XIV. *Illustrations of the Viscous Theory of Glacier Motion.*—Part III.

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§ 6. *On the Motion of Glaciers of the Second Order.*

Up to the year 1844 no attempt had been made, so far as I am aware, to measure the rate of motion of those comparatively small isolated glacial masses reposing in the cavities of high mountains, or on *cols*, called by De Saussure *Glaciers of the Second Order*.

Some observations had indeed been made upon a glacier of this description in 1841, and by MM. Martins and Bravais, during a residence on the Faulhorn. But it was not at that time known that the motion of glaciers was a continuous and regular one, admitting of rigorous measurement even in short intervals of time, and the importance of such observations was overlooked. They accordingly believed that the glacier in question had no sensible motion, and probably they did not attempt to observe it until a subsequent year. It is impossible now to doubt that the *Blau Gletscher*, near the Faulhorn, has a movement like all other bodies of the kind.

In July 1844, I had an opportunity of passing some days at the hospice of the Simplon, in the neighbourhood of which exists a small glacier of the second order, easy of access, and very fit for the experiment which I proposed to myself upon such bodies. Its diminutive size made it all the more suitable; for should it be found to possess a regular motion, we are certain that the *mechanism of a glacier* is continued within the small compass of a mass which may be conveniently examined in detail in all its parts. It is lodged in a niche of the mountain called the Schönhorn*, immediately behind the Simplon hospice: we shall therefore call it the glacier of the Schönhorn. From its inconsiderable extent, it might easily be overlooked by a passing traveller amidst the multitude of vast and striking objects by which he is surrounded†. It is perched, as has been said, in a kind of niche on the northern

* Also called Hübschhorn, an equivalent epithet.

† The reader will not for a moment imagine that it is the Kaltwasser glacier of which we speak, which lies also in the neighbourhood of the Schönhorn, descending from the Monte Leone and Wasenhorn, and from which the *Galerie du Glacier* on the Simplon road takes its name.

face of the Schönhorn, somewhat about an hour's steep climb above the hospice; consequently about 1400 feet higher. The hospice is itself 6580 English feet above the level of the sea; the mean height of the Schönhorn glacier may be taken at 8000 feet. I had not an opportunity of ascertaining it more accurately.

Plate X. figs. 1 and 2, shows a sketch of a front view of the Schönhorn taken from the opposite heights, and a ground plan of the glacier. The latter is sketched merely by the eye, but the scale is furnished by some actual measures. I first visited the glacier on the 20th of July 1844. It was then covered over, by far the greater part of its extent, with snow, as shown in the plan. This snow is in great part manifestly permanent, and the glacier is therefore in the state of *névé*. The general slope is from top to bottom of the plan, and its inclination is variable, depending upon the direction of the avalanches by which it is fed, of which the principal descends the rapid *couloir* marked C, when the inclination is about 35°. This avalanche forms a sort of ridge down the glacier, as indicated by the shading of the map, leaving a considerable space comparatively flat to the eastward. On the west, the snow thins off from the ridge until it exposes the ice near the part marked B, where the slope is still considerable, being 20°, and here we have the real mass of the glacier exposed, although the ice is not of an exceedingly hard or crystalline character. The front or lower termination of the glacier all along presents a steep, nearly precipitous surface of ice, sloping from 45° to 60°. This ice rests on a bed of debris of rock which appears to be inclined about 25°. Except near the precipitous termination of the glacier, there are no apparent crevasses. The surface is uniform and uninterrupted. Some water issues from beneath the steepest part of the ice; but even in the middle of the day, near the end of July, there was exceedingly little. The length, if it may be so termed, of the glacier, from back to front is about 1000 feet, and its greatest breadth 1300 feet. Its surface may be roughly estimated at twenty-six acres.

The rock of which the Schönhorn is composed, is an alternation of the slaty rocks resembling gneiss with talc slate, which are so common in this part of the Alps. To my great surprise, on one of my visits, I heard the sound of hammers and blasting in this elevated and remote spot; and found two men employed in quarrying Potstone (*Lapis ollaris*) for building ovens, from a retired nook beyond the glacier; the quarry is marked on the plan at E.

On the 20th of July 1844, I ascended to the glacier, accompanied by M. ALT, one of the clerical members of the Simplon establishment, and an assistant; and I fixed upon a position, marked St. on the rock on the east side of the glacier, for planting the instrument, which was then directed, as nearly as I could judge, in a line transverse to the prevailing slope of the glacier, and the telescope was made to describe a vertical plane. It was then sighted upon a well-marked quartz vein on the rock on the distant side of the glacier, marked D, by which it could at any time be brought into precisely the same position; the position of the instrument itself being

Fig. 1. p.178.

View of the Glacier of the Schönhorn-Simplon.

Fig. 3. p.201.

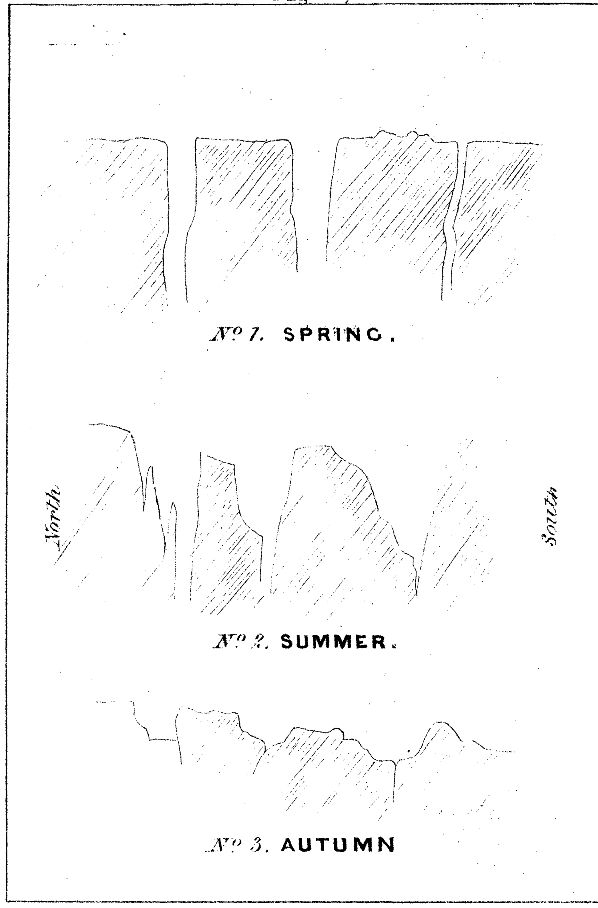
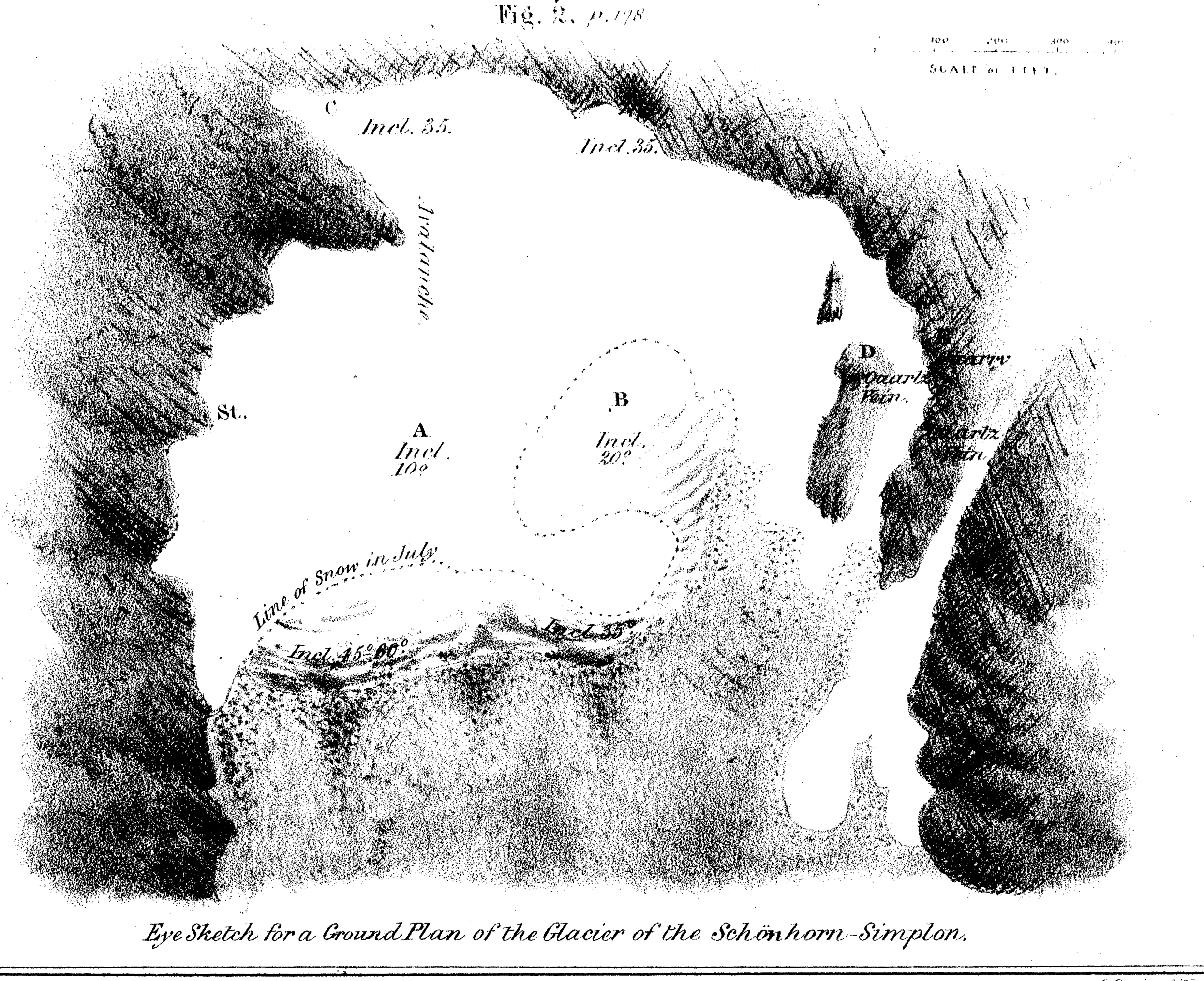


Fig. 2. p.178.



Eye Sketch for a Ground Plan of the Glacier of the Schönhorn-Simplon.

J. Basire, lith.

referred to a mark cut on the rock where it stood. Two marks were then fixed on the glacier; one was a pole stuck in at A, several feet into the snow of the avalanche already described as traversing the length of the glacier. The slope of the snow at the point A was about 10°; and the distance of A from the station St., by an approximate measurement, 340 feet. 350 feet further in the same direction, a hole was made with a blasting iron into the solid ice at B, where the inclination was 20°. The precise position of these marks being determined relatively to the visual line, the observation was finished at 4 o'clock P.M.

On the 23rd of July we returned. The mark A in the snow (which was so firmly driven in that it could not be withdrawn without breaking the pole) had advanced in the direction of the slope exactly four inches at 1 P.M., or in sixty-nine hours; whilst the mark B in the ice had advanced $5\frac{1}{4}$ inches in the same time; whence we have

Velocity of A in twenty-four hours . . . . 1·4 inch.
Velocity of B in twenty-four hours . . . . 1·8 inch.

The result was what I had anticipated, although it must be confessed it might be expected to be nearly the same upon any theory of glacier motion yet proposed. The slope of a glacier, *per se*, is not an index of what should be the velocity of motion on the viscous theory. No doubt, other things being equal, the velocity will be proportional to some function of the declivity, and such we have seen to be fully borne out by experiments on the Mer de Glace of Chamouni; and in the present case, the velocity under a slope of 20° was about one-third greater than that under a slope of 10°. But the analogy of a river, as well as theoretical considerations, show that the slope is but one of numerous considerations; such as (1) the *mass* of the viscous body; the smaller the mass the smaller the velocity on a given slope*; (2) the state of infiltration or wetness of the glacier altering its resistance to change of form†. Without mentioning other causes, these are quite sufficient to account for the small velocity observed, when we recollect the very insignificant mass of this glacier and its dry state arising from its great elevation, its northern exposure, and even the very inclination of its bed which keeps it in a state of perfect drainage and leaves it always in a state tending to the *snowy*, rather than that of imbibition.

§ 7. *On the Annual Motion of Glaciers, and on the Influence of Seasons.*

The first estimate of the least authority on the advance of any point of a glacier from year to year, was made by HUGI on the glacier of the Aar, from 1827 to 1836. The method employed was to measure the distance of a well-marked block of stone, resting on the ice from a transverse line determined by the fixed objects on the shore. This is the only way, generally speaking, practicable upon glaciers at a distance from habitations, and where marks cannot be conveniently renewed in the ice from time to time during the whole year. The velocity of the part of the glacier imme-

* Travels in the Alps of Savoy, 2nd edit., p. 387. † Ibid. pp. 148. 371.

diately below the promontory, called the Abschwung, was found to be about 240 feet per annum, which though neither confirmed nor invalidated by the discordant measurements subsequently made by other observers on the same glacier, has at length been substantially corroborated by a professional surveyor, M. Wild, who has recently undertaken the verification at M. Agassiz's request.

After having myself observed the motion of several points of the Mer de Glace of Chamouni during the summer of 1842, I fixed the positions of two conspicuous blocks, one near Montanvert, marked D 7; and another opposite the Tacul, marked C, or the Pierre Platte (see my Map of the Mer de Glace), by means of which I hoped to ascertain the mean annual motion in succeeding years. With respect to the latter, or the Pierre Platte, I was successful; for in September 1843 I ascertained geometrically its change of position, subject however to the uncertainty of a few yards, owing to the sliding of the block from the pedestal of ice upon which it was so picturesquely poised*, a circumstance which happens once or twice in the course of every summer.

From the 17th of September 1842, to the 12th of September 1843, the advance was (in 360 days)	256·8 feet.
Or reduced to the exact year of 365 days	260·4 feet.
Mean daily motion	8·56 inches.

Again, being enabled to repeat the measures in 1844, I found the advance—

From the 12th of September 1843, to the 19th of August 1844 (342 days)	270 feet.
Proportional motion for 365 days	288·3 feet.
Mean daily motion	9·47 inches.

In the case of the block D 7, I was less fortunate. It was very near the western side of the glacier, and though not thrown up on the shore, yet the ice on which it rested got in some manner so embayed or entangled, that though its motion had been steadily watched during the winter of 1842–43 by my able assistant, Auguste Balmat, it had scarcely moved since his last observation on the 8th of June 1843, when I visited it in September of the same year. It must be presumed that it had been much retarded previously, and hence it is clearly inadmissible to infer a proportional motion for the portion of the year when it had not been observed, as I did in the Postscript at the end of the first edition of my Travels, whilst in ignorance of the then unsuspected retardation. The motion actually observed was 432 feet in 322 days, being at the rate of 483 feet per annum, or 15·88 inches per day. This is therefore undoubtedly *below* the true measure of the annual motion of the side-part of the glacier somewhat in advance of the Châlet of Montanvert (see the position of D 7 in the Map). It may at least be of some service as an *inferior limit* of the annual motion there.

In 1843 I fixed approximately the position of a block marked P, higher up the glacier than the Montanvert, and near its left bank, exactly opposite the spot called Les Ponts,

* See Frontispiece to Travels through the Alps of Savoy.

The observation, being repeated the ensuing year, gave a motion of *about* 486 feet (the nature of the observation did not admit of the same accuracy as at station C) from the 13th of September 1843 to the 9th of August 1844, or 331 days, being at the rate of

536 feet per annum,

or 17·62 inches per day.

In 1844 I made the casual discovery of one of my staves, used to mark the position of the station A at the *Angle*, a little higher up the glacier than the last, a point of which the motion had been most carefully observed during the summer of 1842 (see Travels, p. 140). This stick still bore legibly written upon it the date when it had been fixed in the ice at station A, and as the painted marks on the rock of the *Angle* were still as fresh as when they were made, I had no difficulty in finding the exact position on the glacier which this mark had in any part of the summer of 1842, and by measuring the distance to the place where it was found (which was on a spot of the ice quite unfrequented by guides or any one else), I had good reason for believing that this must be the space over which it had travelled in the mean time; although of course I do not ascribe to this observation the weight of a direct measure, yet it proves an interesting confirmation. Reckoning from the position it occupied on the 1st of September 1842, it had advanced down to the 26th of August 1844,

or in 720 days . . . . 952 feet,
or, per annum . . . . 482·5 feet.
Mean daily motion . . . 15·87 inches.

It will be seen that this result is in close agreement with that observed at station P above mentioned, which is a little further down the glacier, but about the same distance from the side; for though the motion of P is somewhat greater for 1843–44 than the mean motion of A for 1842–44, it will be seen by the comparative observations at C already referred to, that the glacier moved more rapidly in 1843–44 than in 1842–43.

But I am now enabled to present a view of the actual progress of two glaciers during every part of the year from direct observation. For these I am indebted to the intelligent and persevering zeal of my excellent guide and assistant at Chamouni, Auguste Balmat, of whose character I have had the pleasure of forming a more and more favourable estimate the longer I have been acquainted with him. To the long training of the laborious summer of 1842, when he assisted me, he adds the further experience derived from my visits in 1843 and 1844, in the latter of which especially he became familiar with the nice precautions requisite in conducting the most accurate measurements, and received instructions from me which rendered him perfectly competent to continue by himself the simpler kind of measurements which I have alone required of him. The extraordinary exertions which he used to obtain the winter motion of the block D 7, under the Montanvert, in 1842–43, have been noticed in my former publications. On one or two occasions, as I learned afterwards from himself, being unable to ascend the usual path to the Montanvert for fear of spring avalanches,

he actually clambered with a companion up the rugged ascent from the source of the Arveiron, plunging continually up to the middle in snow, for no other purpose than to make the observation which I had requested of him; and it would be unjust not to mention at the same time the admirable, because rare, generosity, with which he positively refused for himself any share of the remuneration which I pressed upon him the following summer, as some recompense for the fatigues and dangers which he had braved to obtain for me this information. With such a person, my confidence in the observations which he has since made at points much more accessible, and with the experience of some additional years, is complete. I do not mean that mistakes may not occur, or even that the measures may not be less exact than I might have taken myself; but from my knowledge of the man, I am nearly as confident in their *being faithfully reported, exactly as they were made*, as if I had done so myself.

With a view to lighten the labour as much as possible, I selected two stations on the *glacier of Bossons*, and desired BALMAT to select two on the *Glacier des Bois* (the outlet of the *Mer de Glace* towards the valley of Chamouni); all these points being tolerably accessible at every season of the year.

The general method of observation was the following:—vertical holes were driven into the ice with a 4-foot blasting iron, at the points whose motion was to be determined; and these holes were renewed from time to time as the surface of the ice wasted. A staff of wood $5\frac{1}{2}$ feet long, was stuck in each, which projected sufficiently above the snow (which never appears to have exceeded $2\frac{1}{2}$ feet deep on the glacier) to make it visible at all seasons. During winter the staves were frozen into the ice, and the waste being small, the holes did not require renewal. Two marks are then made of a permanent kind on the rocks of the moraine, or two staves driven in, or a distant object on the farther side of the glacier was observed, so as to mark out sufficiently a line transverse to the glacier, the prolongation of which passes over the hole in the ice when first made; and the advance of the hole in the ice beyond this fixed visual line marks the progress of the glacier. The want of a theodolite is supplied by directing the eye past a plumb-line suspended over the fixed mark on the moraine nearest to the glacier, the eye of the observer being over the farthest mark. As the spaces moved over were in most cases considerable, an error of a few inches, or even a foot, is not important to the result. The progress was in every case determined by means of a line marked with *English* feet and inches, left by me at Chamouni on purpose.

The results were communicated to me regularly by letter at intervals of a few weeks during the whole year, and all questions asked and explanations required by me were answered by return of post.

Those who may look with suspicion upon observations made in a remote place by a peasant of the better class, though they may not partake of my security in the results from knowing the character of the individual, will, I believe, have their doubts removed by the internal evidence of this important series of observations,

which even a philosopher could not have invented, and which, it will be seen, are confirmed by data of quite another kind over which the observer could have no control, I mean the Meteorological Registers of Geneva and St. Bernard.

Table I.

First Station on the Glacier des Bois, a little way below the Chapeau, and at about *one-third* of the breadth of the glacier from its eastern bank.

From 1844			To 1844.			Space moved over in English feet and inches. feet.	inches.	Daily motion. inches.
October ..	2.	10 A.M.	October	14.	9 A.M.	32	0	32·0
October ..	14.	9 A.M.	November ..	2.	8 A.M.	43	11	27·8
November ..	2.	8 A.M.	November ..	19.	4 P.M.*	34	11	24·2
November ..	20.	1 P.M.	December ..	4.	3 P.M.	13	10	11·8
			1845.					
December ..	4.	3 P.M.	January	7.	3 P.M.	32	8	11·5
1845.								
January	7.	3 P.M.	February ..	18.	3 P.M.	49	2	14·0
February ..	18.	3 P.M.	March	18.	2 P.M.	39	10	17·0
March	18.	2 P.M.	April	17.	10 A.M.	42	1	16·9
April	17.	10 A.M.	May	17.	8 A.M.	56	3	22·5
May	17.	8 A.M.	May	31.	2 P.M.	43	11	37·0
May	31.	2 P.M.	June	19.	4 P.M.	61	11	38·4
June	19.	4 P.M.	July	4.	10 A.M.	52	0	42·3
July	4.	10 A.M.	July	18.	5 P.M.	62	0	52·1
July	18.	5 P.M.	August	6.	4 P.M.	77	6	49·0
August	6.	4 P.M.	October	8.	4 P.M.	187	8	35·7
October	6.	9 A.M. (?)	November ..	8.	2 P.M.	100	9	36·4
November ..	8.	2 P.M.	November ..	21.	1 P.M.	32	6	30·1

Table II.

Second Station, Glacier des Bois, near the lowest extremity, just behind the "Côte du Piget."

From 1844			To 1845.			Space moved over in English feet and inches. feet.	inches.	Daily motion. inches.
December ..	4.	2 P.M.	January	7.	4 P.M.	8	6	3·3
1845.								
January	7.	4 P.M.	February ..	18.	4 P.M.	8	11	2·6
February ..	18.	4 P.M.	March	18.	3 P.M.	7	0	3·0
March	18.	3 P.M.	April	17.	11 A.M.	11	7	4·6
April	17.	11 A.M.	May	17.	9 A.M.	18	4	7·3
May	17.	9 A.M.	May	31.	1 P.M.	10	5	8·8
May	31.	1 P.M.	June	19.	2 P.M.	13	3	8·3
June	19.	2 P.M.	July	4.	9 A.M.	14	1	11·1
July	4.	9 A.M.	July	18.	6 P.M.	17	6	14·6
July	18.	6 P.M.	August	6.	3 P.M.	18	8	11·9
August	6.	3 P.M.	October	6.	7 A.M.	50	1	9·9
October	6.	7 A.M.	November ..	8.	4 P.M.	27	2	9·8
November ..	8.	4 P.M.	November ..	21.	11 A.M.	8	0	7·5

* There is some uncertainty about the circumstances of this observation, which from the difficulty of corresponding satisfactorily at so great a distance about minute local occurrences, I have been unable perfectly to clear up. It is probably correct as it stands.

Table III.

First Station on the Glacier des Bossons. Some way above the Plateau, where the glacier is usually crossed; on the west side, and near the moraine.

						Space moved over in English feet and inches.		Daily motion.
From 1844			To 1844.			feet.	inches.	inches.
November..	20.	Noon.	December ..	4.	3 P.M.	20	4	17·3
			1845.					
December ..	4.	3 P.M.	January	7.	1 P.M.	44	11	15·9
January	7.	1 P.M.	February ..	14.	4 P.M.	46	9	13·6
February ..	17.	4 P.M.	March	17.	Noon.	35	10	15·4
March	17.	Noon.	April	16.	5 P.M.	32	7	12·9
April	16.	5 P.M.	May	17.	3 P.M.	60	0	23·3
May	17.	3 P.M.	May	31.	8 A.M.	49	0	42·9
May	31.	8 A.M.	June	19.	10 A.M.	54	2	34·1
June	19.	10 A.M.	July	4.	5 P.M.	53	8	42·1
July	4.	5 P.M.	July	21.	2 P.M.	43	1	30·6
July	21.	2 P.M.	August	7.	3 P.M.	40	10	28·8
August	7.	3 P.M.	October	6.	Noon.	103	0	20·6
October	6.	Noon.	November ..	10.	2 P.M.	56	8	19·4
November ..	10.	2 P.M.	November ..	22.	11 A.M.	22	5	22·6

Table IV.

Second Station on the Glacier des Bossons. Near the lowest extremity of the glacier, where free from the moraine, on the western side.

						Space moved over in English feet and inches.		Daily motion.
From 1844			To 1844.			feet.	inches.	inches.
October	2.	2 P.M.	October	13.	4 P.M.	12	11	14·0
October	13.	4 P.M.	October	31.	10 A.M.	25	1	17·0
November ..	20.	1 P.M.	December ..	4.	2 P.M.	15	4	13·1
			1845.					
December ..	4.	2 P.M.	January	7.	3 P.M.	36	11	13·0
1845.								
January	7.	3 P.M.	February ..	19.	1 P.M.	43	1	12·0
February ..	19.	1 P.M.	March	17.	1 P.M.	27	10	12·8
March	17.	1 P.M.	April	16.	6 P.M.	25	8	10·2
April	16.	6 P.M.	May	17.	4 P.M.	49	11	19·4
May	17.	4 P.M.	May	31.	9 A.M.	34	6	30·2
May	31.	9 A.M.	June	19.	11 A.M.	44	2	27·8
June	19.	11 A.M.	July	4.	6 P.M.	41	2	32·3
July	4.	6 P.M.	July	21.	11 A.M.	37	3	26·4
July	21.	11 A.M.*	August	7.	4 P.M.	30	3	21·4
August	7.	4 P.M.	October	6.	2 P.M.	82	7	16·5
October	6.	2 P.M.	November ..	10.	4 P.M.	17	3	5·9
November ..	10.	4 P.M.	November ..	22.	1 P.M.	7	2	7·2

These four sets of observations are projected in Plate XI. fig. 1, where the four lower zigzag curves represent the gradation of diurnal velocity by periods, according to the method adopted in projecting my own observations in my Travels, p. 141. The

* Marked 4 P.M., perhaps by mistake, but computed on that supposition.

general accordance is sufficiently manifest, and the effect of the season of the year is beautifully shown, the following being the minimum and maximum values:—

	Daily motion in inches.
Glacier des Bois, No. I., minimum in December	11·5
Glacier des Bois, No. I., maximum in July	52·1
Ratio of maximum to minimum	$4\frac{1}{2}$: 1
Glacier des Bois, No. II., minimum in January	2·6
Glacier des Bois, No. II., maximum in July	14·6
Ratio of maximum to minimum	$5\frac{1}{2}$: 1
Glacier des Bossons, No. I., minimum in March	12·9
Glacier des Bossons, No. I., maximum in May	42·9
Ratio of maximum to minimum	$3\frac{1}{2}$: 1
Glacier des Bossons, No. II., minimum in March	10·2
Glacier des Bossons, No. II., maximum in June	32·3
Ratio of maximum to minimum	$3\frac{1}{4}$: 1

From these observations we may deduce the annual motion from November 1844 to November 1845 with considerable exactness. Allowing for the fractional parts of a year, we obtain the following results, amongst which I have included a separate computation of the mean daily motion for the summer period (April—October), and the winter period (October—April).

TABLE V.

	Bois, No. I.	Bois, No. II.	Bossons, No. I.	Bossons, No. II.
	feet.	feet.	feet.	feet.
Motion for 365 days, November 1844 to November 1845....	847·5	220·8	657·8	489·1
	inches.	inches.	inches.	inches.
Mean daily motion	27·8	7·3	21·6	16·1
Mean daily motion, summer period, April to October	37·7	9·9	28·0	22·2
Mean daily motion, winter period, October to April	19·1	4·7	15·8	10·7
Ratio, summer: winter, motion..........................	2·0 : 1	2·1 : 1	1·8 : 1	2·1 : 1

I. From this Table we deduce in the first place a mean annual motion far greater than has hitherto been observed, or perhaps suspected in any glacier, that of near 300 yards, or almost *one sixth* of a mile. This is on the Glacier des Bois beneath the *Chapeau*, where the inclination of the glacier is very steep, adding a new illustration of the general principle*, that in *similar* circumstances the velocity increases with the slope. To this cause may be added the high temperature of the air of the valley to which in this part of its course it is exposed; but this last cause is alone insufficient; for

II. We find that the lowest part of the same glacier immediately behind the Côte

* Travels, 2nd edit. p. 371.

du Piget, a little way above the source of the Arveiron, and therefore still deeper in the valley, has a mean velocity nearly *four times less*, arising solely from the diminished slope and volume of the glacier in that part*. Hence there must be a condensation of the ice here, a pressure *à tergo*, the quicker moving ice pressing against the slower, consolidating it, remoulding its plastic material and sealing the crevasses; and a slight examination of the state of the glacier at the points in question will show this to be the case.

III. All that has now been said with respect to the two stations on the Glacier des Bois may be repeated with only numerical differences with respect to the two stations on the Glacier des Bossons; the one set of observations confirming the other.

IV. In both glaciers the summer motion exceeds the winter motion in a greater proportion, as the station is lower, that is, exposed to more violent alternations of heat and cold; this we shall find to be general.

Before continuing our deductions, we would call attention to the close relation which may be established between the mean temperature of any portion of the year and the velocity of the glacier corresponding to it. This is done in figure 1, Plate XI., exactly in the same way as I did when comparing my observations in the summer of 1842 with the corresponding changes of temperature†. That is to say, I have projected by *periods* (corresponding to the intervals of observation on the glaciers) the mean temperatures as observed at Geneva and at the Great St. Bernard, which are regularly published in the *Bibliothèque Universelle*, the average of which (separately deduced from the mean of daily maxima and minima, and projected in the upper part of the figure) may represent not inaptly the average temperature to which the glaciers in question, and especially the middle and lower regions of them, are exposed; and further, this average possesses the advantage of being derived from data wholly unconnected with the place or parties where and by whom the observations on the motion of the glaciers were made, and therefore are free from the remotest suspicion of either in any degree influencing the other.

* This explains a circumstance which has always hitherto been a difficulty to me; the united testimony of the best-informed inhabitants, not only at Chamouni but elsewhere (as at Zermatt and at the Simplon), to the effect that during winter the lowest end of a glacier, which terminates in a valley, does not greatly protrude, nor force the snow before it. This arises in fact from the comparative smallness of the motion which the *tongue* of such a glacier appears to possess, especially in winter.

† Travels, p. 141.

FIG. 1. *p. 186.*

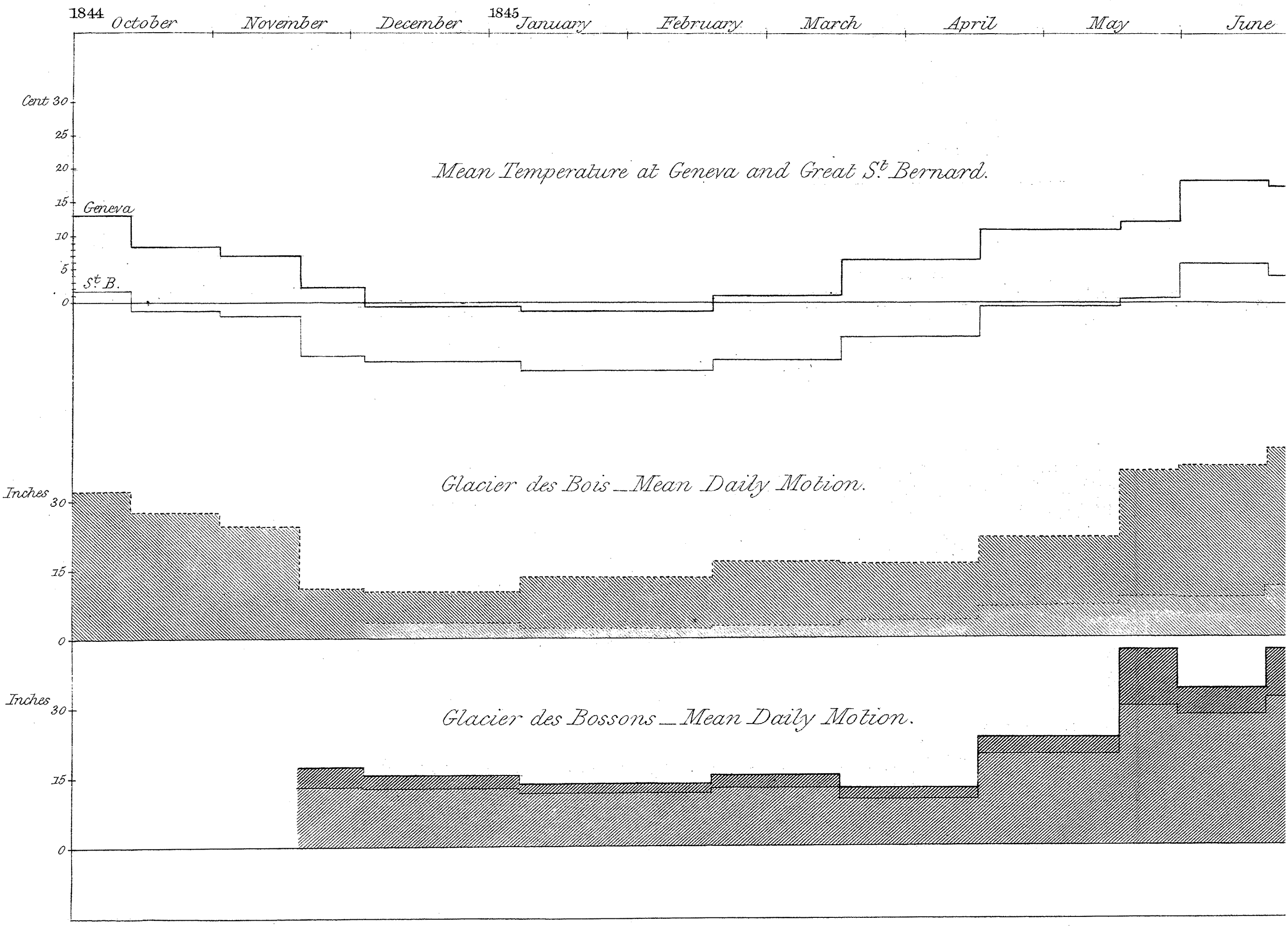
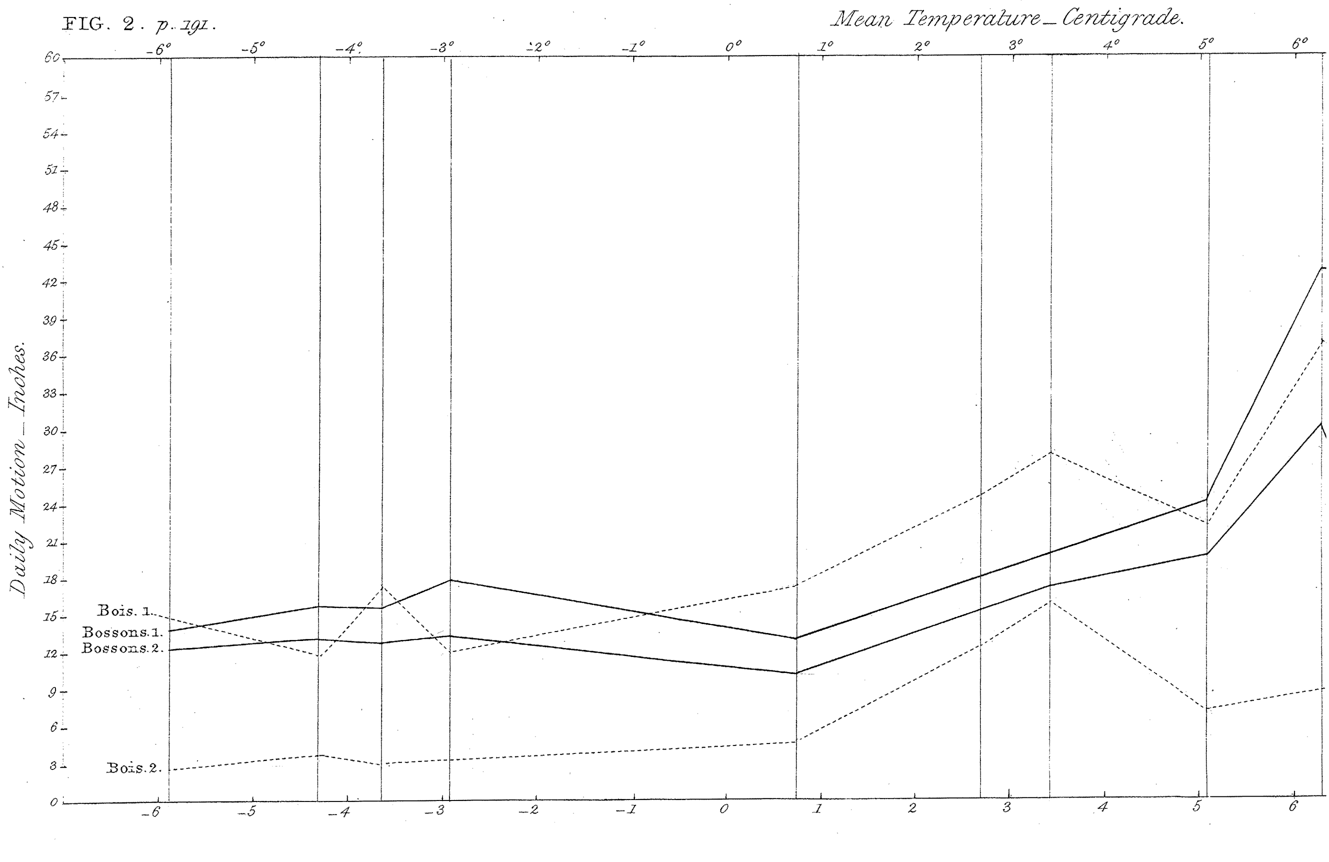
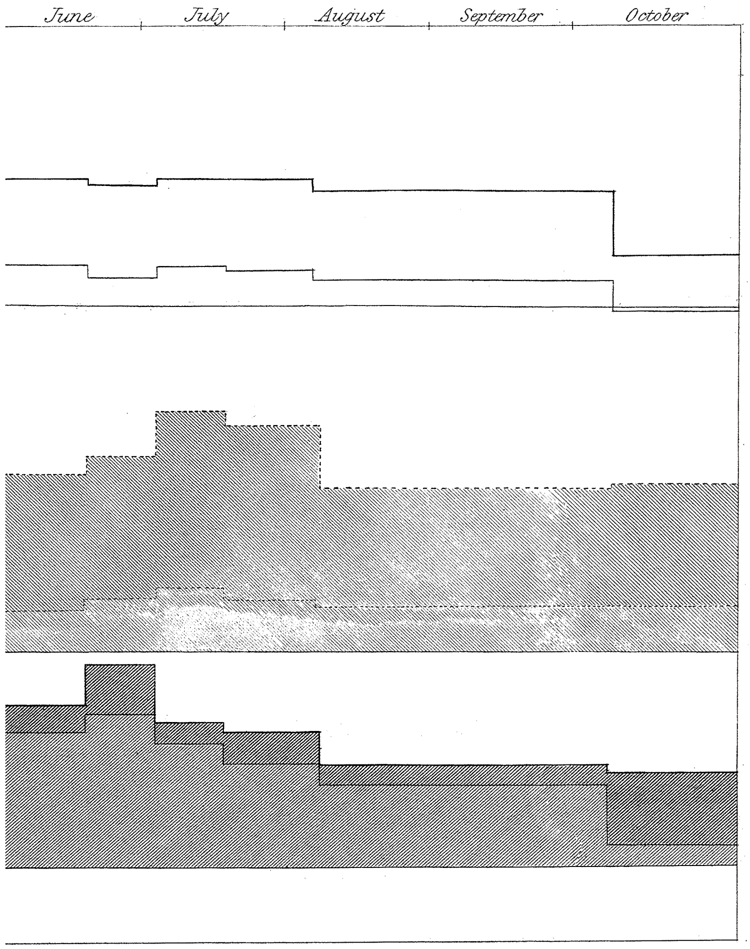


FIG. 2. *p. 191.*




June
July
August
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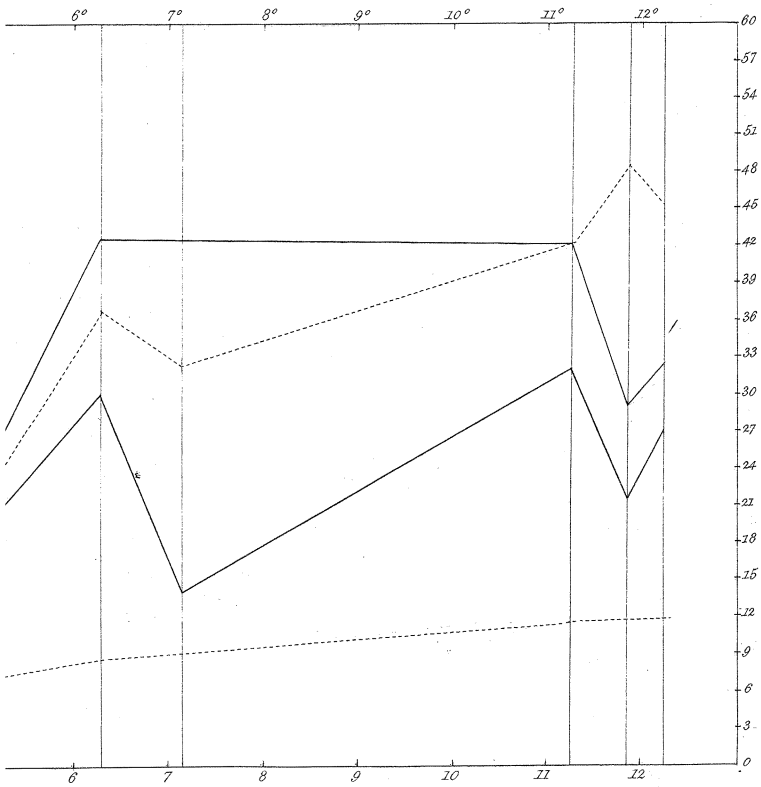
6°
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27
24
21
18
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12
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3
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12

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Table VI.

Mean Temperatures (by periods) on the Centigrade Scale, observed at Geneva and the Great St. Bernard*.

	Geneva.		St. Bernard.		Means of Max. and Min.	
	Max.	Min.	Max.	Min.	Geneva.	St. Bernard.
1844. Oct. 2 to Oct. 14. ..	17·37	8·57	4·16	− 1·58	12·97	1·29
Oct. 14 to Nov. 2. ..	11·77	5·13	1·61	− 4·93	8·45	− 1·66
Nov. 2 to Nov. 19. ..	11·84	3·11	1·62	− 5·66	7·47	− 2·02
Nov. 19 to Dec. 4. ..	4·93	− 0·18	− 5·11	−11·25	2·27	− 8·18
Dec. 4 to Jan. 7.	1·45	− 2·40	− 5·70	−10·96	− 0·27	− 8·33
1845. Jan. 7 to Feb. 18. ..	1·47	− 4·11	− 6·92	−13·86	− 1·32	−10·39
Feb. 18 to March 18.	4·76	− 2·74	− 3·93	−12·56	1·01	− 8·24
March 18 to April 17.	11·27	1·57	0·19	−10·13	6·42	− 4·97
April 17 to May 17.	16·04	5·56	5·15	− 6·36	10·80	− 0·60
May 17 to May 31. ..	16·83	6·91	6·04	− 4·67	11·87	0·68
May 31 to June 19. ..	23·70	12·71	11·48	1·11	18·20	6·29
June 19 to July 4. ..	22·58	12·13	9·81	0·53	17·35	5·17
July 4 to July 18. ..	24·01	12·84	10·20	2·08	18·42	6·14
July 18 to Aug. 6. ..	23·28	13·20	9·25	1·68	18·24	5·46
Aug. 6 to Oct. 8.	21·15	11·31	7·57	0·10	16·23	3·83
Oct. 8 to Nov. 8.	11·84	3·78	2·38	− 3·70	7·81	− 0·60
Nov. 8 to Nov. 21. ..	12·54	5·86	− 1·03	− 6·06	9·20	− 3·54

A general comparison of the curves of temperature and those of glacier motion (more particularly on the Glacier des Bois) affords a proof of the justness of the principle laid down by me in 1842, that the motion of the ice "is more rapid in summer than in winter, in hot than in cold weather, and especially more rapid after rain, and less rapid in sudden frosts†;" the evidence of the connection is plainer by mere inspection than any detail could make it. But I request attention to the apparent anomalies of the curves, as affording a stronger evidence of the fidelity with which the measurements have been made, and to the truth of the plastic theory, than perhaps even the general coincidence just referred to.

If the velocity of the glacier depend upon the completeness of its infiltration with water, rendering the whole an imbibed porous mass like a sponge, it cannot depend solely on the mean temperature of any period, but also upon the *wetness* of the surface, whether derived from mild rain, from thawing snow, or from any other meteorological accident which the register of the thermometer cannot of itself indicate‡. Further, a thick coating of snow on the glacier must defend it from the excessive cold of winter just as it defends the earth and plants, and consequently the minimum of motion will not necessarily coincide with the minimum of temperature. Now to

* The last three lines have been added during printing.

† Fourth Letter on Glaciers, Edinburgh Philosophical Journal, Jan. 1843; and Appendix to Travels, 2nd edit. p. 415.

‡ "The *proportion* of velocity does not follow the *proportion* of heat, because any cause, such as the melting of a coating of snow by a sudden thaw, as in the end of September 1842, produces the same effect as a great heat would do."—Travels, 2nd edit., p. 372.

estimate these more irregular causes is not so easy; but some light is thrown upon them by a register of the weather and state of the snow, voluntarily kept for me at Chamouni by AUGUSTE BALMAT; which forms a valuable supplement to the thermometrical register of Geneva and St. Bernard. Although the daily details would take up too much space, I will endeavour to give a faithful abstract of them so far as to give a general idea of the climate of Chamouni from October 1844 to November 1845. This diary includes (at my request) occasional notes on the state of the source of the Arveiron, which are of considerable interest.

Weather at Chamouni.

1844. *October.*—A good deal of rain during the month, which on the 10th and 16th fell as snow on the hills (nine inches at Montanvert), and subsequently to the latter day the glacier at the Montanvert was not clear of snow during the winter. 14th. Source of the Arveiron diminished to *one-fourth* (of the summer volume). Ice-vault more than half closed.

November.—Till 14th much rain and snow; fine with frost after. 20th. Source of the Arveiron very low; has not shifted its usual position.

December.—Weather generally fine throughout; cold most severe from 7th to 12th.

1845. *January.*—The weather continued splendid till the 20th; greatest cold from -2° to -5° REAUMUR. 19th. The vault has disappeared at the source of the Arveiron. 20th. The first snow fell which lay at Chamouni, and continued from this day, attaining a depth of $1\frac{1}{2}$ foot in February. Up to this time all the secondary heights, even the Breven and Flegère, were clear of snow, and the weather suitable for Chamois hunting. Occasional snow till the end of the month.

February.—Snow at intervals all the month. 13th. Greatest cold of the season; thermometer -15°REAUM. followed by fine weather. 20th. Snow lies $2\frac{1}{2}$ feet deep at the upper stations on the glaciers; $1\frac{1}{2}$ foot at Chamouni. The arch of the source of the Arveiron has wholly disappeared, but the water issues at the usual places as in summer. The water is reduced to a small amount and may easily be stepped across. It is *still whitish and dirty*, though less so than in summer; *except* when a change of weather is threatened (when it is as dirty as in summer)*. *Same date.* The glacier of Bossons has extended itself much. "On ne s'y reconnait presque plus." It is advancing towards the moraine of 1818; and the lower end is at least seventy feet high.

March 1st—3rd, mild, with rain; 3rd—13th, cold; 15th, *heavy rain.* Alternate rain and fine till end of the month. 27th. Not half a foot of snow lying at Chamouni. The source of the Arveiron has not opened a vault. The quantity and muddiness of the water the same as at the last report.

* This important remark proves that in the middle of winter a temporary rise of temperature of the air over the higher glacier regions (which is the precursor of bad weather) not only produces a thaw there, but finds the usual channels still open for transmitting the accumulated snow water.

April.—First week fine; second week cold with snow; changeable to the end of the month. 16th. Source of Arveiron has not much increased in water since the middle of March. In the end of April the snow first disappeared from the lower part of both glaciers.

May.—The first half of the month fine, with occasional snow; the second half changeable, with rain. 17th. The source of the Arveiron has increased three-fourths (*means probably in the ratio of four to one*) since the middle of April, and is dirty. The ice-vault is not yet formed. 26th. The Glacier des Bossons advances rapidly and is crumbling into pyramids. The end of the glacier is at least eighty-five feet high and advances considerably, particularly during the month of May; and widens greatly.

June.—A changeable and wet month; a very late season*. The snow did not entirely disappear from the Mer de Glace opposite the Montanvert till the beginning of July. 6th—7th. The vault opened at the source of the Arveiron. The quantity of water since the end of May is the usual summer supply.

July.—Commenced with warm weather. 5th. Thermometer 27° REAUM. The snow has disappeared from the ice opposite Montanvert, but some patches remain on the way to the Jardin. The Mer de Glace is much higher in level (about forty feet) than in former years, and the marks made in the rock at the *Angle* (in 1842) are all covered. The crevasses much the same as usual. The glacier of Bossons has also increased greatly, and appears to be approaching its old moraines. The register for the greater part of July has not come to hand.

August.—A very changeable rainy month. 8th or 9th. The arch at the source of the Arveiron fell in, and did not form again during the season.

September.—Also a changeable month. Rain twelve days.

October.—A very fine month. No rain mentioned after the 7th.

A careful examination of this interesting register will explain several of the apparently irregular inflections of the curves of glacier motion. Thus (to continue our general remarks, p. 186) we find

V. At the upper station on the Glacier des Bois the least velocity occurred in December, whilst at the lower station (and at both of those on the Bossons) a minimum coinciding also with that of the temperature of the air took place in January. This coincides with the important fact noted in the preceding register, that the upper part of the Mer de Glace was covered with snow from the 16th of October, which only lay in the valley of Chamouni from the 20th of January; the snow screening the ice from the extremity of the cold.

VI. The comparative march of the two glaciers bears a remarkable relation to their positions and form. In the Bossons we detect at once the sudden transitions and seemingly capricious changes of a torrent; in the Mer de Glace we have the

* It will be seen from the temperature curves that the thermometer *fell* considerably in the latter part of June, both at Geneva and St. Bernard.

stately and regulated flow of a river, in which the slighter variations are absorbed by the predominant inertia of a comparatively stable mass. Now the glacier of Bossons is, as every one who has seen it knows, a mere icy torrent, "a frozen cataract," which descends in a continuous mass from the level of the Grand Plateau of Mont Blanc to that of the Valley of Chamouni with very little impediment, with no confining bulwarks of rock, no contracting straits; and throughout this great vertical height of at least 9000 feet, the angle of descent is very steep indeed for so vast a mass. On the other hand, though the part of the Mer de Glace, called the Glacier des Bois under the Chapeau, is very steep, its "*régime*" is regulated by the supply derived from the reservoir glacier above, and, precisely as in rivers of great magnitude and length of course, and of moderate declivity, it yields sluggishly to impulsive or retarding forces which are checked and opposed by the multitude of sinuosities, the embaying of the ice in rock-bound expansions of the channel, the struggle of its passage through defiles and the enormous friction of its lower surface. Yet, lest we might attribute the irregularities of the torrential glacier to causes quite local and uncertain, we find them reflected more or less distinctly in the movements of the neighbouring one. Thus the anomalous retardation in the end of March and beginning of April appears in three stations out of four, as does that in the first half of June, showing clearly that it is not an error of observation. It appears that the thaw of the winter's snow during the month of May, saturating the pores of the glacier with water, produced (as we know that a thaw always does) a sudden and violent march, especially of the more susceptible or torrential glacier. So completely had this sudden move forced on the glacier of Bossons, encumbered by the spring avalanches and loaded with all the fragments and snow masses which had remained temporarily suspended during the winter months, that the lower part of the glacier (as we read in the memoranda to the register) advanced and widened greatly, to an extent which it had not done for many years past, and seemed to change its whole character; and in February a similar temporary increase of volume had taken place; "on ne s'y reconnait presque plus," writes Balmat; thus accounting for the particular accession of speed which appears in that month. In both cases, after the rapid march in February and in May, a reaction takes place; the material is deficient, the excessive pressure has been removed by the previous overflow, and a lull occurs in March and in June.

VII. These irregularities, such as they are, even should we fail in entirely explaining them, are at least not to be attributed entirely to errors of observation, since different observations (which it is to be recollected were sent to England in so rough a state that they required to be reduced and computed before the variations of velocity could be deduced from them) agree amongst one another, and agree with the phenomena casually noted in the Meteorological Register. They are very trifling in the movement of the Glacier des Bois, which presents a curve of remarkable regularity, giving a minimum about the end of December, and a maximum in July. The coin

cidence with the curve of temperature is greater throughout than we could have expected, considering the important difference of circumstances which occur in autumn and in spring when the thermometer stands nearly alike, the first chill of autumn depriving the glacier of its fluid pressure more effectually than the severer cold of winter which is tempered by its snowy covering, whilst in spring the first relaxation of the bands of frost saturates the icy mass with the impetuous streams of melted snow, as effectually as the intensest heat of summer. In fact, the velocity would probably be greatest in spring, were it not that then the ice has attained its greatest consolidation by the slow but continued effect of the winter's cold penetrating its upper layers, though after all probably to no very great depth. But this is undoubtedly the reason why the minimum and maximum approach so near to one another in point of time in the torrential glacier of Bossons, and it receives an important illustration from the independent fact of the observed condition of the source of the Arveiron, which (see the Meteorological Register), though very small in February, was still whitish and dirty before a change of weather, showing that the bands of frost were not so strong as to prevent a temporary relaxation of thaw throughout the mass of the glacier even in winter; and although the *mean* temperature of the air had been rising ever since the middle of January, and the greatest cold had occurred early in February, we find that at the end of March the source of the Arveiron was still as small as in February, and that owing to the coldness of the spring it had not even increased very much till the middle of April, when it almost suddenly resumed its summer volume. Now during all this time the velocities of the glaciers underwent but little change,—some oscillations backwards and forwards,—but took no real start until the frost had given way, and the tumultuous course of the Arveiron showed that its veins were again filled with the circulating medium to which the glacier, like the organic frame, owes its moving energy.

VIII. Being curious to see how far a relation might be established between the temperature of the air and the motion of the glacier independent of the irregularly acting causes above adverted to, I projected in Plate XI. fig. 2, the motions of the several points of the glaciers in terms of the temperature of the air for the periods already mentioned. It is to be recollected, however, that the observations of the thermometer were not made on the spot, and indeed it would have been difficult to have fixed upon a spot which should represent the mean circumstances of the whole glacier. Perhaps, therefore, the average of the observations at Geneva and St. Bernard (the mean of whose elevations is 4750 English feet above the sea, and therefore between that of Montanvert and Chamouni) may represent pretty fairly the climateric conditions of the inferior parts of the Glaciers des Bois and Bossons. Now, if we examine the curves of fig. 2, we are struck with *their almost perfect flatness until zero of the centigrade scale of temperature is reached*; but, the thawing point of ice past, the velocity manifestly goes on increasing with the temperature, in a ratio which would appear to be tolerably uniform if we neglect the irregular inflections of the curves.

IX. I am unwilling to multiply deductions which every intelligent reader will draw for himself; but one more I must add. It very clearly appears that the variations of velocity due to season are greatest where the variations of temperature of the air are greatest, as in the lower valleys; but it also appears from Remark VIII., that variations of temperature below 0° centigrade, or 32° FAHRENHEIT, produce almost inappreciable changes in the rate of motion of the ice. Hence, from this circumstance alone, we should deduce that in the higher parts of the glacier (where, for example, it freezes almost every night in summer) the variations of velocity should be least, and indeed comparatively small at different seasons. This is well illustrated by comparing the summer motions of the stations D, A and C, mentioned in the first part of this section, with their annual motion, which exhibit a much slighter excess in favour of the summer period than in the lower stations which we are now discussing. The same thing was observed by M. AGASSIZ's surveyors on the glacier of the Aar, who at first saw, in this not very great inequality, an objection to my theory. On a more searching investigation, however, the objection disappears, as in their later writings they have acknowledged. Their position of observation far up on the glacier of the Aar, in a spot having a mean temperature near the freezing point if not lower, had a summer daily motion of 7·99 inches, and a mean daily motion during the whole year of 6·41 inches*. Now at station C, or the Pierre Platte, on the Mer de Glace, the mean motion for July 1842 was 10 inches, and for the whole year, 1842–43, it was 8·56 inches. It is quite evident that the motion of any point in the midst of a glacier is controlled by that of those which precede and follow it, and that it does not necessarily result, either that all must at once suffer a similar increase or diminution of speed, or that the times of maxima and minima, or even the general form of the annual curve, shall be the same. This leads to an important practical result which we shall follow out in the next section.

§ 8. *Summary of the Evidence adduced in favour of the Plastic or Viscous Theory of Glacier Motion.*

It is often difficult to obtain a calm and full hearing for any new theory or experimental investigation; not because there is any antipathy to novelty, or that experiment is undervalued, but simply because, in an age of bustle and struggle for preeminence, each man is so busy with his own reputation, or the means of increasing it, that he has no leisure to attend to the claims of others; to which may, perhaps, be added, that in the general diffusion of knowledge and acquirement, each reader, finding something in every course of experiment or reasoning which he knew, or thinks he knew before, is apt to run off with the chain of ideas which that one familiar link suggests, and losing patience to follow an argument of which he thinks he can, by his own penetration, anticipate the close. He sits in judgment on errors which are of his own invention, and confronts the author with arguments and opinions already

* Comptes Rendus, Dec. 9, 1844.

thrice refuted and rejected by himself. In an age when all men would be teachers and all write for the press, the lot of an attentive reader falls to few.

I am far from saying that I have been more than usually unfortunate in this respect. But having, like others, seen my opinions disfigured for want of sufficient attention to apprehend them, or the arguments by which they are supported; ignorance of first principles hinted at, and even errors of observation imputed, where it was convenient that such ignorance and such errors should be presumed; I claim the privilege of stating afresh, though very briefly, the leading opinions which I do hold, and some arguments for them, which, if not altogether new, may be placed in a new light.

My chief analogies for the illustration of glacier motion have been drawn from the motion of a river, and by that comparison it in a great measure stands or falls. Slight and partial as is our knowledge of the mechanics of imperfect fluids, the explanation which I have given is founded upon that knowledge, and it appears to me to be sufficiently precise to warrant the inference of an identity of the mechanism in the two cases;—namely, that the movement is due to the internal pressures, arising from the weight of the mass, communicated partly or principally in the manner of hydrostatic pressure throughout a body whose parts are capable of moving or being shoved over one another (by that exertion of force which Dr. THOMAS YOUNG calls *Detrusive Force**, which overcomes what is commonly called the Friction of Fluids), so that the velocities vary from point to point of the moving body, being most rapid near the surface and centre, and least so near the banks and bottom.

So viscous fluids move, so bodies (even brittle solids, such as hard-boiled pitch) possessing the ordinary properties of solid bodies often do, if sufficient time and sufficient force be allowed†; the efficiency of time being chiefly this, that a pressure insufficient to produce instant detrusion, will, sooner or later, cause the particles to slide insensibly past one another, and to form *new attachments*, so that the change of figure may be produced without positive rupture, which would reduce the solid to a heap of fragments. This change may either take place without any loss of homogeneity, or by numerous partial and minute rents not everywhere communicating, and therefore not necessarily destructive of cohesion, which may be termed a bruise.

A glacier is not a mass of fragments.—As the analogy of the glacier to a river, in which the fluid principle is greatly in defect, and the cohering or viscous principle is greatly in excess, is the theory which I maintain, it is evident that the analogy of a stream of sand, or loose materials shot from a cart, or any other comparison with an aggregate of incoherent fragments or individual masses, must be wrong if mine be right. And I feel confident, not only that such an incoherent mass could not move after the manner of a glacier, but also that attentive inspection of a glacier at once contradicts such an idea.

* Lectures, I., 135.

† See Professor GORDON's Experiment, Philosophical Magazine, March 1845.

On the *first* point, I maintain that a rugged channel, like that of a glacier, with a moderate slope, being *packed* with angular solid fragments, would speedily be choked, and that farther pressure from behind (for such a mass can only convey thrusts, not strains) would tend to wedge the fragments more tightly. Some grains of dry sand will slide easily down a plate of glass; but try to thrust it forcibly through a narrowing tube, or even a uniform one, the lower end of which rests on a surface over which the sand has poured, and your effort is vain, the tube will sooner burst; and even rocks may be blasted rather than the power of the wedge yield*. If the figure of the bed or channel be in any degree irregular, that is, have expansions and contractions, however smooth its surface, however small the sliding angle of ice upon that surface, the choking of a strait or contraction by the piling of the fragments will be as complete and effectual as if the lateral friction were excessive. Now in point of fact we have such cases as this;—a glacier 2000 yards wide (the Mer de Glace at the Tacul) issues by an orifice or strait 900 yards wide;—the glacier of Talefre, a nearly oval basin, pours out its annual overcharge by an orifice the breadth of which is but one-third of its lesser, one-sixth of its greater diameter†. On the supposition of jostling fragments, the facility of motion is increased, as the comminution is greater. The impossibility of the discharge of a fragmentary solid through a gorge by long stripes fractured parallel to its length, and constituting parallelopipedons of a certain breadth, is evident.

Crevasses.—In the *second* place, I maintain that actual inspection shows that a glacier is not the mass of fragments nor of parallelopipedons which some persons have, naturally enough at first sight, supposed it to be. In truth there is not an approach to such a condition in those glaciers which move over moderate slopes of considerable extent, which have very properly been assumed by all writers as the criterial examples of any theory; for it is not denied that portions of glaciers and glacier tributaries do sometimes fall piecemeal over precipices, each fragment descending by its separate and individual gravity, in the manner of an avalanche, although I am disposed to believe, indeed am sure, that the number of such instances is smaller than is usually imagined; and the angle requisite for such a tumultuous mode of descent is far greater than it has, perhaps, always hitherto been considered to be. To him who would form a just estimate of the mechanical constitution of a glacier—who would consider it as a whole—without *always* distracting his attention from the length and breadth of the problem by a minute attention to its lesser features,—I would earnestly recommend the frequent and attentive survey of a glacier or glaciers from a considerable elevation above their level and under varying effects of light. Had I confined myself to studying crevasses on the surface of the glacier, measuring their depths, injecting the ice with fluids and taking its temperature; useful and important as these inquiries

* See HUBER-BURNAND's conclusive experiments on this subject, Ann. de Chimie et de Physique, xli. 166, and FECHNER's Repertorium, i. 65.

† See the Map of the Mer de Glace and its tributaries in my Travels in the Alps of Savoy.

are, (and I might almost include the fundamental and most important inquiry of all, that of ascertaining the velocity of its parts,) I should have been much longer in seizing the general truth of the individual character of a glacier, the importance of the fluid-like connection of its parts, the perfectly secondary importance or unimportance of the fissures by which it is often traversed. The traveller who winds his tortuous and sometimes perilous path amongst these crevasses, forgets, in the fatigue of his circumventions, in the wonder of his curiosity at their beauty and seemingly unfathomable depth, in the appalling steepness of their sides and the comparative insecurity of his own footing—he forgets, I say, in the midst of all these claims upon his attention, his curiosity, and his strength of mind, the comparatively large surfaces of unbroken ice over which he heedlessly walks, and the small, the very small depth at which most of the yawning crevasses which make such an impression on his imagination, dwindle into mere slits;—and when his walk is finished, he imagines that a glacier is a mere network of fissures interlacing in all directions. But let him gain a bold height above its surface, 800 to 1000 feet at least*, so that the whole may be spread somewhat like a map before him, yet not too distant to prevent his seeing the number and forms of the crevasses, and estimating their area compared to that of the unbroken ice, his opinion is first shaken and then changed. He sees in the glacier a *whole*, which, regarded as such, is merely scarred, not dissected by these fissures;—he sees a mass as capable at least of conveying strains as thrusts; of which the cohesion is no more destroyed than (to use a comparison which I long ago employed) a parchment sieve is incapable of being stretched, because it is covered with fine slits.

I am confident that this will be plain to every unprejudiced person who will make the observation which I have recommended, and I have no hesitation in stating my belief that it will be found to be fully confirmed by M. WILD's map of the glacier of the Aar, should it ever be published; I say so without having any recollection how the matter stands, although I once had an opportunity of seeing that fine work for a few minutes; and the verification of this remark, by positive measurement, will, so far as I see, be the chief result likely to flow from the patient and disinterested labour of that competent surveyor.

But if this be true in a merely *superficial* plan, how much more true would it be if we could pare off the upper stratum of the glacier, and view a horizontal section of it at a depth of a hundred feet! The depth of the crevasses has, I am persuaded, been as much exaggerated as the thickness of the ice of the glacier has been underrated. In how few cases (where a glacier does not descend tumultuously) can we

* I may mention, as the very best stations which I am acquainted with, the summit or higher slopes of the hill of Charmoz above Montanvert, Station G*, above Trelaporte, and a point directly above the Couvercle at least 1200 feet higher than the Mer de Glace, which may easily be reached from the glacier of Talefre. Other glaciers offer of course similar points, but few so advantageous; the glacier of the Aar from the Schneebighorn, the lower glacier of Grindelwald from the slopes of the Mettenberg, the glacier of the Rhone from near the Mayenwand, and that of Zermatt from the Riffelberg, are examples.

let a plumb-line down even fifty feet without grazing the sides! and to what an insignificant fissure has the gaping crevasse dwindled even at that small fraction of the glacier's thickness! Supposing the crevasse to become uniformly narrower, how soon would it be extinct!

Again, the crevasses which traverse the surface of the glacier have almost always a determinate direction or directions, of which the simplest type seems to be that of perpendicularity to the veined structure*, which, generally speaking, occasions a convexity of the lines of fissure towards the origin of the glacier. Opposite Montanvert the crevasses form two systems inclined 65° to one another, but this appears to be a casual occurrence arising from a fresh strain being imposed on the ice owing to its rigidity when the direction of the bed or trough suddenly changes, and the two-fold systems probably coexist but for a short space, one tending to close whilst the other opens. Be this as it may, unless where a glacier is falling headlong in the manner of a cascade, the crevasses do not produce any actual dislocation of its mass into blocks or fragments, since the crevasses rarely intersect even where most numerous, but almost invariably *thin out* in the solid mass, whilst another crevasse takes its origin a little to one side or other, leaving a firm connection of ice between them; and the difficulty and danger of traversing a glacier where much crevassed, does not arise from the necessity of leaping from square to square of ice, but from having to traverse these bridges of icy communication, which even there link the glacier together, and which are almost always sharp on their upper edge when the season of the year is pretty far advanced, owing to the continual dripping.

The occurrence of crevasses which cut up a glacier into square or trapezoidal blocks, is sufficiently infrequent to deserve notice. Such occur when a glacier of the second order descends over a boss of granite, or a surface convex in all directions. We have then radiating crevasses combined with concentric ones, producing a tartan-like appearance. Such may be seen in a glacier of the second order on the south side of the Aiguilles of Charmoz and Grepon, above the Glacier du Géant; and it is a very convincing proof of the *essential tenacity* of a glacier, that, with a surface so scarred and intersected, the fragments do not fall away in avalanches. This only is to be explained by the consideration that, thin as are the glaciers of the second order, the apparent dislocation is only superficial.

Were the inequality of the central and lateral movement of the glacier mass to be attributed to longitudinal fissures or discontinuities, by means of which broad stripes of ice slide past each other, we should have to demonstrate the existence of such fissures, which could not be always close unless either (1) the surfaces were mathematically adapted to slide over one another, or (2) the ice possessed sufficient plasticity to mould the surfaces to one another's asperities, in which case the plasticity would alone be sufficient without the discontinuity to explain the motion of the ice. These longitudinal fissures, cutting the common transverse fissures perpendicularly,

* Travels in the Alps, p. 171.

would divide the glacier even where most level into trapezia, and no transverse crevasse could be straight-edged but must be jagged like a saw, or cut *en échelon.* Such a phenomenon never occurs unless where a glacier is moving *torrentially,* or with great disturbance and down a steep. *There* such longitudinal fissures may occasionally be seen, but they form the exception and not the rule. It has been demonstrated by an elaborate proof in § 5, that the only trace of longitudinal discontinuity in the normal condition of the glacier is to be found in the veined structure, which, being caused by a partial discontinuity at a vast number of points, admits of an insensible deformation of the glacial mass without sudden or complete rents, or slips, or the formation of zigzag crevasses.

The existence of the great transverse crevasses, which, even in glaciers not moving torrentially, divide the surface of a glacier by rents perhaps 2000 feet long*, have been thought by some to be comparable to beams of an elastic material, supported at the two ends, and bending under their own weight forward, in the middle. Were this the case, it would scarcely modify the plastic theory as I have propounded it; because in order that such a bar of ice should conform to the known movements of the glacier, opposite the Montanvert for instance, the centre must continually gain upon the sides at the rate of 150 feet per annum at least, consequently the limit of cohesion of an elastic solid would soon be overpassed, and plasticity in the material sufficient to explain the whole motion would inevitably be admitted at last. Independently of this, it is evident, that were such a flexure essential to the motion, the lines of crevasses would be convex in the direction in which the glacier is moving instead of towards its origin.

Argument from the Equable Progression of Glaciers.—The equability of the motions of the various parts of a glacier, united as I have shown them to be by intricate relations†, must, I think, appear conclusive to every one capable of forming a just opinion on the subject, that the relative movements of the various parts of the glacier are due to the action of forces at small distances and to the antagonism of molecular cohesions and molecular strains, and not to the casual jumbling of a quantity of rude fragments. To myself, I confess that this now appears the strongest argument of all for considering the glacier as a united mass like a river, in which there is a nice equilibrium between the force of gravitation, acting by hydrostatic pressure, and the molecular resistances of the semi-solid; the degree of regularity of the law which connects the partial movements is wonderful, and I maintain that it is inexplicable except upon the viscous theory. Thus (1) the glacier moves continually, summer and winter, day and night, and never by fits or starts; for if it does—if gravitation overcomes mere friction, it occasions a shock or avalanche; (2) its mean annual motion is nearly alike from year to year; (3) the relative velocities of points widely distributed over the glacier (but exposed to similar influences of climate), change simultaneously in the same directions, often in the same proportions; thus "the variation of velocity

* Travels, p. 171, 2nd ed.

† See § 5 of this paper, pages 167 and 168.

in the breadth of a glacier is proportional to the absolute velocity at the time of the ice under experiment*." (4) The progression of velocity from the side to the centre is marked by insensible gradations†. (5) When we compare the motion of a given point of a glacier any day of one year and the same day of another, the probability is that the velocity will be exactly the same, if the season be equally hot or cold; hence, surely, a most unexpected result, which I first announced in 1842, that *a few days' observation of a glacier will enable any one to compare its mean rate of motion over its various parts and with different glaciers.* Thus, the motion of a point marked D 2 on the Mer de Glace, was in 1842, from August 1 to August 9, $16\frac{1}{2}$ inches daily; from August 9 to September 16, 18 inches; now next year, 1843, *one* observation at the same point in August gave 16 inches; and in 1844, *one* observation in September gave $17\frac{1}{2}$ inches. But still further, (6) the very law of flexure of the ice is the same from year to year: a series of stations across the ice at the Montanvert gave, in 1842, the following (simultaneous) relative velocities‡:—

1·000	1·332	1·356	1·367.

The same points being recovered in 1844, the relative motions were (by a single observation of the space moved over in five days)—

1·000	1·339	1·362	1·374,

ratios almost the same but slightly increasing, which corresponds with the fact mentioned above (3), that when the absolute velocities are greater, the relative velocities are so too, which was here the case, for the velocity denoted by 1·000 was a little greater in the second case than in the first.

Tensions and Thrusts.—The occurrence of open crevasses plainly indicates the existence of strains in the ice of glaciers producing disruption, at least partially. Hence some writers have precipitately inferred that the whole glacier must be in a state of tension; an uncertain inference surely in a problem of singular complexity, and one which is not warranted by a more accurate analysis. Yet for a time rival theories seemed poised on the inappropriate question, "Are glaciers in a state of internal tension or compression?" Even if the glacier moved as a mass of fragments, therefore without tension, the cohesion must first have been broken before it could be reduced into fragments. I have been inconsiderately censured for quoting, with approbation§, the observation of M. ELIE DE BEAUMONT, that a glacier appears to be rather in a state of distension than compression, whilst I adopted a hydrostatic pressure, acting from the origin as the source of motion. A careful examination of the passages in question will show that my assent to the view of M. ELIE DE BEAUMONT was limited to *portions* of the glacier, and especially to those portions most crevassed, the parts, namely, which connect the sides and centre, and which serve to drag the more sluggish, because retarded, lateral portions after the freer central part

* Travels, p. 148.

† See § 5 of this paper.

‡ Travels, 1st edit., p. 146.

§ Travels, 1st edit., pp. 178, 370; 2nd edit., p. 370 and *note*.

on which the *vis a tergo* acts with most advantage; and in a direction generally parallel to the blue bands, so far as they are due to inequalities of motion in the horizontal plane*. My earliest attempts to obtain clear views of the internal forces acting on a semi-rigid body, impelled by self-contained hydrostatic forces, convinced me how little could be founded on the completeness of any mathematical investigation of them, which in our present state of knowledge may well be considered as hopeless; and reserving to myself the not so difficult task of extricating at a future time the more important practical laws of these strains and thrusts, I very carefully avoided, in my first publication, any allusion to what might be considered as their actual distribution; a distribution varying not only from point to point of the glacier surface, but throughout its thickness, and most undoubtedly varying also for the same point at different seasons of the year, or even changing its sign, so that a tension at one season may become a thrust at another.

I had no reason to repent of this caution, from which I only departed so far in my Seventh Letter on Glaciers, published subsequently, as to deduce in an approximate manner, from elementary mechanical laws, the directions of the *surfaces of tearing* within such a mass as I had described, upon the simple supposition that the hydrostatic pressure acting uniformly, the tendency of motion of any particle will be in the direction of least resistance when all the resistances are taken into account, and that the surfaces of rupture will divide particles whose motions are dissimilar, but will not divide particles whose motions are alike. I repeat that I had no reason to repent of my abstinence from theorizing, when I found that a far better mathematician than myself, taking up the inquiry where I had left it, and after applying himself for a long time to the exclusive mechanical considerations which the viscous theory had suggested, left the subject, as I conceive, little more advanced than he had found it, and fell into some mistakes and inconsistencies, almost inseparable from this way of treating a problem which extensive observation and patient thought can alone disentangle.

Formation of Crevasses.—It has been seen in the third section of this paper, that De Saussure, and almost all his successors, have regarded the crevasses as *accidents* of glacier motion, and not essential to it; and in this view I of course concur. Nevertheless, the study of crevasses is one of considerable, though secondary interest, and is very far indeed from being completed. It requires, among other things, a very sedulous attention to the state of the glacier at various seasons, and even whilst covered with snow; and it requires further a two-fold classification of crevasses, into those which may be considered as proper to the mass of the glacier, and those which merely seam its surface.

I will first speak of the last point.

Though the formation of a crevasse betokens a local distending force, such a force cannot with any certainty be referred to the whole depth of the glacier below the

* See Philosophical Magazine, May 1845, p. 408.

point where the chasm opens. On the contrary, there is a fully greater probability that under that very spot the ice is compressed. If one cause of a crevasse be, as is universally acknowledged, a protuberance or inequality in the bed over which the ice is impelled, for the same reason that a beam, broken by means of weights, is in a state of longitudinal compression below, where its surface is concave, and of distension above, where its surface is convex, the cracks in the glacier may be due solely to this last and partial cause. Superficial crevasses may consequently be occasioned where there is no *general* distension of the mass, either (1) by the shoving of the semi-rigid glacier as a whole, over a convex declivity, or (2) from an internal turgescence arising from hydrostatic pressure, resisted by the intense friction of the anterior or more advanced parts of the glacier, which, causing the line of least resistance to be upwards and forwards, forces the pasty mass to tumefy or increase in thickness, exactly as it has been seen in § 2, p. 153, that sluggish lava streams do in a similar case. But if the tumefaction be pushed beyond the limits of plasticity of the superior and more distended portions, they must burst and assume the crevassed forms actually observed in the plastic models described in p. 144. Hence the existence of crevasses not only does not always result from a state of *general* distension in the glacier, but may arise from the precisely contrary condition of great internal compression. This argument is well illustrated by the recent observations of M. Agassiz's co-operators on the glacier of the Aar, whose observations I have elsewhere shown* to be incompatible with any other view than that of intense longitudinal compression in the mass generally, and yet the surface abounds in crevasses of the usual form and dimensions.

The manner of formation of crevasses generally, including such as may betoken a real distending force acting on any part of a glacier throughout its thickness, is not only a most curious question in itself, but suggests others which a correct theory of glacier motion can alone answer. If a crevasse once formed remain a fissure in the ice for ever after, why is the horizontal projection or ground plan of the crevasses of a canal-shaped glacier convex towards the origin of the glacier, and not protuberant in the direction of its motion, as the ascertained greater velocity of the centre would assign? Why are the crevasses for the most part vertical and not inclined forwards, or at least not notably so, on the same account? Why, if the glacier be urged downwards by a longitudinal force distending it, do not the crevasses continually widen in proportion as they are further from the origin? These questions seem incapable of a sound answer except by supposing that the crevasses are, at least in a great degree, the fresh production of every spring, and arise from the sudden start which the glacier makes when that extremity which descends into the valley begins to experience the thawing effects of returning summer. I should not wish to speak positively upon what involves a difficult if not impossible observation,—the state of the glacier with respect to crevasses whilst still under the winter's covering of snow. But the fact

* Ninth Letter on Glaciers. Appendix to Travels, 2nd edit., p. 443

of the transverse direction of the crevasses, or even their convexity towards the origin, from year to year, seems to admit of no other explanation. But besides this, I can affirm, from a careful observation of the crevasses of the Mer de Glace from June to September in one year, that the changes which they underwent were such as preclude the possibility of a crevasse of autumn being merely preserved by the snow of winter, and re-appearing afresh in spring as it had done the previous one. The thing is impossible, because the character of the crevasse is essentially altered. In order that an autumnal crevasse may become a spring crevasse it must be sealed up, annihilated, and opened again. A glance at the three sections in Plate X. fig. 3, will illustrate this. No. 1 shows crevasses freshly opened soon after the snow has quitted the surface of the ice—the edges are sharp, the sides vertical, the openings so small that they may be easily stepped across, and in other instances they are not wider than may admit the blade of a knife. No. 2 shows the crevasse opened to its widest extent by the acceleration of the motion, by the force of the sun which has altogether wasted away the side with the southern exposure, and by the copious drippings of the melting ice and mild rain. No. 3 (which as well as No. 2 is taken from a sketch on the spot, No. 1 being done from recollection) shows what I have elsewhere called the state of collapse of the glacier, which affords the most direct possible evidence of its plastic condition; for we there see, not merely the prominences worn away and blunted by the heat of summer, but subsiding into the hollows, the crevasses being choked by the yielding of their sides, and the glacier again resumes a traversable character, only that the plane surface of spring is changed into irregular undulations preparatory to a complete amalgamation of the whole glacier into one mass*.

The collapse is thus described in my Journal of 1842, written at the time, and therefore more emphatic and unbiassed than after my theoretical views had been matured and published. "1842, Sept. 16, Friday. The level [of the Mer de Glace at the 'angle'] has sunk since the 9th of August, nine feet 8½ inches. The effect of this immense fall is abundantly evident in this part of the glacier. On my first visit this time [*i. e.* after an absence of a month], on the 10th, I was quite struck with its shrunk appearance, as I was today with the collapsed state of the crevasses. There cannot be a question but that the glacier has subsided bodily into its bed, and that the semifused pliancy of its materials causes them to recover a uniform and lower level. The crevasses are much less deep than in July and August, as at that time they were larger and more numerous than in June. They are collapsed and (opposite Trelaporte) almost soldered up; the edges all rounded and melted by the sun's heat." The phenomena here described, "the shrunk appearance," "the *semifused pliancy*," "the soldered crevasses," "the rounded edges," convey to the attentive spectator an intuitive conviction of the plasticity of ice at the thawing season, which no words can

* See Travels, p. 174; and Fourth Letter on Glaciers.

express, no mathematical symbols weave into a demonstration. I can only say that it is easier to believe than to disbelieve; and that sooner or later, it will, I doubt not, be generally admitted.

Considering the crevasses as chiefly superficial in the normal glacier (I mean that of which the inclination is not excessive), it is evident that the formation of the crevasses must depend mainly upon the configuration of the bed. Where the section of the bed parallel to the length of the glacier is convex upwards, there the tension at the surface will cause the crevasses to expand; when the bed is concave and the surface is being compressed, the crevasses tend to close. Hence the surface of the glacier descending an irregular bed may be alternately in a state of distension and compression, and the crevasses do not tend to widen indefinitely, which would be the case if the whole glacier were distended. This tendency in the crevasses to expand and contract in accordance with their position is beautifully seen in viewing the Mer de Glace from a height, as we have recommended. The steep fall opposite Trelaporte shows the expansion of the crevasses, but the comparative level opposite the little glacier of Charmoz gives it time to recover its solidity by the general closing of the crevasses under compression. The careful study of such a scene as this gives a more clear insight into the glacier phenomena than any other part of the inquiry, excepting only the measurement of velocities.

Law of Velocities.—To these velocities we now return. The varying velocities in different glaciers, at different seasons and in different parts of the same season, are all in accordance with the motions of a viscous or plastic body. They depend upon the slope, being greatest, *ceteris paribus*, when the slope is greatest; and upon the climate to which the glacier is exposed, being greatest in glaciers which descend into deep valleys, and least in those which, though very steep (such as that of the Schönhorn described in § 6), are placed in so elevated and therefore dry and cold an atmosphere as to afford in sufficient water to moisten the snowy mass or *névé*, and which are therefore endowed with very feebly hydrostatic qualities. This is demonstrated on the one hand by the extreme smallness of their motions, and on the other by the insignificant streams of water to which they give birth even in the height of summer. In any individual glacier the velocity of the parts must (on any theory) vary with the area of section through which the ice stream has to pass; but yet it may happen that the contraction of a valley, if not accompanied (as is often the case) with an increased slope, will oppose so great a resistance to the efflux of the mass, that under intense longitudinal compression its forward motion is retarded, and the condition of uniform discharge is satisfied by the accumulation of the ice in a vertical direction, the rise of the surface being necessarily accompanied with a thrust from below upwards, and a sliding of the particles over one another in that direction. This appears conclusively to be the case for a great extent of the lower part of the glacier of the Aar, as already mentioned, and affords the most direct evidence which

could be desired, that the kind of internal motion necessary for producing the *frontal dip* in the veined structure (which arises from tearing or crushing in sliding in the vertical plane*) was correctly foreseen.

The law of velocities at different points of the axis of a glacier from its origin towards its termination, must evidently depend upon the configuration of each particular glacier. It may be constantly increasing from the origin to the extremity, it may be diminishing, or it may have alternations of increase and diminution; and upon this circumstance the frequency and magnitude of the crevasses will mainly depend. But the *régime* of the glacier, by which we mean to express the combination of circumstances determining its motion, varies from one season of the year to another, owing not only to the general influence of heat and cold, but also to the progressive communication of that influence to portions of the glacier in successive stages of elevation. Evidently the extremity nearest to the valley will receive the earliest and most violent impression of solar heat, whilst the middle and upper regions are involved in complete winter. Partial dilatations must take place in spring, partial condensations in the decline of the year; as is evident from the consideration that temperatures inferior to freezing do not sensibly affect the motion of the ice (see above, p. 192) which higher temperatures do, consequently the influence of season will be chiefly felt in those parts of the glacier where the temperature of the air seldom falls in summer to 32°, whilst the more stable motion of the higher part acts as a drag or equalizer upon the whole system. The condition of violent distension produces crevasses, that of violent compression produces the frontal dip of the veined structure, or that share of it which is due to the relative motions in a vertical plane. The longitudinal veins will result whether the axis of the glacier be distended or compressed. Hence the reason why the frontal dip is difficultly seen in all the middle region of a glacier, which like the Mer de Glace, is subject to much extension due to great and increasing declivity, and to be well seen must be sought for in the higher parts of the glacier, as above Trelaporte, at the foot of the Couvercle†, and in glaciers subject to great compression, as that of La Brenva, the glacier of the Rhone, the Aar, &c.

Ablation of the Surface.—One phenomenon is most satisfactorily explained by the variations of velocity established and illustrated in this paper. The collapsed state of the glacier after the hot summer of 1842, and the absolute lowering of its surface level by thirty feet in the space of a few months, had struck me as requiring an energy altogether extraordinary in kind and degree to restore next spring the level which had been lost, in order to allow for an equal ablation the succeeding summer; and at first I was disposed to admit so much of the dilatation theory to be true as would account for the swelling of the surface in a vertical direction by the freezing during winter of the infiltrated water‡. Further reflection convinced me however that this explanation was insufficient and also not required, and I accordingly concluded

* Seventh Letter on Glaciers. Appendix to Travels, p. 435.

† Travels, 2nd edit., p. 167.

‡ Fourth Letter. Travels, App., p. 415.

"that the main cause of the restoration of the surface is the diminished fluidity of the glacier in cold weather, which retards (as we know) the motion of all its parts, but especially of those parts which move most rapidly in summer. The disproportion of velocity throughout the length and breadth of the glacier is therefore less; the ice more pressed together and less drawn asunder; the crevasses are consolidated, while the increased friction and viscosity causes the whole to swell, and especially the inferior parts, which are most wasted*." I have nothing to add to this explanation, except that the observation of the motion throughout the whole year confirms it in every particular. The more elevated portions of the glacier, which during a large portion of the year are exposed to a mean temperature under 32°, move in a manner comparatively uniform, the lower extremities undergo great oscillations in their speed (in the ratio of four or five to one; see page 185); hence the attenuation during the summer *régime* which is owing to the drag taking place downwards in an excessive degree; but the winter's cold, equalizing in some measure the velocity everywhere, brings the plasticity into full action, fills the crevasses and swells the surface to its old level.

As it is universally admitted that the glacier proper does not grow in thickness by snowy accumulations, the important variations in its level in different years† cannot be ascribed to the severity of certain seasons increasing the mass of snow falling upon it, but rather to the prolongation of the winter cold into spring and summer, which causes the condensing or accumulating process to be in excess, and therefore the thickness of the plastic mass to accumulate beyond its due amount.

Thus we have the following phenomena, all independently observed, reconciled and explained by one hypothesis; the general convexity of the crevasses upwards, notwithstanding the excess of motion in the centre; the general verticality of the crevasses, notwithstanding the retardation of the bottom; the perfect state of the crevasses every spring succeeding their visible collapse in autumn; the ascertained velocity of different parts of the glacier, and the diversity of the annual changes which these velocities present; the seemingly opposed facts showing the glacier to be subjected to powerful tension, producing crevasses, and yet to be under a compression which produces in some places the *frontal dip*; and finally, the renewal of the level of the ice during winter, which has been lost partly by superficial melting, but as much or more so by the attenuation and collapse of the glacier during summer. These various effects of one cause, though they do not embrace all the phenomena of glaciers, certainly include a very remarkable and complicated group of facts.

* Travels, p. 386, 2nd edit.

† For instance, it has been seen from BALMAT's narrative (p. 189 above), that in 1845 the glacier attained a much higher level at the *Angle* than it had done for three previous years at least, since all the marks of measurements which were cut on the rock in 1842 were concealed; and he attributes this, apparently with reason, to the extreme lateness and coldness of the spring.

Plasticity—Veined Structure.—I certainly never expected, when promulgating the viscous theory, that it would have met with so much opposition on the ground that the more familiar properties of ice are opposed to the admission of its plasticity; and that the fragility of hand specimens should be considered as conclusive against the plastic effect of most intense forces acting on the most stupendous scale upon a body placed in circumstances which subject it to a trial, beneath which the most massive constructions of the pyramid-building ages would sway, totter, and crumble. In an age when generalizations of the more obvious kinds are no longer proofs of genius and perspicacity, and when popular writers on science delight to startle their readers by showing how bodies the most dissimilar possess properties in common; in an age in which *gradations* of properties and organs have been studied with such persevering sagacity, and in which so many unexpected qualities have been discovered;—when iron is classed as a combustible, when metals are found which float on water and which catch fire on touching ice, when a pneumatic vacuum is formed and maintained in vessels five miles long, and whose sides are ripped open twenty times a day; —when, moreover, the simpler abstractions of former times are being daily overset, when no body seems to possess any one property in perfection, and all seem to possess imperfectly every quality admitting of degree; when adamant is rejected from our vocabulary, and softness means only less hardness, and the definition of a perfect fluid is as imaginary as that of a solid without weight;—when a vacuum and a plenum are alike scoffed at, and even the heavenly bodies toil through media more or less resisting; when no substance is admitted to expand uniformly by heat, when glass may be considered a conductor of electricity, and metals as imperfect insulators;—in these days, when the barriers of the categories are so completely beaten down, I had not expected to meet with so determined an opposition to the proposition that the stupendous aggregation of freezing water and thawing ice, called a glacier, subjected to the pressure of thousands of vertical feet of its own substance, might not under these circumstances possess a degree of yielding, moulding, self-adapting power, sufficient to admit of slight changes of figure in long periods of time. Still less could I have anticipated that when the plastic changes of form had been measured and compared, and calculated and mapped, and confirmed by independent observers, that we should still have had men of science appealing to the fragility of an icicle as an unanswerable argument! More philosophical surely was the appeal of the Bishop of Annecy from what we already know to what we may one day learn if willing to be taught: "Quand on agit sur un morceau de glace, qu'on le frappe, on lui trouve une rigidité qui est en opposition directe avec les apparences dont nous venons de parler. *Peut-être que les expériences faites sur de plus grandes masses donneraient d'autres résultats**."

* Théorie des Glaciers de la Savoie, p. 84. Quoted in my Travels, p. 367, 2nd edit. Since this paper was read, Mr. Christie, Secretary of the Royal Society, has kindly communicated to me a very striking remark upon a well-known and easily-repeated experiment. The experiment is this. If, in the course of a severe

The "ductility" is indeed not great; the compact ice even of the slowest moving glaciers bears evidence, in the veined structure, or "blue bands," to the bruise which it has received from the all-powerful strain which has acted on it. When the difference of motions is excessive, or the slope occasions the speed to be greater than permits the gradual molecular adaptation of the semi-rigid parts to one another, the masses are broken up and fall more or less tumultuously; *the strain being then removed by the dislocation, the veined or bruised structure is invariably extinguished at last.* I shall quote a series of examples of the gradation of phenomena, which I conceive to be plainly connected by a common cause.

1. In any torrential glacier, such as the Glacier des Bossons, the upper part of the glaciers of La Brenva, Allalein, or the Rhone, and many others, the fractures are so numerous that the ice descends in blocks, almost as water in a cascade often does in spray, and hence the internal strains being destroyed, no structure is developed, or if previously developed, tends to wear out*.

2. In a glacier moving torrentially, that is with frequent and considerable changes of velocity, but without being divided into blocks by intersecting crevasses, we find real internal cracks in the ice, some feet in length, and an inch or more in thickness, marked by the pure frozen water which fills these spaces in the comparatively opake whitish ice of which glaciers descending rapidly from the region of the *névé* are composed. Such are peculiarly visible in the lower and more accessible region of the glacier of Bossons†; perhaps the most instructive which can be named as showing these infiltrated cracks, which by their dimensions, direction, and in every other particular, form a true link between the longitudinal dislocations of a torrential glacier, and the perfect veined structure or bruise into which it passes by imperceptible gradations, including a perfectly regular development of the frontal dip, where we might expect it to be well shown, for the observations of page 184 show that the lowest portion of

winter, a hollow iron shell be filled with water and exposed to the frost with the fuze-hole uppermost, a portion of the water expands in freezing, so as to protrude a cylinder of ice from the fuze-hole; but if the experiment be continued, the cylinder continues to grow, inch by inch, in proportion as the central nucleus of water freezes. "In the first instance," says Mr. Christie, "a shell of ice containing water was formed, no doubt, within the iron shell, and the fuze-hole might be filled by the expansion of the water in the act of freezing; so that there may be no reason for attributing plasticity to the ice as far as this goes; but the shell of ice once formed, and the fuze-hole filled with ice, the subsequent rise of the ice must have proceeded from the ice of the interior shell being squeezed through the narrow orifice. No thawing took place during the process. Does not this show plasticity even in very small masses of ice?" I have also been lately informed, on excellent authority, that in a new work by a most eminent German mineralogist, the plastic character of ice in masses is assumed as an admitted fact. In corroboration of what has been said in the text, I may farther add, that whilst these sheets are passing through the press, I observe in the Athenæum (June 20, 1846), an account of a patent process for moulding solid tin into tubes and other utensils, in the course of which it is stated that "tin under a pressure of about twenty tons to a circular inch, will *run* according to the law of fluids."

* See Third Letter on Glaciers. Travels, Appendix, p. 407.

† See Travels, p. 181.

the glacier of Bossons moves slower than its middle portion; there is therefore a manifest longitudinal compression arising from the friction of the bed*.

3. The next stage is that of the perfect bruise or veined structure, best seen in the most united and least fissured parts of glaciers with rocky sides and moving over a moderate slope. Whatever increases lateral compression (without however necessitating dislocation), such as the union of two or more glaciers in one, tends to develope the structure more perfectly†. Such cases are well seen on several parts of the Mer de Glace, and of the glacier of Miage‡.

4. In very wide glaciers, moving with feeble velocities, the veined structure is slightly developed, except near the sides, simply because the *twist* being small the ice is hardly bruised. Nor can we wonder to find the structure at the distance of many hundred yards from the sides of a vast slow-moving glacier of this description, if developed at all, to be complex and irregular, exhibiting twists such as I have figured in my Travels, p. 164, and which are peculiarly conspicuous in the magnificent glacier of Aletsch. This circumstance finds a precise analogue in the case of a great river, such as the Rhine, or indeed in any river moving with a very slight inclination; the excess of velocity of the central above the lateral parts, not very great at any rate, is distributed over such a space that the slightest casual disturbance of the current, from an irregularity in the bottom or sinuosities of the course, produces local differences of velocity, occasioning ripples and eddies in various parts of the breadth. If these ripples and eddies, in other words, differential motions of adjacent particles, could be visibly represented by using differently coloured fluids, they would undoubtedly afford sections exhibiting undulations and contortions exactly like those which the ice presents in the cases mentioned above. We claim therefore the apparent exception as a real proof of our general rule.

5. In the *névé* proper, no true veined structure is developed; *first*, because, whilst the mass is snowy, its powdery nature yields without admitting of a fracture or bruise; *secondly*, because the true *névé* has rarely any lateral compression worth mentioning, being widely spread and not contained between steep barriers; *thirdly*, because its motion is altogether very small; *lastly*, because its extreme dryness does not afford water enough to percolate its substance and there to be frozen; when it does so, it ceases to be *névé*.

On these grounds I hope that the theory of the veined structure, so important to that of glaciers, may be considered as explaining a number of intimately connected phenomena.

* The internal rents in the lava of Zafarana referred to in § 2 of this paper, and figured in Plate IV. fig. 8, present a perfect analogy with those of the glacier of Bossons, and appear to be due to the same cause.

† Third Letter. Travels, Appendix, p. 407.

‡ See the figures of the structure of the glacier of Miage. Travels, p. 197.

"The glacier struggles between a condition of fluidity and rigidity*." "A glacier is not a mass of solid ice, but a compound of ice and water more or less yielding, according to its state of wetness or infiltration†." "The pressure communicated from one portion to the other, will not be the whole pressure of a vertical column of the material equal in height to the difference of level of the parts of the fluid considered; the consistency or mutual support of the parts opposes a certain resistance to the pressure, and prevents its indefinite transmission. * * A glacier is not coherent ice, but a granular compound of ice and water‡." "When the semifluid ice inclines to solidity during a frost, the motion is checked; if its fluidity is increased by a thaw, the motion is instantly accelerated. * * It is greater in hot weather than in cold, because the sun's heat affords water to saturate the crevasses§." Such were the terms in which within a few months after suggesting the viscous theory I expressed my opinion of the influence of the compound structure of the glacier, a mass composed, not of ice alone, but of ice including water in its countless capillaries never frozen ‖ even in winter. The quality of plasticity or viscosity resulting from the union of a nearly perfect fluid with an imperfect solid is seen in very numerous and familiar instances, as for instance in sand, which is itself devoid of any tenacity until its interstices have been saturated with just so much water as to cause it to flow; or in the still more familiar instance of water-ice prepared for the table, in which the varying proportion of the solid and fluid ingredient gives to it every shade of consistency, from a brittle solid to a liquor including suspended solid grains. The prodigious effect of capillary infiltration in determining the motion of even the most solid and ponderous bodies, breaking up their parts, and giving to the motion of the whole a more or less river-like character, is seen in the frequent case of land-slips, as for instance that of Goldau. And scarcely less instructive are the numerous examples, cited in the first section of this paper, of huge masses of almost cold and brittle lavas being pressed on with a uniform and graduated motion, by the almost unimpeded hydrostatical communication of pressure from the yet active fluid which circulates unseen in their pores. With this analogy before me, I replied in 1844 in the following terms to the question, "How far a glacier is to be regarded as a plastic mass?" "Were a glacier composed of a solid crystalline cake of ice, fitted or moulded to the mountain bed which it occupies like a lake tranquilly frozen, it would seem impossible to admit such a flexibility or yielding of parts as should permit any comparison to a fluid or semi-fluid body transmitting pressure horizontally, and whose parts might change their mutual position so that one part should be pushed out whilst another remained behind. But we know in point of fact, that a glacier is a body very differently constituted. It is clearly proved by the experiments of Agassiz and others, that the glacier is not a mass of ice, but of ice and water; the latter percolating freely through the crevices

* Third Letter on Glaciers, August 1842. Appendix to Travels, p. 407.

† Travels, 1st edit., 1843, p. 175.

‡ Travels, p. 367., edit. 1843. § Ibid. p. 372. ‖ Ibid. p. 361, 372.

of the former to all depths of the glacier; and as it is matter of ocular demonstration, that these crevices, though very minute, communicate freely with one another to great distances, the water with which they are filled communicates force also to great distances, and exercises a tremendous hydrostatic pressure to move onwards in the direction in which gravity urges it, the vast porous crackling mass of seemingly rigid ice in which it is, as it were, bound up*."

Now the water in the crevices does not constitute the glacier, but only the principal vehicle of the force which acts on it, and the slow irresistible energy with which the icy mass moves onwards from hour to hour with a continuous march, bespeaks of itself the presence of a fluid pressure. But if the ice were not in some degree ductile or plastic, this pressure could never produce any, the least, forward motion of the mass. The pressure in the capillaries of the glacier can only tend to separate one particle from another, and thus produce tensions and compressions, *within the body of the glacier itself*, which yields, owing to its slightly ductile nature, in the direction of least resistance, retaining its continuity, or recovering it by re-attachment after its parts have suffered a bruise, according to the violence of the action to which it has been exposed.

The action of warm weather in accelerating the movement of the glacier is plainly due to the abundance of the water saturating its pores; but this may act in two ways; first, by rendering the frame-work of ice less brittle when it is in the very act of dissolving by the circulation of water in a perfectly fluid state through its pores†, and secondly, and more particularly, from the hydrostatic effect of *gorging* a porous mass with fluid. When an incipient frost dries even momentarily the surface of the glacier, the vast porous mass begins to *drain*. This is a very slow process, owing to the resistance to the passage of a fluid through very long and complicated canals. Were it not so, glaciers would be entirely dry after sunset and in winter, which is not the case. The hydrostatic pressure within the whole glacier is however sensibly diminished by the process of drainage; this is evident from watching the level of water in a vertical hole of any depth made within the solid ice of the glacier. After much rain or heat this level is always higher than after dry cold. In the former case the glacier may be said to be gorged, the supply of water from the surface exceeding the power of the drainage to carry it off. The circulating vessels are therefore overcharged. In the latter case the superficial supply is stopped, the drainage goes on slowly though uninterruptedly, and the level of the water in the vertical shaft slowly descends, indicating the diminution of internal pressure. If it were not for the capillarity of the ducts, it is plain that no effective hydrostatic pressure would

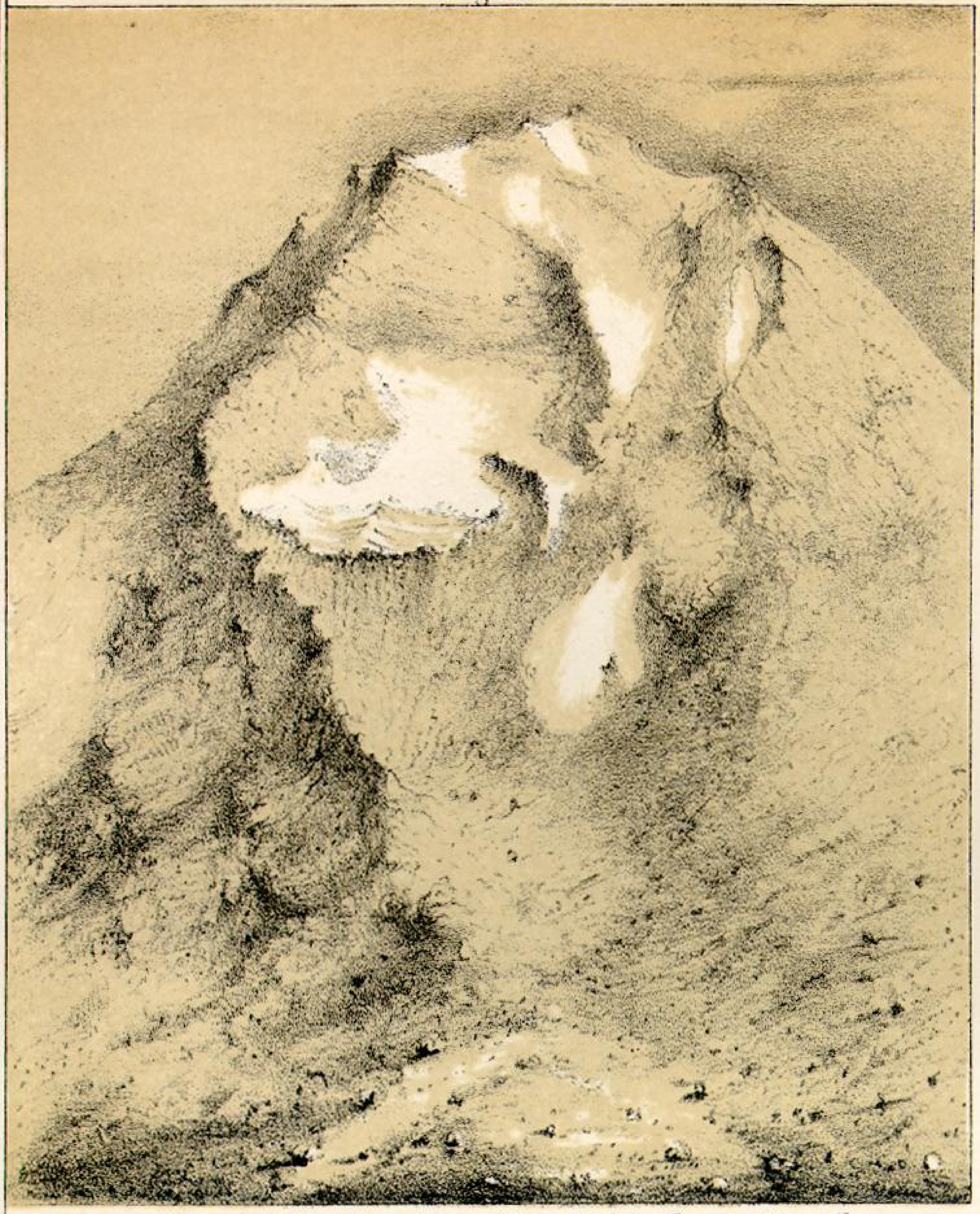
* Sixth Letter. Appendix to Travels, 2nd edit., p. 428.

† This I think is undeniable, from the appearance of the collapsed crevasses above referred to, notwithstanding the difficulty of imagining any variation in the sensible heat of water circulating in ice. It is not the only fact in the glacier theory which seems to require some modification of the commonly received laws of latent heat at the very limit of congelation and liquefaction.

be developed at all; the flow being equal to the supply, no part of the *vis viva* would be expended in producing internal pressures. With this concluding observation I commit the Plastic or Viscous Theory of Glaciers to the impartial judgment of those qualified to decide on its merits in explaining facts, and on the variety of difficult and complicated considerations which opposed and still oppose themselves to a complete development of it.

Edinburgh, Jan. 10, 1845.

Fig. 1. p.178.



View of the Glacier of the Schönhorn-Simplon.

Fig. 3. p.201.

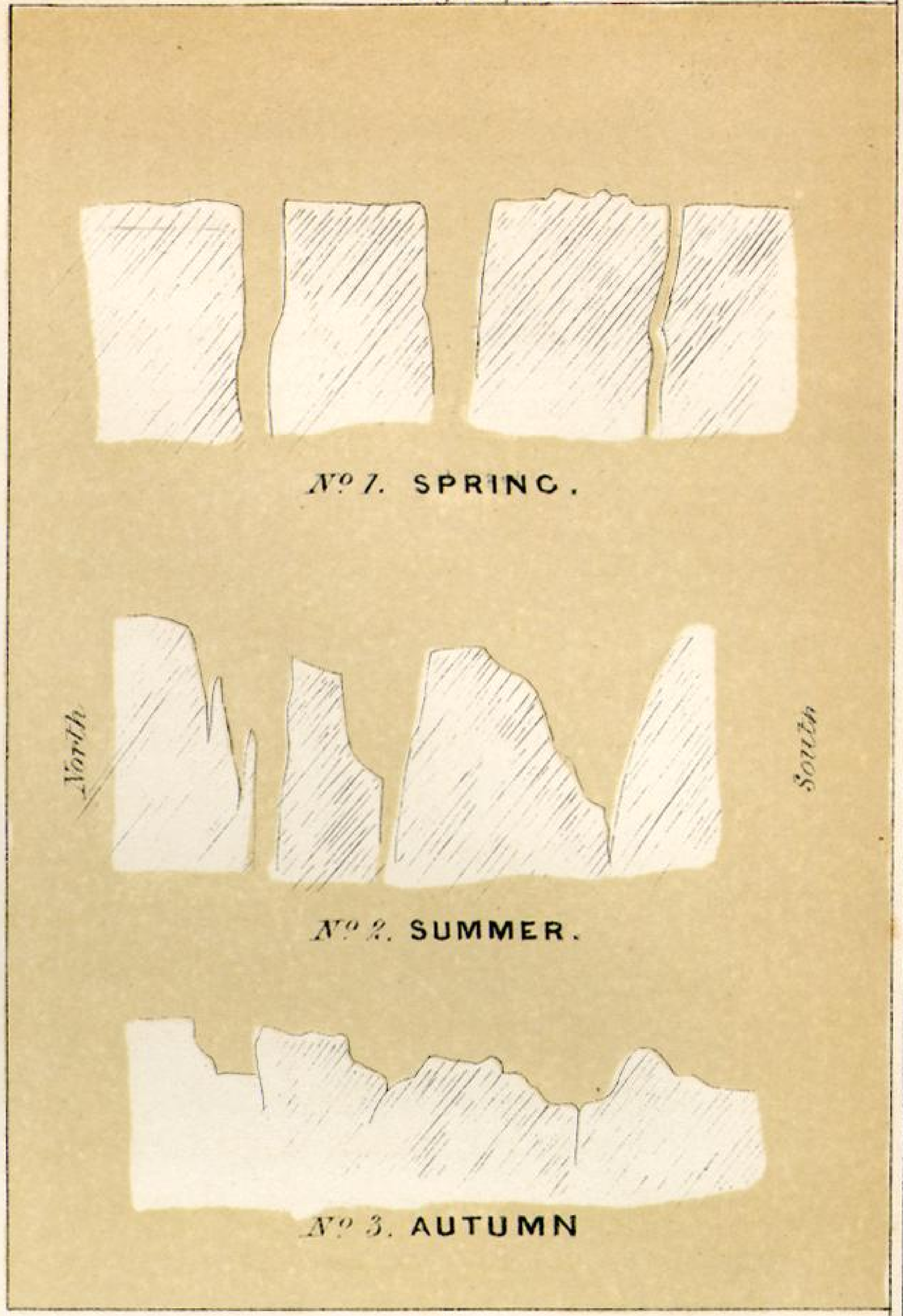
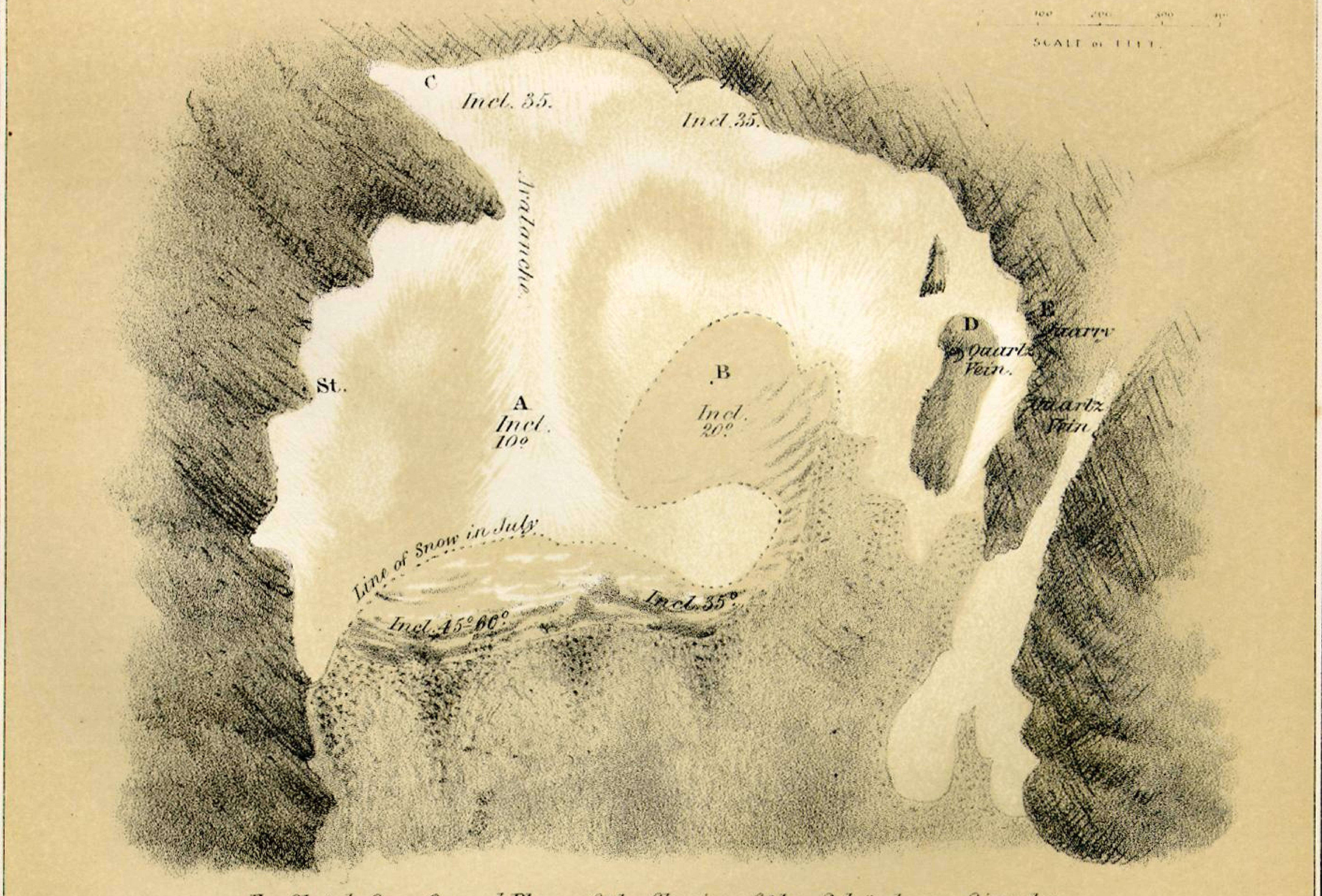


Fig. 2. p.178



Eye Sketch for a Ground Plan of the Glacier of the Schönhorn-Simplon.

FIG. 1. p. 186.

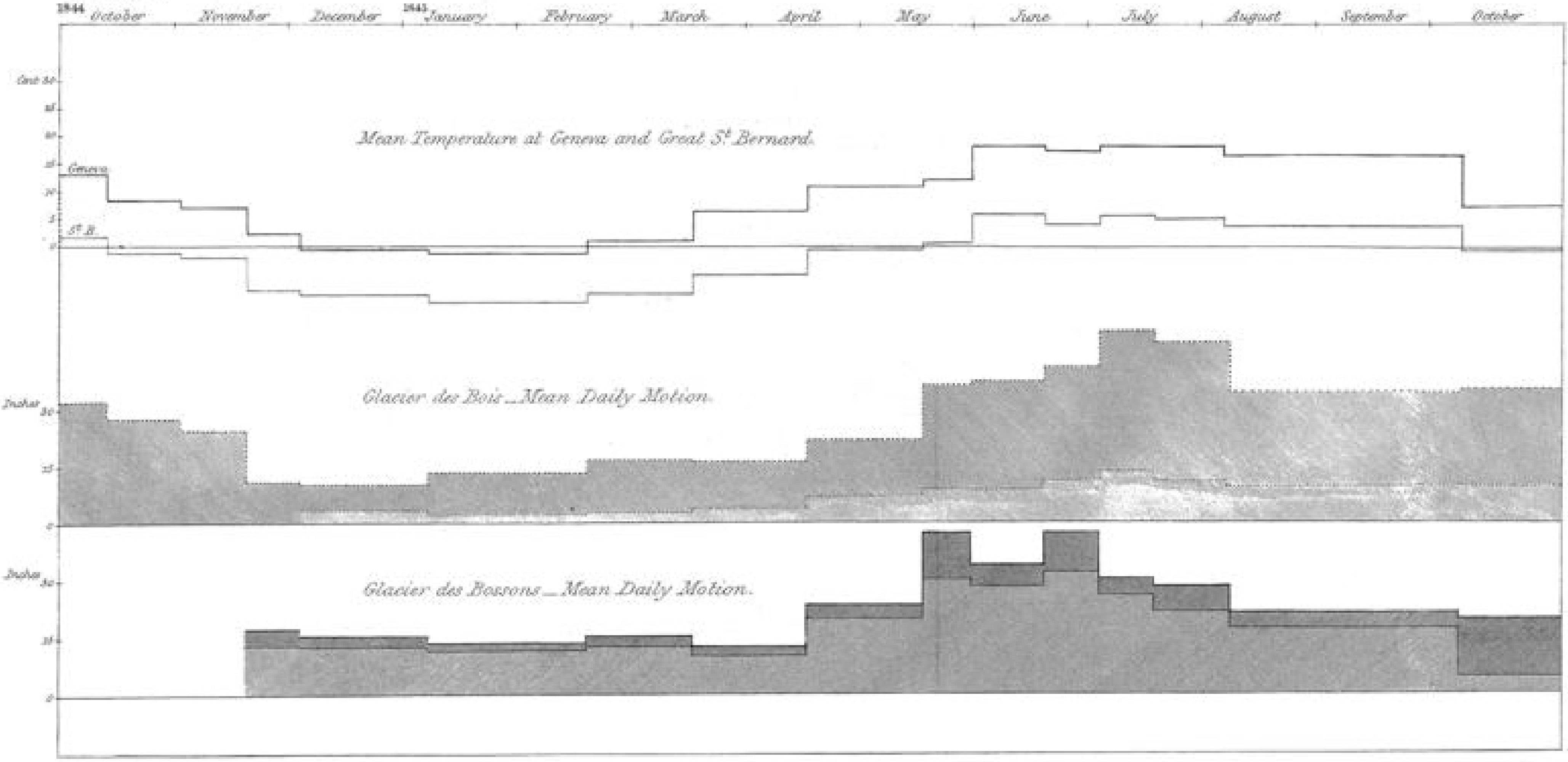
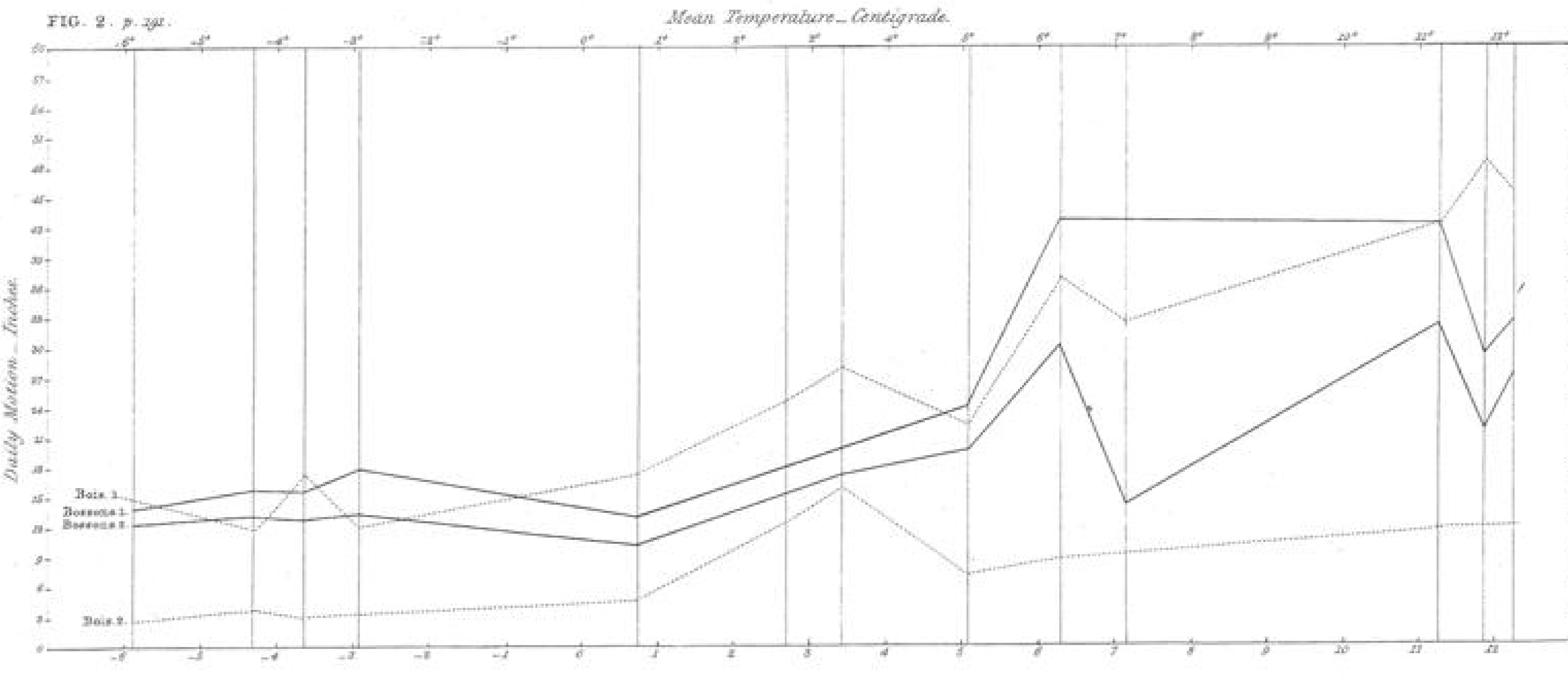


FIG. 2. p. 191.



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