by the introduction of heat into a mass so built the architecture is taken down in a reverse order. In watching the formation of artificial ice, by the machine of Mr. HARRISON referred to in my paper, I have seen little solid stars formed, by freezing, which were the exact counterparts of the little liquid stars formed by melting. So far as I can see, the complementary character of the phenomena is perfectly natural, and presents no difficulty to the mind in conceiving of it.

When the beam is intense, and its action continued for some time, the flowers expand, so as to form liquid plates within the mass. Looked at edgeways, these liquid spaces appear like fine lines; which proves that the melting is not symmetrical laterally and vertically, but that the ice melts in the planes of freezing much more readily than at right angles to these planes.

If an air-bubble exists within ice, and if the ice melts at the concave surface of this bubble, as might be expected from the foregoing facts, the ice will so yield that the composite cell of air and water will not be spherical, even though the bubble of air may originally have been so. In the planes of freezing the mass yields most readily, and the cavity containing the air and water will appear *as if flattened by a force acting perpendicular to these planes*. This is not a deduction merely, but an observation which I have made in a hundred different cases.

What I have here said applies to ordinary lake ice; but glacier ice has no definite "planes of freezing." The substance is first snow, which sometimes, it is true, falls regularly in six-rayed crystals, as observed by myself on the summit of Monte Rosa; but it is usually disturbed by winds, while falling, and whirled and tossed by the same agency after it has fallen; the mountain snow is often melted, mixed with water and refrozen. Even after it has become consolidated it is often shattered in descending precipitous slopes. In such ice definite planes of crystallization are, of course, not to be expected.

If we suppose a mass of lake ice to be broken up into fragments, and these fragments thrown together confusedly and regelated in their new positions to a continuous mass, we have an exact image of the character of the glacier ice in which this flattening of the bubbles in different directions has been observed.

In the paper already referred to, I have given a sketch of a piece of ice composed of such segments, and have described the effects obtained with it. That ice was sold to me as Norway lake ice. I am not aware whether glacier ice is ever imported into this country from Norway; but if it be, the piece in question must, I think, have belonged to it. It is so like all the glacier ice that I have examined since that time, and so unlike all the lake ice, that I feel little hesitation in saying that it belonged to the former*. No matter how coherent and optically continuous a mass of ice may be, a condensed sunbeam would at once tell us whether it belonged to a lake or to a glacier.

* Perhaps formed from the connecting together of confused fragments.

I have given in fig. 19 a sketch of a piece of ice taken from the end of the great Allalein glacier, on the Swiss side of the Monte Moro. On reference to M. Agassiz's figure, it will be quite manifest that we are both dealing with the same phenomenon; we have the division of the ice into "angular fragments," the flattening of the "bubbles," and the non-parallelism of their directions in the different fragments.

Fig. 19.

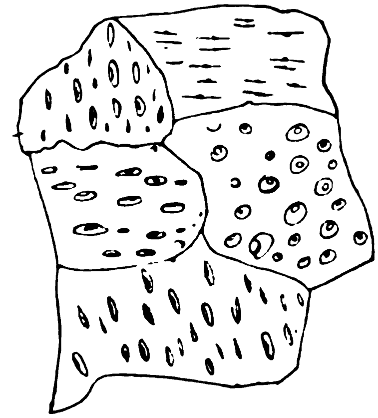
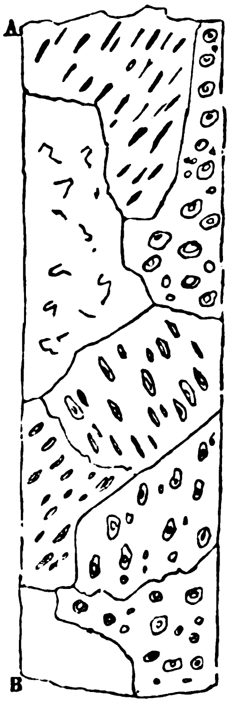


Fig. 20 is a sketch of a piece of ice which showed the veined structure. The line AB was parallel to the veins, and it will be seen that the "bubbles" are inclined to this line at different angles, and in different azimuths. The circles indicate, of course, that the "bubbles" were there parallel to the horizontal face of the slab, while the *lines* indicate that they were perpendicular. In one case the bubbles are seen in plan, in the other case in section. The ellipses show the bubbles foreshortened where their planes are oblique to the surface of the slab.

Fig. 20.



Associated with the air-bubbles, and usually beyond comparison more numerous in ice taken from the "ends" of glaciers, were the round liquid disks which I have described in my paper on the Physical Properties of Ice. Associated with each liquid disk was a *vacuous spot*, which shone with exceeding lustre when the sunbeams fell upon it. That the spots were vacuous, and not bubbles of air, I proved by permitting them to collapse under warm water; the collapse was complete, and no trace of air arose from them.

These, I doubt not, are the "bubbles" observed by M. Agassiz "near the end of the glacier," and which were "so flat that they might be taken for fissures when seen in profile."

These "vacuum disks," as I have usually called them, were invaluable as indicators of the planes of crystallization. When a condensed sunbeam was sent through the mass, the six-petalled flowers, which always indicate the planes referred to, started into existence parallel to the disks. Consequently, as the beam passed through different fragments, flowers were formed, in different planes, along the track of the beam.

True air-bubbles, associated with water, also occurred in these masses of ice, and such composite cells were always flattened out in the planes of the vacuum disks.

The fact then is that many of the so-called air-bubbles are not air-bubbles at all, and that the so-called "*flattening*" is in reality no flattening at all; and that pressure, in the sense hitherto conceived, has had nothing whatever to do with the shape of these bubbles. In glacier ice, as in lake ice, their shape is determined by the crystalline architecture. The conclusion that they were *squeezed* flat seems to have been drawn by M. Agassiz, and reproduced by subsequent writers, without due regard to the difficulties associated with it. That the pressures of a glacier are so parcelled out as to squeeze *contiguous fragments* of ice, not exceeding a cubic inch in size, in all possible directions, is so improbable, that reflection alone must throw great difficulties in the way of its acceptance.

It is with some diffidence that I here venture to express an opinion upon a question that I have not specially examined; but it appears to me probable that the decomposition of glacier ice into large granules, regarding which so much has been written, may be connected with the foregoing facts. The ice of glaciers is sometimes disintegrated to a great depth; causing it to resemble an aggregate of jointed polyhedra more than a coherent solid. I was very near losing my life in 1857 on the Col du Géant by trusting to such ice; and last summer I found vast masses of it at the end of the Allalein glacier. Blocks a cubic yard and upwards in volume, fell to pieces to their very centres on being overturned; they were an aggregate of granules, whose average volume scarcely exceeded a cubic inch. From the constitution which the foregoing observations assign to glacier ice, this disintegration seems natural. The substance is composed of fragments which are virtually crystallized in different planes; and it is not to be expected that the union along the surfaces, though they may be *invisible* when the ice is sound, is as intimate as that among the different parts of a mass homogeneously crystallized. Besides, ice no doubt, and all uniaxal crystals, expands by an augmentation of temperature, differently in different directions, and hence a differential motion of the particles on both sides of one of the above surfaces when the volume of the substance is changed by heat or cold is unavoidable. Such surfaces then would become surfaces of discontinuity, and perhaps produce that granular condition which has occupied so much of the attention of observers.

§ 10. *Physical Analysis of the Veined Structure.*

The relation of pressure and structure has been shown in the foregoing pages, but the mode in which the pressure acts remains yet to be considered. As regards their causes, slaty cleavage and slaty structure have been reduced to one and the same; but as regards the operation of that cause, no two things can, I imagine, in some respects at least, be more different.

In a note at page 336 of the 'Proceedings of the Royal Society' for January 1857, I refer to an experiment in which a clear mass of ice was caused by pressure to resemble a piece of fissured gypsum, and I there promised the full details of the experiment in due time. In my paper on the Physical Properties of Ice this promise is fulfilled; I have shown how a mass of compact ice may be liquefied by pressure, in parallel planes perpendicular to the direction of the force, and explained the effect by reference to the ingenious deductions of Mr. James Thomson from Carnot's maxim.

Let the attention now be fixed on the state of a glacier at the base of one of the ice-falls where it is bent so as to throw its surface into a state of longitudinal compression. According to the above experiments, the glacier whose temperature is 32° Fahr. must here be liquefied *in flats*, perpendicular to its axis. A liquid connexion is thus established between all the air-bubbles which are intersected by any one flat, and a means for the escape of this air from the glacier is thus furnished. The water produced is also partially expressed, partially absorbed by capillary attraction of the adjacent bubble ice, and partially refrozen when the pressure is relaxed. It is, I think, perfectly manifest that such a process, each step of which may be illustrated by experiment, must result in the formation of the blue veins*.

All the experiments and observations recorded in the paper on the Physical Properties of Ice were made with reference to the glaciers, and one experiment there recorded illustrates the present point more forcibly than any words could do,—it is that in which I have thrown a prism of ice into a state of compression, which brings one side of it into the exact condition of the glacier at the base of an ice-fall. Fig. 21 is precisely the

Fig. 21.

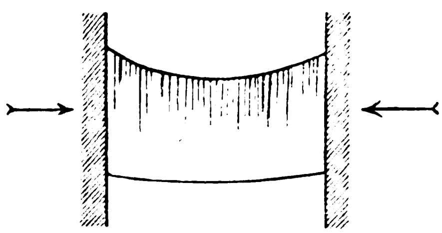
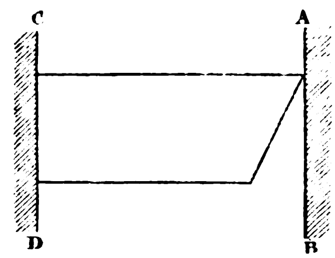


Fig. 22.



same as that given at page 226 of my paper, the prism being placed horizontal instead of vertical, so as to show its bearing upon the present point more distinctly. The original shape of the piece of ice is given in fig. 22, which by compression between the surfaces AB and CD is reduced to the shape and condition of fig. 21. The vertical lines represent the planes of liquefaction, and they correspond exactly to the planes of the blue veins in the glacier. The application of the same principles to all cases where pressure comes into play is sufficiently obvious.

In the experiments with the hydraulic press, the portion of ice between each two liquefied flats transmits the pressure without sensible yielding. We have no difficulty in conceiving that the same holds true of glacier ice, and that pressure may be transmitted through one portion of a bubbled mass of ice and produce the liquefaction of another portion, without sensible distortion of the bubbles contained in the former. One of the objections which have been urged against the pressure theory is thus, I conceive, completely answered,—the objection, namely, that the white ice which transmits the pressure ought to have its bubbles flattened. Indeed this objection continued to be of weight only so long as it was imagined that the observed flat bubbles had been *squeezed* to this shape; a notion, which I think will no longer be entertained.

* The mechanical actions which accompany the development of ordinary slaty cleavage, must, I think, also manifest themselves to some extent in the glacier.

§ 11. *Remarks on Glacier Motion.*

It is only by slow degrees that we master from actual observation, a problem so large as that presented by the glaciers; the muscular labour alone being such as to render the expenditure of a considerable amount of time unavoidable. The examination of the various questions connected with glaciers, has been therefore, in my case, distributed over some years, and not until last summer was I able to devote the requisite attention to the subject of the present section, which, however, is essential to a right comprehension of the physics of glacier motion.

It would be a problem eminently worthy of any geologist, to lay down upon a trustworthy map of Switzerland the directions of the striæ on the rocks over which ancient glaciers have moved; and to one who sees its importance and desires exact information upon this subject, it must be a matter of surprise that nothing of the kind, in a systematic way, has yet been attempted. A suitable map furnished with such lines of direction, carefully and conscientiously drawn, would impart more satisfactory information than all the volumes that ever have been, or ever will be written upon the subject. Here is a piece of work loudly calling for accomplishment, and one on which any young geologist may base an honourable reputation.

Mr. Hopkins, I believe, was the first to urge the existence of *roches polies* at the ends of existing glaciers and along the continuations of existing glacier valleys as an evidence in support of the sliding theory. That such facts exist is known to every body, and that the rocks are thus polished and rounded by the glaciers sliding over them is incontrovertible. Let a traveller, if he wish to obtain a wealth of information upon this subject, transport himself to the terminus of the Unteraar glacier, and walk thence down the valley through which the river Aar now flows. On all sides he will obtain the most striking evidence that the base of the valley was once the bed of the glacier. The rocks are polished and striated, and present at some places the appearance of huge rounded mounds, which, at first sight, would appear to offer an insuperable barrier to the motion of the glacier, but which show by their aspect that the ice actually moved over them, grinding off their angles and furrowing their summits and sides. All along the valley towards Meyringen, similar evidences exist. In fact, the phenomenon is very common, and admitted on all hands.

The conclusion which Mr. Hopkins has drawn from these facts is unavoidable; the glaciers must have *slidden* over the rocks on which such traces are left. To an eye a little practised in those matters, the precise limits reached by the ancient glaciers are perfectly visible. The junction of the rounded and abraded portions of the mountains, with those portions which in ancient times rose over the then existing ice, is perfectly distinct; and I should say in the valley of the Aar reaches to a height of more than a thousand feet above the present bed of the river. The valley of Saas, in the Canton de Valais, furnishes magnificent examples of the same kind.

At all places, from the base of the ancient glacier to its surface, sliding must have occurred; the evidence of it is perfectly irresistible. The summit of the Grimsel pass constituted the bed of an ancient névé; and the groovings and polishings, at the very summit of the pass, show that the ancient névés, as well as the ancient glaciers, slid upon their beds. In company with my friend Professor Ramsay, and assisted by his great experience, I visited the sites of other ancient névés, and found the same true of all of them; they all slid more or less over their beds.

No investigator of glacier motion can shut his eyes to those facts, nor refuse to give them their proper weight. *The sliding theory is beyond doubt to some extent true*; and many of the objections raised against it, and still repeated in works intended to instruct the public, are altogether futile.

Here, as in other cases, we find that the extreme facts have been dwelt upon principally by rival theorists, and coexistent truths have, by partial treatment, been rendered apparently hostile to each other. It is perfectly certain that a glacier *changes its form* by pressure like a plastic mass, but it is equally true that it *slides over its bed.*

§ 12. *On the Dirt-bands of the Mer de Glace.*

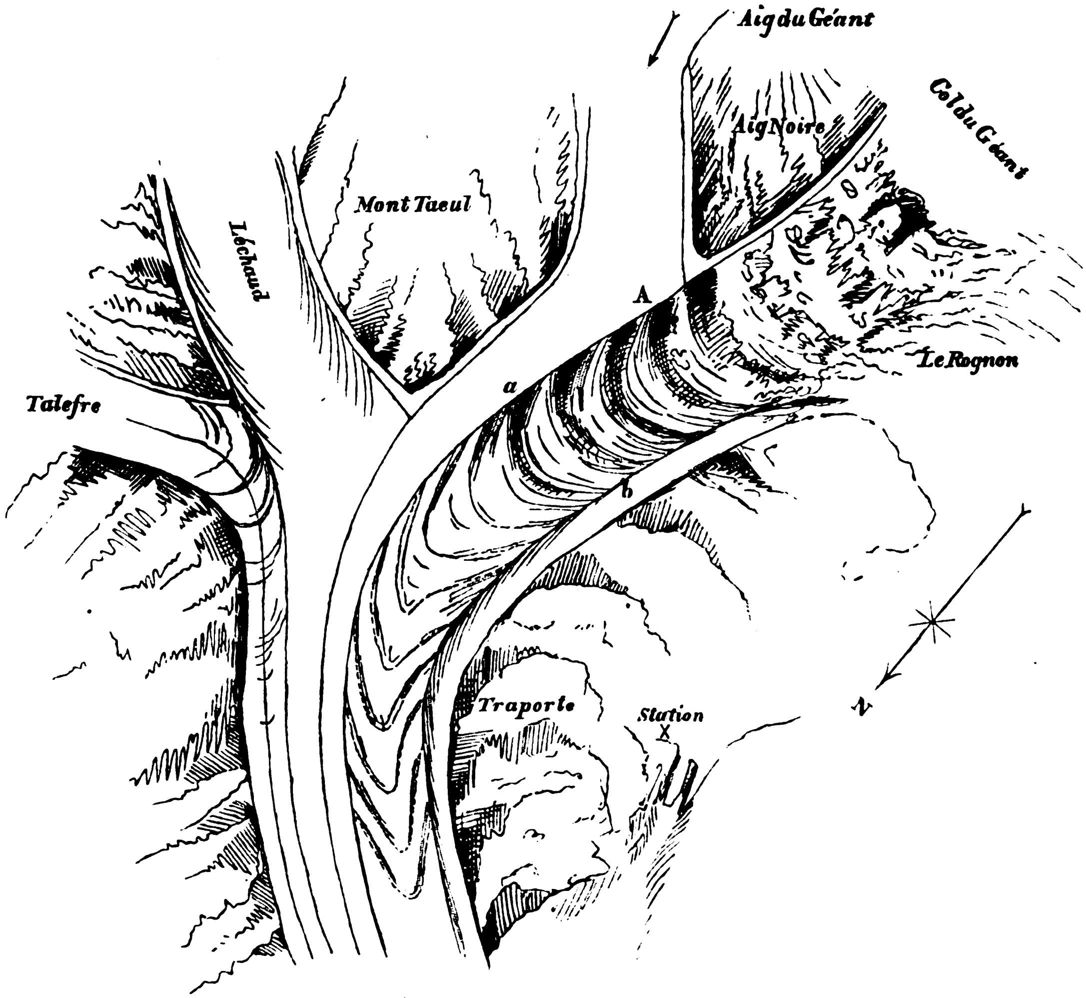
In walking over the Mer de Glace, we soon observe differences in the distribution of the dirt upon its surface; but while standing on the glacier itself, no orderly arrangement of the dirty and clean spaces is observed. From a point, however, which commands a view of a large portion of the glacier, it is seen that the dirty spaces are arranged so as to form a series of broad brown curves, which follow each other in succession down the glacier. They were first observed by Professor Forbes from the heights of Charmoz, on the 24th of July, 1842, and from the same place, on the 16th of July, 1857, I observed them. Last summer I counted eighteen of them from the same position, and this agrees with the number observed by Professor Forbes. This agreement, after an interval of sixteen years, proves the regularity of their occurrence.

These bands were different from anything of the kind I had previously seen, and I felt that the explanation given of the "dirt-bands" observed by Mr. Huxley and myself would not completely account for those now before me. They were perfectly detached from each other, and resembled sharp hyperbolas with their vertices pointing downwards.

From Charmoz, however, I could only see the existence of the bands, but could not see their origin. To observe this, I climbed a summit to the left of a remarkable cleft in the mountain range between the Aiguille du Charmoz and Trelaporte, which strikes most visitors to the Montanvert. The place is that referred to by Professor Forbes at page 84 of the 'Travels,' and which he was prevented from reaching by the fire of stones which a secondary glacier sent down upon him. From the pile of stones on the summit I, however, infer that he or some of his guides must have reached the place. Fig. 23 represents what I observed from this excellent station.

From the base of the Aiguille du Géant and the Periades, a glacier descends which is separated by the Aiguille and promontory of La Noire from the great glacier which descends from the Col du Géant. A small moraine is formed between both, beside which the letter *a* stands in the diagram. The glacier descending from the Col is bounded on the west by the small moraine *b*, and between *b* and the side of the valley is another little glacier derived from one of the lateral tributaries.

Fig. 23.



With regard to the "dirt-bands," the following significant fact at once revealed itself. *The dirt-bands extended over that portion of the Glacier du Géant only which lay between the moraines a* and *b*, or, in other words, were confined to the ice which had descended the great cascade between Le Rognon and La Noire. It was perfectly evident that the cascade was in some way the cause of the bands.

The description which I have already given of the ice-fall of the Rhone and of the Strahleck arm of the Lower Grindelwald glacier, applies generally to the fall of the Glacier du Géant. The terraces, however, are here larger, and the protuberances at the base of the fall of grander proportions. These latter are best seen from a point near A upon the Glacier du Géant; they are steepest on that side, in consequence of the oblique thrust of the western tributaries of the glacier. All that I have said regarding the toning down of the ridges to rounded undulations which sweep in curves across the glacier, applies here also. Referring to the section of the glacier of the Rhone in fig. 4, it will be seen that the word "dirt" is written opposite to each hollow. In fact the depressions between the protuberances are, to some extent, the collectors of the fine superficial dirt. This is also the case upon the Glacier du Géant; but here I noticed that the *frontal slopes* of the protuberances were also covered with a fine brown mud. Lower down the glacier the swellings disappear, but the dirt retains its position upon the ice, and afterwards constitutes the *dirt bands* of the Mer de Glace.

A remarkable change in the form of the bands occurs where the glacier is forced through the neck of the valley at Trelaporte. They sweep across the Glacier du Géant in gentle curves with their convexity downwards; but in passing Trelaporte the arms of the curves are squeezed more closely together, the vertices are pushed sharply forwards, so that on the whole the bands resemble a series of hyperbolas which tend to coincide with their asymptotes.

Looking down from the Convercle upon the Glacier du Talèfre, a series of swellings like those upon the Glacier du Géant are observed. Along the intervening hollows streams run, and sand and dirt are collected, forming the rudiments, so to speak, of a series of dirt-bands; but these latter never attain anything like the precision of those upon the Mer de Glace. I saw no such bands upon the Léchaud, for here the necessary ice-fall is absent: if bands at all exist on this glacier, they must, I imagine, be of a very rudimentary and defective character.

I will not occupy the time of the Society in describing my various expeditions up the Glacier du Géant in connexion with these bands; but one circumstance, to which the definite printing of the bands is mainly due, must be mentioned. The Glacier du Géant lies nearly north and south, being only 14 degrees east of the true north. Standing with his back to the Col du Géant, an observer looks northward, and consequently the frontal slopes of the protuberances to which I have referred have a *northern aspect*. They therefore retain the snow upon them long after it has been melted from the general surface of the glacier. The summer of 1857 was unusually warm in the Alps, but its great heat was not sufficient entirely to remove the snow. No doubt, in colder summers, the snow is retained upon the slopes all the year round. Now this snow becomes the collector of a fine brown mud, which is scattered over the surface of the glacier. It catches the substance transported by the little rills and retains it. The edges of the snow still remaining, when I was on the glacier, were exceedingly black and dirty; and in many cases the entire surface of the snow appeared as if fine peat mould had been strewn over it. Lower down the glacier this snow melts, but it leaves its sediment behind it, and to this sediment the distinctness of the dirt-bands of the Mer de Glace is mainly due.

The regularity of the bands depends on the regularity with which the glacier is broken, and the ridges or terraces formed as it passes over the brow of the fall. It is the toning down of these ridges which produces the undulations, which are to some extent modified by the squeezing at the base of the fall; and it is the undulations which produce the bands. Thus the latter connect themselves with the transverse fracture of the glacier as it crosses the brow of the fall.

In the figure I have given the general aspect of the bands, but not their number. Thirteen of them exist on the Glacier du Géant. I may add that the bearing I have assigned to this glacier differs from that assigned to it on the map which accompanies Professor FORBES'S 'Travels on the Alps,' and which I had with me at the Montanvert. The reason is, that on the map the true north is drawn on the wrong side of the magnetic north, thus making the "Declination" easterly instead of westerly. I have since learned that this error is corrected in the smaller work of Professor FORBES.

It has been affirmed that the dirt-bands cross some of the medial moraines of the Mer de Glace, and they are thus drawn upon the map of Professor FORBES. Were this correct, my explanation would be untenable; but the fact is, that the bands are confined to the Glacier du Géant from beginning to end.

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