ing, carding, roving, and spinning cotton, and other fibrous substances."—16th May 1842.

22. To Peter Kagenbusch of Wetter on Rhur, in Westphalia, in the kingdom of Prussia, dyer, now residing in the parish of Whitby, in the county of York, in England, "an improvement in the dyeing of wool, woollen cloths, cotton, silks, and other fabrics and materials."—17th May 1842.

23. To Joseph Clisild Daniell of Tiverton Mills, near Bath, "improvements in making and preparing food for cattle."—25th May 1842.

24. To Robert Logan of Blackheath, in the county of Kent, Esquire, "improvements in obtaining and preparing the fibres and other products of the cocoa-nut and its husk."—28th May 1842.

25. To Thomas Henry Russell of Wednesbury, in the county of Stafford, iron tube manufacturer, and Cornelius Whitehouse of the same place, "improvements in the manufacture of welded iron tubing."—28th May 1842.

26. To Thomas Middleton of Loman Street, in the borough of Southwark, and county of Surrey, engineer, being a communication from abroad, "an improved method of preparing vegetable gelatine, or size for paper, and also an improved mode of applying the same in the manufacture of paper."—6th June 1842.

27. To John Railton of Blackburn, in the county palatine of Lancaster, machine-maker, "certain improvements in machinery, or apparatus for weaving."—6th June 1842.

28. To Thomas Hedley of the town and borough of Newcastle-upon-Tyne, gentleman, and Cuthbert Rodham of Gateshead, in the county of Durham, millwright, "an improved apparatus for purifying the smoke gases and other noxious vapours arising from certain fires, stoves, and furnaces."—7th June 1842.

29. To John Burnell the younger, of High Street Whitechapel, in the county of Middlesex, manufacturer, "improvements in the manufacture of leaves or sheets of horn, commonly called lantern leaves, and in the construction of horn lanterns."—8th June 1842.

30. To Otto Rotton of Gracechurch Street, in the city of London, Doctor of Medicine, being a communication from abroad, "certain improvements in machinery, or apparatus for spinning cotton, wool, and other fibrous substances."—14th June 1842.

31. To John Bould of Oxenden, in the parish of Halifax, in the county of York, cotton-spinner, "an improvement or improvements in condensing steam-engines."—23d June 1842.

32. To John Cox of Gorgie Mills, Edinburgh, tanner and glue manufacturer, "certain improved processes of tanning."—23d June 1842.

THE

EDINBURGH NEW PHILOSOPHICAL JOURNAL.

The Glacial Theory and its recent Progress. By Louis Agassiz, Doctor of Philosophy and Medicine, LL.D. of Edinburgh and Dublin, Knight of the Order of the Red Eagle of Prussia, Professor of Natural History in the Academy of Neuchâtel, &c. (Communicated by the Author.)*

There are few branches of natural science which have made such rapid progress as the subject of glaciers and the phenomena connected with them. When, about six years ago, I invoked, for the first time, the aid of a vast sheet of ice, covering the whole northern hemisphere, in order to explain, on the one hand, the transport of the erratic blocks distributed over the southern flank of the Jura, as well as the surface of the north of Germany, of England, of Sweden, and of Finland; and, on the other, the formation of rounded surfaces, which are often as smooth and as highly polished as the finest marble, and on which are traced fine parallel lines, resembling those produced by the burin of the engraver, I scarcely ventured to hope that any impartial minds would take the theory into consideration, so hostile was it to the ideas generally entertained up to that time. The researches on glaciers and their former extension, previously carried on by MM. Venetz and Charpentier, were scarcely known to naturalists; and those who were acquainted

* The publication of this valuable Memoir has been somewhat delayed in consequence of unforeseen circumstances.—Edit.

with them, considered the views promulgated as inadmissible. The glacial theory, *a fortiori*, was rejected as completely Utopian; and, assuredly, there could have been no reason to complain of this, if it had been merely a speculation elaborated in the seclusion of a study, and apart from nature. In the midst of the varied productions which bear testimony to the activity of our epoch in all the domains of science, it is in some degree a matter of necessity for a scientific man to engage in such researches only as rest on a real foundation. Now, the glacial theory, however daring it might seem, was supported by numerous facts, which, though but little known, were not the less positive. There lay the germ of its future career; and attention was speedily directed to these facts, as strange as they were of general occurrence. Observations were made, researches were carried on, the data of the system were verified, and discussions ensued on all sides. In the midst of this contest, the glacial theory has extended itself; and though it has not obtained the suffrages of all, and still involves many contradictions, yet the various facts belonging to it have become the subject of serious study, and, at the present day, there is no naturalist who does not recognise their importance. In this consists the advantage derived from it by science; an advantage of immense consequence, inasmuch as it promises to us prospectively the solution of one of the most beautiful and extensive problems in the history of the earth. It is no longer a mere theory, which is the subject of discussion; the object of research is the connection of a whole series of phenomena, apparently very different, but whose relations are evident to all observers: these are the erratic blocks, the mounds of loose materials, the ancient moraines, the polished and striated surfaces, the *surfaces moutonnées*, the furrowings of rocks in a constant direction, and the whole group of remarkable facts which an illustrious geologist has designated by the characteristic name of the *erratic phenomenon*.

Since the domain of observation has been fairly entered, the investigation has advanced with gigantic strides. The beauty of the subject, the vast field which it embraces, the exciting questions belonging to it, have awakened on all hands zeal, interest, curiosity, and ambition. There is now not an academy, not a scientific society, in which the erratic phenomenon has not been discussed and supported by new facts; and such has been the activity displayed by the *savans* of every country, that the most succinct abstract of the works and memoirs on the subject which have appeared within the last two years, would greatly exceed the limits of an article like the present. M. de Charpentier, in his *Essai sur les Glaciers et le Terrain Erratique*, has described in detail the traces of ancient glaciers in the great valley of the Rhone and its lateral valleys, and also at a multitude of other points in Switzerland; M. Studer has observed them on the southern side of the Alps; and Mr Martins in the Grisons. The French geologists assembled at Grenoble in 1840 studied them in the Alps of Dauphiny, and made them the subject of their discussions at the meeting held at Lyons in 1841. The polished rocks, in particular, seem to be very distinct on Mount Cenis, where they have been detected by Mr Trevelyan and by Captain Le Blanc. MM. Renoir, Hogard, and Le Blanc have continued to observe the erratic phenomenon in the Vosges; MM. Max Braun and Du Rocher have noticed it in the Pyrenees; and I myself have done so in the Black Forest. The Swiss and French Jura has in this respect been made the object of continued study by MM. Gressly, Guyot, and Desor, who have proved that the erratic blocks of the Alps extend far beyond the limits assigned them by MM. de Buch and Charpentier; and, lastly, I have discovered erratic blocks, accompanied by polished and scratched surfaces, in a host of localities in the Alps, where they had not previously been known to exist.

The great phenomena of the north, although attributed to other causes, do not the less belong to the same subject; and, since the investigations of MM. Alexander Brongniart and Sefström, they have been made the object of continued researches by MM. Buckland, Murchison, Eichwald, Durocher, Robert, Hausmann, Murchison, De Verneuil, and Kaiserling. Finally, the American geologists, also, have very recently noticed a vast network of polished rocks and erratic blocks in the United States.

But it is more particularly in Great Britain that the most unexpected discoveries have been made. Who could have sup-

posed that in these islands, equally remote from the glaciers of the Alps and the ice of the north, traces of the action of ice should have been found! And, nevertheless, all the phenomena which indicate the former existence of glaciers are there just as evident, and just as well preserved, as in the neighbourhood of the glaciers of the present day. England likewise, —thanks to the activity and the zeal of her *savans*—already possesses quite a literature on the subject of glaciers; and it would be necessary for me to cite the names of most of the geologists of that country, were I to mention all the individuals there who have occupied themselves with this question.

The purely theoretical part of the erratic phenomenon has also attracted much attention; and the discussions to which it has given rise in many places, and particularly in the Geological Society of France, have contributed, on their part, to render the study still more interesting, by connecting it with the great problems of the cosmic system.

Passing from the geological domain, attention ought naturally to be directed to the glaciers of the present day, which are, in fact, the touchstone of the whole system; for, in order to be entitled to call in their intervention to explain the erratic phenomenon, it is necessary to know, even to the most minute details, their actual condition, and their action on neighbouring bodies. Now, although the researches of naturalists since the time of Scheuchzer and Saussure have made us acquainted with the general laws which preside over their formation and their metamorphoses, there is, nevertheless, a multitude of details regarding their internal structure and their modifications, as connected with the different seasons, the import of which is still unknown to us; and there is even a diversity of opinion among naturalists on some of the capital points. Since the publication of my *Etudes sur les Glaciers*, I have made many additional expeditions to the interior of glaciers with the view of answering certain objections made to my theory by natural philosophers. I have also resided a second time on the *Mer de Glace* of the Lauter-Aar and of the Finster-Aar, in order to study the structure of the ice at all heights and in all conditions of the atmosphere, and I hope to be able to continue these same researches still longer,—thanks to the munificence of his Majesty the King of Prussia, who has deigned to grant me his support in this undertaking.

A study of equally great importance, with reference to our theory, is that of the history of glaciers. The chronicles and the registers of the communes situated in the high valleys of the Alps contain scarcely any thing else but the recital of that long struggle between man and the rivers of ice; a history sad and melancholy when the enemy gains possession of the tract, but joyous and animated when he beats a retreat under the influence of a series of warm summers. The same influences and the same reactions must exist in the north of Europe, and the history of the formerly flourishing colonies of Greenland, when it becomes better known, will doubtless tell us of some of those extensive changes in the covering of ice at the North Pole.

In the present article I shall confine myself to some details regarding the observations I have made since the publication of my work; and, as these observations are of a very varied nature, some belonging more peculiarly to the domain of geology, and others to that of natural philosophy or of meteorology, I shall divide the subject into two sections, and intend, in the first, to treat of the erratic phenomenon properly so called, such as I have observed it in Britain. In the second, I shall communicate some of the experiments which I made on glaciers last summer, during my sojourn upon them, and in the course of my expeditions; and the whole will be terminated by a short narrative of our residence on the glacier.

I. The Erratic Phenomenon.

I have already said that the most striking discoveries, in relation to the erratic phenomenon, had been made in Britain. The most important facts which I have observed may be arranged in three categories:—

1. The phenomena proper to the interior of valleys.
2. The dispersion of erratic blocks in plains, at great distances from their origin.
3. Parallel terraces.

1. *The phenomena peculiar to valleys* are almost identical at all places where they have been observed. When we study

the arrangement of erratic blocks of certain valleys in Scotland, we feel inclined to imagine ourselves in a valley of the Swiss Alps. I shall never forget the impression I experienced at the sight of the terraced mounds of blocks which occur at the mouth of the valley of Loch Treig, where it joins Glen Spean; it seemed to me as if I were looking at the numerous moraines of the neighbourhood of Tines, in the valley of Chamounix. These mounds or ramparts abut against the walls of the valleys, frequently forming at the mouths of the valleys a series of concentric belts, which occur precisely at those places where, supposing that the valley had at one period been occupied by a glacier, it ought to have terminated by the terminal moraines pushing against one another. Similar mounds are observed at the mouth of nearly all the valleys of mountainous countries. The most remarkable in the British islands are, in Scotland, those of the banks of Loch Awe and of Loch Etive, especially in the vicinity of Bunawferry; in England, those of the environs of Penrith and Kendal; and in Ireland, those which traverse the road that skirts the base of Cuilcagh to the west of Florence Court. The latter are more distinct than any that I have seen in the United Kingdom. The nature of the blocks composing these moraines, proves that they have not come from a great distance; but that they have been detached from the upper part of the valley, and transported by some cause to its extremity. It is among these blocks, sometimes of very considerable size, that we find the most angular. Now, if we consider the arrangement of the valleys, which proceed in all directions from the most elevated chains, and all of which present the phenomena of erratic blocks, and of more or less continuous moraines, we cannot for a moment doubt, that the cause of this transport has extended its effects by radiating from the interior of the elevated points of the district towards the plains. This is a fact of capital importance, for it proves that the phenomenon of transportation is to a certain extent a local phenomenon, inasmuch as it is connected with the neighbouring chains of mountains. Each great group of mountains in Britain has thus its system of erratic blocks limited to the extremities of its valleys. It is thus that Ben Lomond on the one

hand, and Ben Nevis on the other, have their system of blocks independent of that of Ben Wyvis; Schihallien and the Grampians have equally theirs, as also the Pentland Hills, the Cheviots between Scotland and England, and the mountains of Cumberland and Westmoreland; lastly, the mountains which rise above Belfast, those of the county of Wicklow, and Cuilcagh, also seem to me to form so many separate groups, as regards the dispersion of their erratic blocks. But these relations of the blocks to the chains of mountains are only one of the peculiarities of their arrangement; it is indeed that very one which has been least insisted on, and with which the defenders of the theory of currents have the least occupied themselves; and yet they ought above everything to have endeavoured to explain it, because it includes facts the most contrary to their theory. How is it really possible to attribute to an eruption of the ocean, or to the effects of a continual *soulèvement*, the dispersion of different groups of erratic blocks arranged like a fan around each particular system of mountains? How, moreover, is it possible to conceive the existence of so many deep lakes, by whose beds, however, all these currents must nevertheless have passed, in order to perch the erratic blocks on the flanks of the mountains, rather than accumulate them in the bottom of the valleys?

A circumstance which further adds to the importance of these scattered blocks and continuous mounds, is, that the valleys in which they are met with have generally their walls more or less worn, rounded, smoothed, polished, and striated. Now, this particular appearance is evidently to be attributed to the same cause which transported the blocks; for these two series of facts are everywhere intimately connected together.

It was in England and in Sweden that the first polished surfaces were observed, and these were everywhere attributed, until recently, to the action of great currents, without any regard being paid to the improbability of a current, or rather currents, spouting like springs from the top of all the valleys, and being sufficiently powerful to convey from their place of origin blocks sometimes of immense dimensions. It can easily be imagined, that, at a period when almost all geological pheno-

mena were attributed to the action of water, no endeavour was made to search for another cause for the transport of erratic blocks. But if a comparison had been instituted between the polished surfaces and the effects produced by currents, very remarkable differences between them would have been discovered. As I have said elsewhere, rocks polished by glaciers of the present day present surfaces gently rounded, smooth, and continuous over large spaces, sometimes even perfectly flat, and passing uniformly over the most resisting portions of rocks as over the softest, without forming sinuosities or edges. They are, moreover, furrowed, in the direction of the movement of the glacier, by furrows more or less deep and rectilinear, and scratched by fine striæ, perfectly rectilinear, and evidently parallel to one another and to the furrows; and, when the latter offer deviations from the general direction of the valleys, it is in consequence of circumstances which it is easy to appreciate. Such are likewise the polished surfaces remarked at the bottom and on the flanks of the valleys which are encompassed by erratic blocks and moraines, even when they are no longer occupied by glaciers. But such are not the appearances exhibited by rocks worn by water; although smooth they are never polished, and their undulated and sinuous surfaces present hollows or irregular excavations wherever the nature of the rock favoured erosions; no portion of the surfaces worn by currents of water has exhibited to me those long rectilinear striæ so characteristic of the polishing of glaciers. These differences between the abrasion occasioned by glaciers and that caused by water, are very well explained by the difference presented by a current of water, which, while it bounds along, follows all the sinuosities of its bed, and a rigid mass of ice which advances slowly on account of its consistence. The conformity which I have already pointed out between the aspect of polished valleys whose flanks are charged with erratic blocks together with continuous mounds, and whose mouths are closed by concentric barriers of blocks, and the aspect of the valleys at present occupied by glaciers flanked by their lateral and terminal, ancient and recent moraines, and whose bottoms are polished, striated, and furrowed in the direction of the movement of the glacier; this conformity, I say, is the principal argument that has caused me to attribute to the existence of glaciers which no longer remain, the phenomena similar to those produced by the glaciers of the present day, and which we meet with in so many localities far distant from glaciers. The granitic and porphyritic rocks of many valleys in Scotland exhibit polishings equally brilliant with those at present observed on the slaty serpentines of the flanks of the glaciers of Monte Rosa. The most remarkable of these polishings that I have seen in Scotland are those of the banks of Loch Leven near Ballahulish, those of Glen Spean opposite Loch Treig, those of Bunaw Ferry, those of Schihallien, pointed out by Dr Buckland, and those of the neighbourhood of Edinburgh, where the late Sir James Hall was the first to observe them. In England, I have seen very fine instances between Shap and Kendal, and near Ambleside; and in Ireland, near Donaghadee and near Virginia. I shall not here enumerate the numerous localities where the polishing of the rock has disappeared entirely or in part, but where the form of the surface still attests its former existence, but will limit myself to the notice of a very curious fact of this kind which occurs on the left bank of Loch Treig. We have there a small hill of gneiss, of a rounded form, whose surface is no longer flat, but is traversed by veins of quartz, having perfectly smooth and striated surfaces. The contours of these veins are exactly conformable to those of the eminence; they are cut according to the same forms, but they are raised two or three inches above the surrounding rock. Precisely the same phenomenon is to be seen near the Hospice of the Grimsel, where veins of quartz, having a polished surface, traverse *roches moutonnées* of gneiss, whose surface has been rendered rough by the action of the atmosphere. The projection of two or three inches of the veins of quartz on the hill on the banks of Loch Treig, evidently indicates the amount by which the neighbouring surface has been lowered by the decomposition of the gneiss, since the time when the quartz was polished, and the whole eminence rounded.

Limestone rocks are equally polished in a multitude of localities; but the most remarkable instances are in Lancashire,

and in the vicinity of Florence Court in Ireland. I have already said that the mounds of blocks, and the polished rocks, are every where found to be intimately connected together; but I do not mean to say that polished rocks are not met with where there are no mounds, and that mounds are not met with where there are no polished rocks; for it may happen that the rocks on which the ancient glaciers moved were, in certain localities, very little calculated to retain the polish, and that they may have lost their original lustre, while the moraines composed of rocks of the elevated portions of the valleys still exist on their flanks. It is thus that we see very beautiful examples of mounds and ancient moraines in Glenary, above Inverary Castle, although there are no polished rocks in the immediate vicinity; and it is thus that the magnificent moraines of Cuilcagh, near Florence Court, are not accompanied by polished rocks, in consequence of the facility with which the solid rocks of that locality are decomposed. On the other hand, we frequently see beautifully polished rocks without moraines; and this is especially the case with the very abrupt walls of narrow valleys, where the blocks have fallen down at the period of the retreat of the ice, and have reached the bottom of the valleys without resting on their flanks. In such instances, instead of presenting the form of moraines, the blocks are scattered irregularly over the surface. We have an example of this on the northern walls of Loch Etive.

Dr Buckland and Mr Lyell, receiving the interpretation that I have given of the facts quoted above, have also observed a multitude of localities which, joined to those that I have myself visited, form a very extensive net-work of traces of ancient glaciers, stretching over the largest portion of Scotland, a large part of Ireland, and the whole north of England.

II. *Dispersion of erratic blocks in plains.*—The phenomenon of erratic blocks and polished rocks is not limited to the chief groups of mountains, but is seen extending over the whole surface of the country where it presents itself; with this difference, that in the lower regions it assumes peculiar

characters, different from those which I have described as belonging to mountains and their valleys.

Just as the erratic phenomenon is localized in the vicinity and in the interior of mountains, so does it exhibit uniform characters in the low country and in flat regions, covering vast tracts whose limits cannot with precision be referred to determinate centres. Blocks are seen extending from one mountain-chain to another, across considerable depressions of the surface; the accumulations of blocks transported from one place to another are no longer arranged in linear continuous series as in the valleys, where they form mounds or ramparts, which are moraines properly so called, but they are dispersed irregularly over the surface; the nature of the rocks mixed together in these accumulations no longer indicates an origin so limited as that of those moraines even which are at the mouths of the valleys. The dispersion of these blocks in different countries has not hitherto been described with sufficient care, and more particularly the erratic angular blocks with a rough surface have not been sufficiently distinguished from those that are rounded, polished, and scratched. There are, however, very important differences in this respect. In Switzerland, for example, we nowhere meet with *large* blocks, whether angular or round, whose surface is rubbed, polished, and scratched with rectilinear striæ, at great distances from their origin. Whatever may have been the cause of the transport of the erratic blocks of the Alps and the Jura, it always happens that the great mass of the large blocks have arrived there with rough surfaces and well marked angles, and that the pebbles of smaller dimensions alone are worn, rounded, polished, and scratched with rectilinear striæ. We may easily convince ourselves of this fact by walking along any part of the Jura chain. Another peculiarity worthy of attention is, that with us the large angular blocks generally repose on the more or less considerable masses of rounded and polished pebbles, and that these latter often pass into a fine sand or a clayey paste, which covers directly the polished surfaces of the solid rocks wherever the pluvial water, the melting snow, and the torrents resulting from it, have not caused

them to disappear. This arrangement is very well seen in the environs of Neuchâtel.

The state of matters is by no means the same in Britain, and more particularly in Scotland. There the erratic blocks of all dimensions are, in certain circumstances, rounded, perfectly smooth and polished, and even scratched with rectilinear striæ, like the polished solid rocks—a feature only observed in the smaller pebbles in Switzerland. It is not to be understood that there are no large angular blocks in England and in Scotland; but there is this distinction to be made, that these blocks are generally not far distant from their natural position *in situ*, or that they are in small number compared with those which have evidently been acted on by a prolonged mechanical operation. But this is not all; far from being found lying at the surface of the ground, the large blocks are for the most part heaped up in a confused manner along with the smaller ones of all degrees of size, from the dimension of the smallest pebbles to the colossal volume of the largest erratic blocks, in a deposit of clay unequally distributed over all the low portions of the country. This deposit of clay, which is of very unequal thickness, and exhibits no trace of stratification, is what is termed *till* in Scotland. There is no locality in which I have been able to study the till more completely than at Glasgow, where the numerous works carried on in 1840 for the embellishment of the town had exposed it at many points; but everywhere it presents the same characters; the rounded, polished, and scratched blocks of very various dimensions, are every where indiscriminately mixed together in a marly or clayey paste. It is evident that it was with this mass, and in this mass, that the rounded and polished blocks have been transported during the whole journey which they have performed together, while the angular blocks have certainly not been rubbed in this manner. Mr T. Edington has, to the advantage of geologists, brought together, in his park at Glasgow, a magnificent collection of these polished and scratched blocks from the neighbourhood of the town.

Differences of this description in the facts observed at different localities, are an additional difficulty for all those who

endeavour to explain them by means of currents. How, indeed, can it be now seriously pretended that a current can convey blocks in such a manner as to rub, round, and scratch one set of them, without their being heaped up according to their weight, and without their being covered by regular beds of finer materials, while the others remained angular, and retained their unequal and rough surfaces? These differences are very favourable to the glacier theory, which explains them in a manner that is quite natural.

Let us return to the glaciers of the present day, and we shall find in some of the phenomena presented by them the greatest analogy to the arrangement of erratic blocks, as I have just described it. When a glacier moves, it wears and rubs the bed on which it reposes; scratches the smoothed walls; triturates the detached masses which are interposed between the ice and the rock, and reduces them to sand or to an argillaceous paste; rounds the blocks, which are of an angular form, and which offer resistance to the pressure; and polishes completely those which have broad sides. At the surface of the glacier, matters proceed in quite a different manner. The fragments of rock which are detached from the neighbouring walls, and which fall there, rest upon the ice, and are at most thrown out to its edges. They thus advance with the glacier without being displaced, or at least without being rubbed against one another, excepting those which have become interposed between the rock and the ice, and they arrive at the extremity of the glacier with their angles entire, their edges sharp, and their surfaces irregular. Let us suppose, now, that, in consequence of certain circumstances, one of these immense glaciers charged with debris of rocks, such as the lower glacier of the Aar, or the glacier of Zermatt, should be melted, and it would result that all the angular blocks at the surface of the glacier would repose on the irregular mass of rounded debris which at present lies under the ice. Some of these blocks would likewise be carried to a great distance on rafts of ice, if the melting were sufficiently rapid to cause currents capable of floating large masses of ice charged with blocks. Now suppose, on the contrary, that a glacier or a large sheet of ice, like that which extends over the Col de St Théodule, were

not commanded by numerous mountain peaks, then few or no angular blocks would fall on its surface, but the rounded blocks underneath would not the less be present. If we imagine that, in such a case, particular circumstances should also occur to cause the melting of the ice, there would then be found at the bottom an irregular deposit of rounded blocks, imbedded in the more comminuted materials, along with a few angular blocks above—in short, to the very letter, a sort of till. In this case, again, the melting of the ice would give rise to currents; and the more considerable these currents, the more they would contribute to operate farther on the materials already acted on by the glaciers, whether by conveying to a distance the lighter portions, and depositing them in regular stratification, or by penetrating them more or less, and giving them a false appearance of stratification. We actually observe something of a similar kind, on a small scale, in the oscillations that occur at the extremity of glaciers which sensibly advance and retreat; as, for example, under the extremity of the lower glacier of the Aar in the Grimselgrund; and, among the localities where glaciers no longer exist, I may cite the lower extremity of Loch Treig, and the neighbourhood of Muckairn, between Loch Awe and Loch Etive.

In order to explain the whole of the facts relative to the erratic phenomenon, in the limits within which they have hitherto been observed, it is sufficient to admit that the polar ice formerly extended as far at the North Pole as it now extends at the South. Thus, then, if the influence which has established the difference that exists at present between the extent of the ice at the two poles be a periodical influence, and if it describe one of those cycles of long revolution, which astronomers have been enabled to determine, we can not only conceive the possibility of a cold in our regions sufficiently intense to produce all the phenomena which I have described, but may even be able to determine its date and duration. I shall not reproduce here my general theory of the periodical refrigeration of our globe, for that would raise useless discussions in a field which the light of observation has not yet sufficiently illuminated; I shall only cite one fact, which tends

to make us suppose that there really existed in the North a covering of ice, whose southern limits in Europe, at a certain epoch, reached about 50° N. Lat. I allude to that belt of blocks observed by Russian geologists (see the letter from M. de Meyendorf to M. Elie de Beaumont*), which extends across the centre of Russia, by N. Novogorod towards Pinsk, and over the surface of Silesia. It seems to me much more natural to regard this limit as an *isopagetic* line (*une ligne isopagétique*), than as the southern limit of a current coming from the North, and charged with blocks; and this so much the more, because the phenomenon of the transport of the Scandinavian blocks extends not only into Russia and Germany, but reaches the eastern coasts of England. In attributing this effect to the action of a current, it would thus be also necessary to imagine a fan-shaped current; whereas a solid limit, during a certain time, of a covering of ice as extensive as that of the South Pole, obviates all the difficulties presented by such a phenomenon, such as the continuity and the regularity of the outlines, the uniform furrows of the polished surfaces of the North, the passage across the Baltic and the North Sea of the blocks which lie on the surface of Germany and of England, &c. In a second zone of blocks, more to the south than the first, and observed likewise in Russia, to the north of the White Sea, and of the lakes of Onega and Ladoga, we have a direct proof of the successive and slow retreat of the covering of ice, a second isopagetic line more remote than the first. If this covering of ice really existed, it must at last have retired beyond the northern limits of the British Islands, after having enveloped them partially or entirely; but as long as the northern ice had not retired to its present limit, the climate of Europe must have been colder than it now is, and even when the primitive ice had abandoned the plains, groups of glaciers must have remained in all mountainous

* Archiv für Wissenschaftliche Kunde von Russland; von Erman. Berlin.

† Isopagetic, that is to say, of equal ice; in some sense the *isotherme* of the limits of the northern covering of ice; but as the limits of this ancient ice do not coincide with the isothermal lines, I have been obliged to pro-

countries. Hence it appears natural, that during the retreat of this covering of ice, there must have been a period when the mountains of Scotland were the focus of numerous glaciers, which at first descended from their summits into the plains, but afterwards occupied only the interior valleys, before disappearing completely.

There would thus be two very distinct periods to be particularized in the epoch of the existence of ice in the north of Europe,—that during which the general covering enveloped the region, and that when glaciers existed only in the high valleys. The dispersion of erratic blocks over great spaces, across considerable depressions of surface, the formation of the till, the furrowing and uniform striation of the polished rocks of Sweden and of Finland, seem to me the chief phenomena which have been produced by the northern covering of the epoch of ice. The differences which exist as to the erratic phenomenon between the north and the centre of Europe, appear to me to be susceptible of easy explanation by the differences of latitude and of the configuration of the surface. In Britain, the ice, at the time of its greatest extension, seems to have covered completely great tracts of country, and consequently rendered the fall of blocks on its surface, if not impossible, at least extremely rare; so that the great mass of the blocks was necessarily buried under the ice, and was therefore subjected to all the effects of a gradual and long-continued trituration, just as is observed beneath the glaciers of the present day. Mountains of considerable elevation in Scotland—Schihallien, for example—have their summits as polished as their flanks; whereas in Switzerland there exists a limit, at about 9000 feet,* in the centre of the Alps, above which the summits are no longer polished, but where the rugged peaks present a very striking contrast to the lower surfaces, which are polished, or, at least, *moutonnés*.†

* All the measurements given in this paper are in *pieds de Roi*, or French feet; and the temperatures are all indicated in centigrade degrees, unless where other measurements or other degrees are specially mentioned.

† *Vide* the Comptes Rendus de l'Académie des Sciences, 1842; tome 14, p. 412.

In the exterior chains of the Alps, the polishing does not reach to a greater height than 6000 or 7000 feet. It cannot be doubted, that this limit, which is so well marked, indicates the level of the bed of ice at the epoch of its greatest thickness. The rugged peaks, which exceed that height, thus rose like islets in the midst of this sea of ice, and the blocks which were detached from them fell on the surface. Not being confined in narrow valleys, but the whole vast sea of ice being open to them, these blocks were not liable to be knocked against one another in their progress towards the lower districts, and it is thus that they could be transported as far as the Jura, with their surfaces rough and their angles prominent; whereas, the matters which were beneath the ice, were triturated, polished, rounded, and scratched. Now, if in Switzerland, the limit of the great mass of ice extended as high as 9000 feet in the Alps, and if it oscillated between 4000 and 5000 feet in the Jura which no longer presents glaciers, what is more natural than to admit, taking into account the geographical portion of the localities, that, in Scotland, the great proportion, if not the whole, of the surface, was entirely under ice during the whole duration of the glacial epoch. Hence the majority of the detached blocks of the Scotch mountains must have been transported under the ice, and consequently rubbed, rounded, polished, and scratched. I say the majority, for it is probable that some were detached when the ridges were free from ice, and when the valleys alone were occupied by glaciers; and these latter have necessarily remained more or less angular, and have retained their rough surfaces, just like the blocks of the moraines of the glaciers of the present day. Foreign blocks, whose origin is not British, and which were doubtless transported on the surface of the great sheet of ice, or on rafts during the period of its dissolution, ought to be angular, and, for the most part, are so in reality. In this way, the form of these blocks implies, in some degree, at first sight, their mode of transport. I am able to add, as a confirmation of what I have said as to the form of the erratic blocks of Scotland, that the blocks of the Jurassic rocks, which we meet with in the diluvium of the interior valleys of the Jura, are all rounded; a

proof that they have been transported under ice; and in fact this ought to be the case, because the polished rocks furnish us with the proof that the sheet of ice covered nearly all the summits of the Jura.

The melting and the retreat of the ice seem to me to have caused, at different times, according to the climatological circumstances, all those deluges, more or less extensive, of which records have been sent down by tradition and history. It is doubtless to these inundations that we must also attribute the dislocation of a large portion of the moraines, especially of those that, by their position, were not beyond the reach of the currents, which, by acting on the detritus at the bottom of the sheets of ice and of the glaciers, have given it, in many localities, a stratified appearance; so much so indeed, that we might be deceived as to the origin of these detrital matters, and attribute their rounded form to the effects of great currents, as has often been erroneously done. I do not believe that I deceive myself when I affirm, that whenever rounded blocks, lying in accumulations of gravel, stratified or unstratified, are scratched by long rectilinear striæ, their aspect is due to the action of the rubbing of glaciers against their beds; and that currents, in acting subsequently on these same matters and rolling them, could not but cause these characteristic marks to disappear by the friction. I therefore regard the rarity of scratched pebbles and blocks, in a deposit of stratified gravel, as a proof of a longer transport by water, and their total absence as a proof of an action due exclusively to currents; whereas, the complete absence of stratification in the accumulations of gravel and blocks uniformly rounded and scratched, seems to me to be the exclusive effect of glaciers. Lastly, these characters may be combined when such accumulations are the combined effect of the two causes, as may have been the case on maritime shores, where the glaciers of neighbouring mountains terminated at the coast. It must likewise not be forgotten, that sometimes small lakes are formed on the flanks of glaciers, in which the matters triturated by the glacier are deposited in regular beds, without being carried very far. It is of consequence to keep all these facts in view, when we study

the formations which geologists have termed *diluvium*, and whose various phenomena have hitherto been erroneously attributed to one single cause,—currents.

It appears to me probable, according to the facts which I have been able to combine in considering this question, that the organized beings of our epoch were created successively, after the commencement of the retreat of the ice. Wherever the surface of the ground made its appearance between the glaciers, under the influence of a milder climate,—wherever, yielding to the temperature, the ice produced pools of water,—the development of organized beings might take place; and direct observation has already confirmed what the theory required. Mr Smith of Jordanhill was the first to point out in the post-tertiary clays, which are superior to the till (that is to say, which have been deposited posteriorly to the accumulation of these masses of gravel and rolled blocks in the mud under the ancient glaciers), numerous fossils of species that no longer exist similarly associated on the neighbouring coasts; he has even ascertained the identity of some of those shells with species which have hitherto been observed only in the Arctic seas. A fact so unexpected did not fail to excite my curiosity in a high degree, and I have ever since been unremitting in my endeavours to compare these fossils with living species. Assisted by a collection of living species from Greenland, which I owe to my friend Professor Eschricht of Copenhagen, I have not only confirmed the first impression of Mr Smith, but have further found among the fossils of these clays a much larger proportion of Arctic species than could have been expected. Extending this species of research to the most recent fossiliferous deposits of other parts of Europe, I have every where met with a certain proportion of species whose types no longer exist in a living state in the neighbouring seas, but at 12° or 15° of latitude more to the north. Thus, while the shells, which are now found in lat. 65° to 70° on the coasts of Iceland and Greenland, where the mean temperature is several degrees below zero (32° F.), lived in lat. 55° to 60° on the coasts of Scotland and of England, where the mean temperature at present is + 8° (46°. 4 F.); the species of the coasts of Scotland of the British Channel which now live in lat. 50°

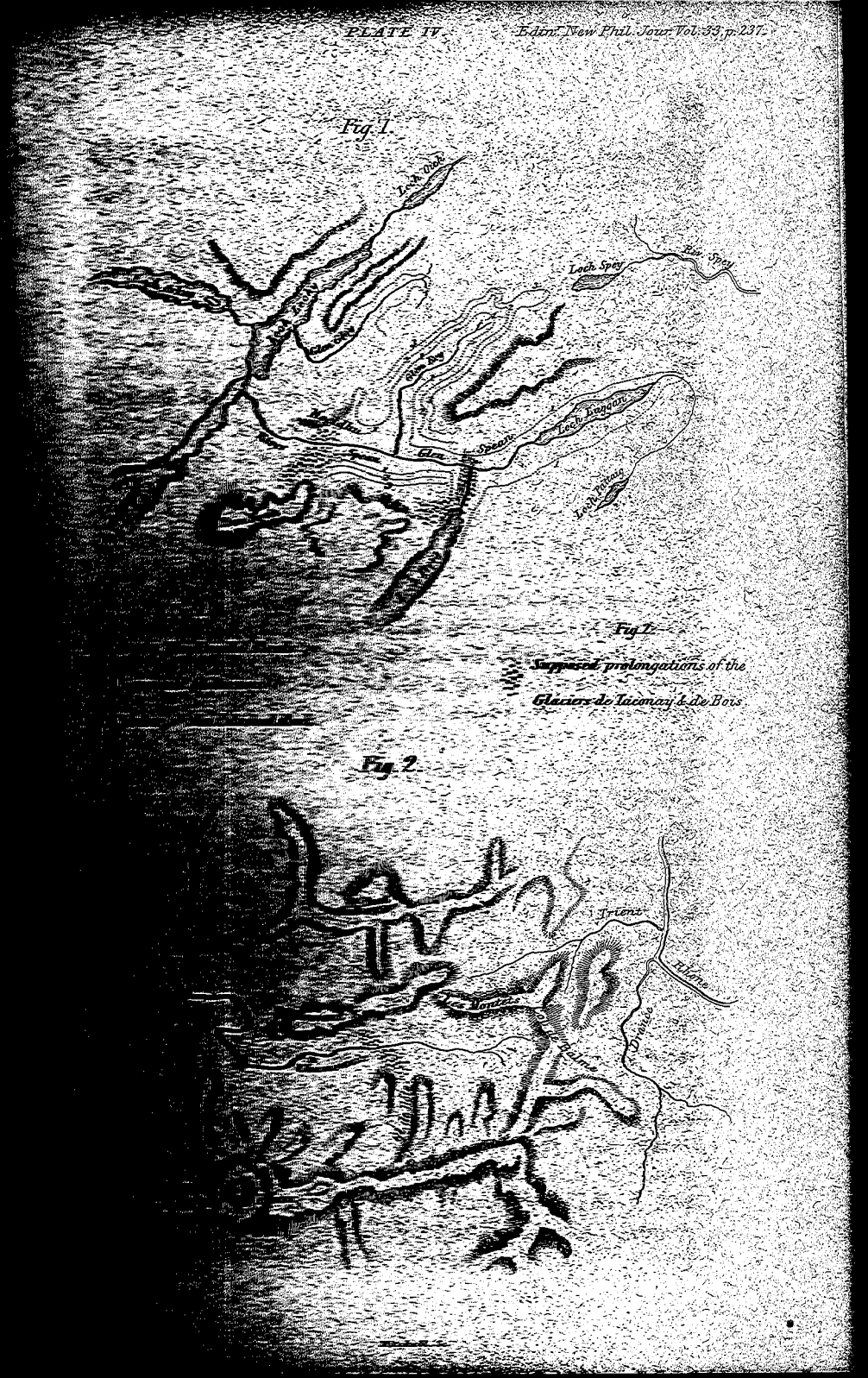
to 55°, lived in Sicily in lat. 35° to 40°; or, in other words, when the climate of Greenland extended its frosts beyond Scotland, when the mean temperature of the British Islands, in place of being above + 8° (46° .4 F.) cent., scarcely reached zero, the present climate of England, and of the north of Germany, prevailed in those parts of Europe which are now the warmest, and where the mean temperature exceeds + 16° (60° .8 F).

I shall afterwards publish the details of these observations, when they embrace a basis sufficiently complete to form an intimately connected whole; it is sufficient, for my purpose at present, to have indicated the principal results of these researches, which confirm the opinion of the former existence of a climate much more rigorous than that which now exists in Europe, by proofs independent of those derived from the traces of ancient glaciers. Now, a climate so different, could not have existed without exercising a marked influence on organic life; and it is thus that the Arctic faunas, in our temperate regions, confirm as fully the existence of ancient glaciers, as the presence of these same glaciers explains the existence of northern animals; and, nevertheless, the facts which establish the presence of the one, have nothing in common with the facts which prove the presence of the other.

III. *Parallel Terraces.*—The third order of facts is that of parallel terraces. This phenomenon is too well known to require description. Justly valued memoirs have delineated these terraces in full detail; and the publications of Macculloch, of Sir T. Dick Lauder, and of Mr Darwin, are so well known, that I shall confine myself to the task of establishing the connection which I conceive there is between these facts and the existence of glaciers in Scotland, without discussing the various theories that have been proposed for their explanation. When I visited the parallel roads of Glen Roy with Dr Buckland, we were convinced that the glacial theory alone satisfies all the exigencies of the phenomenon; and, as this locality is the best known of all those where parallel terraces have been observed, I may limit myself to this example for the explanation of all the others. The flanks of Glen Roy and Glen Spean exhibit horizontal and continuous terraces one above the other, which preserve completely their parallelism throughout their whole extent,

notwithstanding the numerous sinuosities of the valleys. The upper part of Glen Spean, as far as the part opposite Loch Treig, presents only one of these terraces, which surrounds Loch Laggan, and is prolonged round Loch Treig. This same terrace extends along the left side of the valley nearly down to the Bridge of Roy; on the right side it penetrates into Glen Roy, of which it makes the round, following all its sinuosities, and, continuing always at the same level along the lower part of Glen Spean, it terminates, on this side also, opposite the Bridge of Roy. It is worthy of remark, that the level of the *col*, separating the valley of the Spean, which runs to the west, from that of the Spey which runs to the east, is only a few feet above that of this first terrace. As far as Loch Treig, the valley of the Spean is surrounded by one terrace only; but lower down there are two others at different levels; which follow, in the same manner, all the sinuosities of the lower part of Glen Spean and of the whole of Glen Roy; which, almost equal elevations, correspond perfectly on both sides of the valley; and both of which likewise terminate at the same point as the first, but at different levels, viz. near the Bridge of Roy. The first, or the least elevated of these three terraces, is 848 English feet above the level of the sea; and, as it is horizontal, its height above the bottom of the valley depends on the point of observation. The second is 212 feet above the first, and the third 212 feet above the second. It is to be remarked, that the two upper terraces make the round of Glen Roy, whereas, in Glen Spean they do not extend higher than the opening of the valley of Loch Treig. I noticed them on the left side of Glen Spean between Loch Treig and the Bridge of Roy, as well as on the flanks of Glen Roy; and I mention this particularly because they are not indicated at that point in the maps which represent their position (Plate IV. Fig. 1). It is evident that these terraces indicate levels of water. The next inquiry is, if the barriers which restrained these lakes have disappeared, or if the valley has been elevated at different times above the level of the water? The perfect horizontality of these terraces, at three different levels, appears to me irreconcilable with the idea of a repeated soulèvement of the surface. The ablation of a rocky barrier seems impossible, with-

out the influence of a cause which would, at the same time, have occasioned the disappearance of terraces having so little consistence; whereas, in a country which presents so many traces of ancient glaciers, the supposition of a great glacier, descending from Ben Nevis, and shutting up the valley of the Spean, by resting on Moeldhu, which is opposite, combined with the influence of a glacier issuing from Loch Treig, and which would bar the valley a second time at that height, would explain all the facts. The glacier of Loch Treig, of inferior size to that of Ben Nevis, would, first of all, be lowered at two different times, after having for a certain period maintained the water contained between the two glaciers at the level of the two upper terraces. During these two loweringings, the waters would run to the east, proceeding by the valley of the Spey, owing to the inconsiderable height of the *col* which separates that valley from Glen Spean. Whenever the glacier of Loch Treig disappeared completely, the water would be able to extend to the end of Glen Spean, and likewise invade Loch Treig; which explains the continuity of the lower terrace, while the two upper ones terminate abruptly opposite Loch Treig. Afterwards, when the great glacier of Ben Nevis no longer reached Moeldhu, the waters would run to the west, and water would remain only in the hollows which are now occupied by Loch Treig and Loch Laggan. The sudden termination of the three terraces, on the two sides of Glen Spean near the Bridge of Roy, will likewise be understood from this explanation. The supposition now made is confirmed by a fact which there is no other mode of accounting for: viz. that the bottom of Glen Spean in front of Loch Treig is not only polished with that polish characteristic of glaciers, but is moreover scratched transversely, that is to say, at right angles to the direction of the valley, by a cause which evidently proceeded from Loch Treig. I do not believe that a locality exists, where the facts indicate, in a more special manner, the cause which has produced them. The horizontal terrace of Glen Gloy is susceptible of a very natural explanation by a glacier issuing from the valley of Loch Arkeig, crossing Loch Lochy, and damming up Glen Gloy above Low Bridge. This supposition would also clear up the difference



of level between the terrace of Glen Gloy and those of Glen Roy, and would obviate the necessity of imagining *soulèvemens* of the neighbouring valleys, which communicate in the same manner with the ocean, and do not nevertheless exhibit any trace of terraces.

In following up these facts in all their variety, we are easily enabled to explain the numerous terraces which we meet with in Scotland, by supposing barriers of ice at the mouths of the valleys, whether it was that the lateral valleys closed them by their glaciers, as at the Bridge of Roy, or that the waters of the sea, by heaping up ice on the coasts, offered a temporary obstacle to the running off of the waters of the land, or interrupted large sheets of salt water. The presence of an Arctic fauna, in the deposits superior to the till, which might be found in these creeks of the sea, would thus present nothing but what is quite natural.

In order the better to understand the explanation I have given of the phenomena of Glen Roy and of Glen Spean, I will now endeavour to compare them with what would take place in the valley of Chamounix, if the *Glacier des Bosons*, or that of Tacony, by extending a little farther than it now does, had barred the valley in such a manner as to interrupt the passage of the waters of Arve, and force them to flow off by the valley of Thiers (Plate IV. Fig 2). A lake would thus be formed above Chamounix, of which the level would leave some traces on the sides of the valley; but, when the glacier was no longer sufficient to the height of the *Col des Montets*, the water would flow to the west, as at present, forming terraces as often as it remained for a certain time at a fixed height. To render the comparison still more striking, let us suppose the case of a second glacier, as, for example, the *glacier des Bois*, also shut up the valley; then, as in the instance of Glen Roy, there would be between the two glaciers a lake, whose waters would first of all descend to the east over the least elevated glacier, and would not acquire their natural direction to the west until the two glaciers disappeared, and the bottom of the valley was free. Ice, cast on the coasts of Holland at the present day, and interrupting the dispersion of the sand transported by the Rhine and by the Scheldt, would every where reproduce the

phenomenon of stratified fossiliferous deposits and of parallel terraces, such as we find so frequently in Scotland.

It was in Scotland that I acquired precision in my ideas regarding ancient glaciers. The existence in that country of so considerable a network of these traces, enabled me to appreciate better the geological mechanism of glaciers and the importance of many facts of detail observed in the neighbourhood of those which now exist. There, also, I was able to appreciate the influence exercised on these phenomena by the vicinity of the sea, and to distinguish the effects due solely to the waters of the sea from those where the ocean and glaciers were in contact, and from those produced where the glaciers never reached the sea. Who would now deny that glaciers formerly possessed in many localities an infinitely greater extension than at present? Who would reject the idea that the cause of this cold has been general, and attribute to local causes effects so diffused over the surface of the globe? And if these conclusions must be admitted, who does not perceive that physical theories must undergo some modifications before they can be made to embrace the whole phenomenon which I have described? It is thus that the study of facts in detail always reacts on general ideas; while, in their turn, theories, in order to be supported, force their partisans to the investigation of new facts. The activity of mind that is engendered by the contests to which researches of this kind always give rise, is probably the greatest enjoyment afforded us in this world.

II. Researches on Existing Glaciers.

The chief question in the glacial theory is undoubtedly that of the motion of glaciers; it is the keystone of the arch of the edifice, for it includes at the same time the solution of the phenomena of the present time, and of those which occurred before our epoch. It is well known that opinions have hitherto been divided as to the progression of glaciers, and that many geologists, refusing to admit the theory of dilatation, still defend the opinion of Grüner and of Saussure, which maintains that a glacier slides along its bed.

In my *Etudes sur les Glaciers*, I have entered fully into the

reasons which caused me to reject the theory of Saussure, and to deny the melting action of terrestrial heat on the bottom of a glacier. This negation leads to a consequence of great moment for the theory of the motion of glaciers, the immobility of glaciers in winter; and it does so in the following manner:—If, as I suppose, the motion of glaciers be really produced by infiltration and the daily congelation of the rain-water or of the water resulting from the melting of the superficial ice, glaciers ought to be stationary so long as there is no water at their surface, consequently during the whole winter, because they are destitute of water at that season. If, on the contrary, it be the terrestrial heat, which, by melting glaciers at their base, produces partially or entirely their sliding movement, this action ought to be perceptible at all periods of the year, independently of the seasons and of the oscillations of the temperature of the atmosphere; and if this last be the case, glaciers ought to furnish water during the whole year, in winter as in summer. Saussure himself followed this train of reasoning; and in order to arrive at a solution of the question, he went during winter to the valley of Chamounix, and witnessed the escape of pretty considerable streams from the arched terminations of glaciers, although they were less abundant than in summer. Thenceforward the point appeared to him to be settled. It was too cold for the sun to have the power of melting the ice; elsewhere, the whole country was buried under a thick crust of snow. It was, therefore, there could no longer be any doubt, the terrestrial heat which melted the glaciers beneath. Nevertheless, one reflection could not but present itself: if we consider that glaciers generally occupy the bottom of deep valleys, we must admit that it is there where the springs which circulate in the interior of the mountains ought to have their exit. The presence of water does not then prove absolutely that it results from the melting of the glacier. On the other hand, if the action of terrestrial heat exercise its influence in winter as in summer, it ought to do so on all glaciers, and not one of them should be dry. Further, the water of glaciers has a peculiar character, which distinguishes it from spring-water; it is always mixed with earthy matter,

which it carries off from the bed of mud or of gravel that is interposed between the glacier and the rock. If it be really glacier-water which escapes from glaciers in winter, that water ought to possess these qualities; whereas, if it be spring-water, it ought to be clear and limpid, from where it begins to run.

It was of importance that I should myself examine on the spot the value of his reasoning; and it was with this view that I visited, in company with M. Desor, the glaciers of the Bernese Oberland, at the beginning of the month of March last year (1841), a period when winter reigns supreme in the High Alps. We selected the glaciers of the Aar and of Rosenlaui as the field of our observations, and proceeded towards the Hospice of the Grimsel, by ascending the fine valley of Hassli, which was still entirely covered with snow. The Aar, above Meyringen, was reduced to a small stream, the water of which was much clearer than in summer,—a circumstance which led us to suppose that it was chiefly, if not entirely, composed of spring-water. Bridges of ice traversed the bed of the river at a multitude of places, and as they were very thick, we were able to cross them in perfect safety. Above the height of 4000 feet, there only remained but a very small thread of water. The snow had frequently a thickness of 10 feet; the great and beautiful cascade of the Handeck had disappeared; and, on examining the beds of gneiss forming the precipice over which the water is precipitated in summer, we saw with astonishment, that they had partially preserved their projecting angles, and were not at all worn, as we should have supposed they must have been, especially when we remember that the Aar transports an enormous quantity of gravel. I insist on this fact, because it proves that if the water which has struck with violence against these rocks for a long series of ages, has not succeeded in rounding them, it is impossible to understand how we can attribute to the action of water the polish and the rounded form of the flanks of this same valley, at places where the inclination is much less considerable, and the valley much less contracted, and where the current must have acted with a proportionably much smaller degree of violence. It may perhaps be objected that these

projecting angles are the result of a recent fall of rock, as in the case of the Falls of Niagara, and that the water has not yet had time to round them; but we must attend well to the fact, that we have not here to do with stratified rocks, like those of Niagara, but with a very compact gneiss, which is very obscurely stratified, and which, consequently, is not at all liable to breaking down.

At the Grimsel the snow was still thicker than at the Handeck; the bed of the river was entirely filled, and the glacier, as well as its moraines, were concealed under the snow. We were not deterred, however, from visiting the *Hôtel des Neuchâtelois*, which is situated at a distance of more than two leagues above the extremity of the glacier, and at a height of 7200 feet. I borrow from the yet unpublished narrative of our expedition by M. Desor, some passages which will give an idea of the aspect of those regions in winter, and of the obstacles against which we had to struggle, and will, at the same time, report the observations we were enabled to make:*

"The distance from the Handeck to the Grimsel is only two leagues, but as the snow became always more abundant as we advanced, we could not hope for an easy road. The most difficult places were the woods of young pine trees. The bed of snow which covered them was unequally distributed, and when we accidently stepped near one of the trees, we were immersed up to the waist; an occurrence which, on each occasion, caused very fatiguing shocks. At Raetherischboden, the last encampment of the valley, a small thread of water occupied the bed of the Aar; but the water was so pure that, from the first, we supposed that it must come from some spring, and not from the glacier. It did not carry along with it any of those plates of mica whose presence gives to the water of glaciers that sparkling appearance and that milky tint which characterize it.

"The last league seemed to us the longest. The heat and

* The narrative from which M. Agassiz extracts this passage has been published, since the present memoir was written, in the *Bibliothèque Universelle de Genève*, No. 76. We had intended to transfer it to our pages, in whole or in part; but this is now rendered unnecessary.—Edit.

the difficulties of the road had so exhausted us, that we were obliged to repose several times in order to recover our breath. At length we heard the barking of the dogs of the Hospice. It was, as it were, a friendly voice calling us to be of good cheer. We felt our vigour reanimated by this encouraging sound, and in a few moments afterwards, we saw making their appearance, on the mountain which rises above the Grimsel, the keeper of the Hospice, accompanied by his fine Newfoundland dog, Barry.

"A small traffic of exchange is carried on between the Valais and the Hassli, which is not entirely discontinued during the winter, and of which the Grimsel is the warehouse at that season. The Hasslians bring their cheese, the Valaisans their wine, their brandy, and various kinds of provisions, among others, rice, which comes from Italy by the Simplon or the Gries. The two parties stop at the Hospice, sleep there, and are at home next day, carrying with them cheese, if they descend to the Valais, and wine and brandy, if they return to the valley of Hassli. It is for the purpose of facilitating this communication that the keeper of the Hospice is bound to have a man and two dogs at the Grimsel during the whole winter, and likewise to place picquets on the whole mountain of the Grimsel between the Hospice and the Valais, to point out the path to travellers.

"It is unnecessary to remark, that, to allow of this commerce being carried on in winter, the weather must not be too severe, for it would be madness to attempt such a journey in the midst of snow or wind. Thus the Grimsel, at such a time, is altogether solitary, and the keeper told us, that during the winter of 1839–40 he had passed thirty-five days without seeing a human figure. 'This long isolation,' he added, 'seemed to me so painful, that, on perceiving the first traveller who passed the Grimsel, I threw myself on his neck, embraced him, and offered him a bottle of wine.' The dogs here are at least as important as the men for watching, on account of the extreme delicacy of their senses, and especially of that of smell. All the guides assert that in serene weather, and especially in winter, they detect the presence of a man at the distance of a league, and Jaun assured us, that an hour before our arrival,

he had already remarked, from the inquietude of Barry, that some one was approaching the Hospice.

"Those who have visited the Grimsel in summer will doubtless remember, that, in order to enter the vestibule, it is necessary to ascend a stair about seven feet high. Now, to give an idea of the quantity of snow which was accumulated round the house, it is sufficient for me to state, that, in place of *ascending* to the vestibule, we *descended* there by a stair which Jaun had cut in the snow. The lake of the Grimsel was completely invisible, for an uniform bed of snow extended over it, and did not even allow its limits to be recognised. But the stream which issues from it, although more abundant than we should have expected, was only visible in the interior of the building, whose floor it traverses. It is well known that the spring which gives rise to this lake, like many other springs in the Alps, is a thermal one. We were not, therefore, astonished to find the water at some degrees above zero (32° F.) We were subsequently assured by M. Zybach that the lake never freezes, whatever the degree of cold may be, and that even the enormous bed of snow does not rest on a crust of ice, but extends over the surface of the water; and he added, that, on sinking a rod through the snow, the water spouted to the surface. We were entirely ignorant of this peculiarity, or we should not have failed to investigate it. Perhaps other naturalists may find an opportunity of doing so.

"The first thing we did on our arrival was to place our thermometers. M. Agassiz sank a minimum thermometer in a hole pierced in the snow to a depth of five feet, while another thermometer was fixed at the surface. We were astonished to find that at seven o'clock in the evening the temperature of the air was not lower than −4° C. (+ 24° 8 F.), although the sky was perfectly serene. We were very desirous to ascertain the humidity of the atmosphere, but, to our great vexation, our hygrometer had been broken during our journey.

"We went to bed at an early hour, immediately after supper; having decided to start for the *Abschwung* the following morning at four o'clock. Our guides made us still hope that perhaps the cold of the night might be sufficiently great to cause the snow to bear us, which would considerably facilitate

our walk. At three o'clock we were up, and while Jacob prepared the coffee, we examined our thermometers, and saw, to our great disappointment, that the cold was far from being so intense as we imagined; for, as during the previous summer, we had on serene nights, seen the thermometer descend to −5° and 6° (+23° and 21°.2 F.), we expected to find it now at −12° or 15° (+10°.4 or 5° F.), or even lower. In place of that, the temperature of the air was −2° (+28.4 F.), and the thermometrograph which was sunk in the snow indicated −3° (+26°.6 F.). To what cause are we to attribute this singular state of the temperature? We asked Jaun if it happened often that the nights were so mild, and he replied, that, for a long time past, it had not been so cold as formerly. Notwithstanding this temperature, the snow bore us while we descended the declivity which leads from the Hospice to the bed of the Aar. We now looked forward to the prospect of proceeding with light steps over the hardened crust, and of scaling the edge of the glacier with equal facility; but we had scarcely advanced a few paces in the valley, when we arrived at a place where the crust gave way under our feet. Under this superficial crust the snow was very fine, dry, and powdery, and we sank sometimes with the one foot, sometimes with the other, generally up to the knee. We attempted to regain the flank of the valley, but this was still worse. It was necessary for us, nolens volens, to be satisfied with advancing very slowly. We had to endure a double punishment; in the first place, on account of our impatience, which could not be reconciled to such a mode of progression; and in the second place, owing to our knees, which suffered from the constant repetition of the shock against the superficial crust, each time we went down. In the mean time, day made its appearance, after having been announced by a sensible diminution of the temperature; and the sight of the summits becoming gilded one after the other, still further augmented the provoking annoyance caused by our detestable path. There is nothing more painful for a man who is conscious of possessing a certain degree of energy, than to feel himself yielding to the pressure of physical obstacles. I was conscious of my strength becoming exhausted at every

step I made, and I was impatient at seeing Agassiz taking the lead. On no other occasion had I ever felt the slightest jealousy, because I should have had the conviction, that, by means of a little effort, I could have made up to him; but now, on the contrary, I was obliged to confess my weakness, and was therefore deeply mortified.

"We could have concluded from the aspect of the glacier, as seen from a distance, that no stream escaped from it, or at least that it was not visible externally, owing to the thickness of the snow. We encountered the last traces of water near the hut of the shepherd, in front of the gorge of the Oberaar, at a place where the snow was moist. Its temperature was at zero, and it proceeded, according to all appearance, from those springs which flow in summer on the polished flanks of the valley, and give rise to small marshes which here occupy the left flank. The cascade of the Trübtenen and the torrent of the Oberaar had disappeared, and their bed was only indicated by immense stalactites of ice, suspended on the rock. But what interested us most, was the outline and the form of the glacier itself. If the glacier had continued to move and to increase during the winter, it ought to have left traces of that movement; and as the snow accumulated at its extremity had fallen some months before, it would have been gathered together in a forward direction, or at least some swelling would have been perceptible, immediately in front of the terminal edge. In place of that, the snow presented a regular talus, such as is formed by the snow where it has been driven by the wind against the flanks of valleys.

"It was seven o'clock when we reached the edge of the glacier, and we had thus taken two hours to accomplish what in summer never occupied more than 40 minutes. I was dreadfully fatigued, and I had had so many and such repeated falls, that my knees were quite galled. I declared to Agassiz that I should proceed no further. If the plain had seemed so smooth, what would it not be when we should arrive upon the glacier? But notwithstanding my remonstrances, Agassiz was determined to persist; and he represented to me, that the snow could not fail to be softened when the sun had acted

for some hours on its surface, and that our journey would then be much less laborious. These and other reasons induced me to attempt the ascent of the terminal edge of the glacier, which we found much less difficult than we had figured to ourselves. The snow had rendered the slope much more gentle than it is in summer; for not only was there no trace of the moraine to be discovered, but even the inequalities and the very considerable notches of the extremity of the glacier had completely disappeared. We saw no vestige of the stream. Having arrived on the surface of the glacier, we found here and there blocks whose tops emerged from beneath the snow; but their lower sides were alone visible, for the surfaces directed to the upper part of the glacier were invariably concealed by a covering of snow—a proof, that, in winter as in summer, the prevailing winds are from the west, parallel to the axis of the glacier. Having found the surface of the glacier more practicable than the plain, we decided on continuing our march, with the intention of retracing our steps afterwards. Our guides did not give us much encouragement, for they knew no better than we did the state of the glacier in winter. Our march was still very slow and arduous, as may be well supposed; but we did meet with portions where the snow was bearing. We then experienced extraordinary relief, and, in spite of fatigue, we ran like children upon the hardened surface, until the snow anew gave way under our feet, and again calmed our ardour. At about a third of the distance between the extremity of the glacier and the Hôtel des Neuchâtelois, we met with an enormous block supported on several pedestals, and covering a pretty considerable hollow, at the bottom of which we perceived pure ice. It was of importance for us to ascertain the state of the ice of the glacier. Having descended with due precaution into the hollow, we saw that the block covered the widened extremity of a *crevasse*, which penetrated to the right into the interior of the glacier, and whose walls exhibited that same magical reflection which is observed during summer; the azure appearing to us even more brilliant than usual, doubtless because this was the only point where we were able to see it. We were thus assured that the *crevasses* are not filled with snow, but only covered by an arch more or less thick. Our

guides assured us, that the water disappeared from those *crevasses* which contained it during summer, either by its flowing away or becoming frozen. Let it not be objected, that this is quite natural, and that it is useless to mention it. It frequently happens that, in summer, the mean temperature of several consecutive days is some degrees below zero (32° F.), without the water of the *crevasses* being frozen; there is only a pellicle of ice formed, which covers it during the night, and disappears during the day. A circumstance which inclines me to believe that the water they contain during the summer freezes rather than runs off, is, that, on the glaciers, we have on several occasions met with circumscribed portions of ice in the form of an ellipse, which had completely the appearance of congealed *baignoires*. The ice was full of air-bubbles, but its surface was perfectly smooth; it was extremely hard, and more brittle than usual. The bubbles were pretty uniformly distributed, and were nearly of an equal size, affecting in general an elongated and pyriform shape; but we did not remark any of these vertical laminæ of varied colour, to the study of which M. Agassiz means afterwards to devote particular attention. Subsequently we did not find any example of ice so full of bubbles, except on the last summit of the Jungfrau. It would be extremely difficult, in the present state of our knowledge, to point out the cause of these differences. Up to the present time we have only obtained very imperfect data regarding the modifications to which ordinary ice is subjected under the influence of atmospheric agents; and, as to the modifications which glaciers undergo in the interior of their mass, at different periods of the year, these are as yet entirely unknown.

We were scarcely able to recognise our glacier of the Aar, so varied and animated in summer, under the uniform veil of snow. The great medial moraine itself was more or less effaced, and only formed a longitudinal ridge, whose sides were much less inclined than in summer. We first of all visited the northern flank, and when we had passed over the upper third of the glacier, we crossed to the southern flank, at the point where the moraine is considerably swollen. We were happy to see, to our great satisfaction, that our route improved

more and more. The snow was much more compact, so that even when its external crust yielded, we did not sink very far. There was no longer any doubt of our arriving at the Abschwung. But another inconvenience came in the place of the difficulty of walking, and that was the intensity of the light. In proportion as the sun attained a greater elevation, its rays were reflected with such power by the millions of crystals of this vast snowy region, that the blue glasses with which we were provided became insufficient; and in order to remedy this, and to preserve the skin of our faces, we were obliged to envelope our heads in a double veil, under which we breathed as if we had been in the middle of summer. It was not without some astonishment that we here met with a small butterfly, which fluttered around us. It seemed to be perfectly at its ease, and was, according to M. Agassiz, the species named *Vanessa urticæ (La petite Tortue)*.

"It was eleven o'clock when we arrived at the height of our old dwelling, and we were very much astonished at not being able to discover the Hôtel des Neuchâtelois. Was it possible to conceive that the immense block, which was seen from so great a distance in summer, and whose summit had so often reanimated the courage of our visitors, had been entirely interred in the snow? At last, after having sought for it on all sides of the moraine, we descried at some distance a swelling in the snowy ridge, and this proved to be our hotel. It was entirely covered by snow. On one side only we saw one of its walls uncovered for a space of some feet; but, in order to penetrate into the interior, it would have been necessary to clear away an enormous bed of snow, which would have occupied a great deal of time, and we therefore preferred reposing on the snow. Agassiz was in very high spirits, rejoiced to find himself, in such magnificent weather, in the midst of that sea of ice which he had made the scene of his observations. In truth, the spectacle which we had before us was of an unique character. It appeared to us, that we had never seen the air so transparent. The outlines of the mountains were delineated on the blue back-ground of the sky with a precision never witnessed in summer. All the peaks

which bound the glacier were clothed with snow from their base to their summit; and the Finsteraarhorn alone was black as in summer, for its walls are too precipitous on the side next the glacier, to allow of the snow adhering to them. As to the glacier itself, it really did not exist for us at that time; for we had nothing else before us but an immense extent of very uniform snow, which wanted the magic charm given by moraines, as well as the crevices with their brilliant tints, the icy cascades, and the thousand rills of water with their harmonious murmur, all of which constitute the delights of the scene in summer. The two rods which we had introduced the preceding autumn into the holes that had been bored, and of which mention will afterwards be made, were elevated only a few feet above the surface of the glacier; but they had preserved their respective positions, and were both nearly vertical;—a proof that the superficial beds of the glacier had not since that period advanced in an unequal manner. We then ascended to the Abschwung, and saw that the snow had completely filled up the space between the rock and the *névé*. We estimated the thickness of the bed of snow at that place at 30 feet. At noon, we returned to the Hôtel des Neuchâtelois; and as I felt myself indisposed, I decided on returning with a guide. Agassiz remained for the purpose of making some observations on temperature. His object was to ascertain if the temperature of the snow was the same as at the Grimsel. With this view, he introduced into the snow, at a depth of 8 feet, the same thermometrograph, taking care to close the hole. After two hours, the instrument indicated—4°.5 (+ 25°.7 F.), the air being at + 1° (+ 33°.8 F.).

"The descent seemed to me as easy as the ascent had appeared difficult. We sank about half a foot; but the snow was firm, less powdery than in the morning, and slightly granular, which considerably facilitated our walking. It is not so much the thickness of the snow, as the inequality of surface, that produces fatigue in these walks, and it does so by causing unexpected jerks, which are always annoying. I fell, whenever I met with an eminence which cast shade. To measure the temperature of the air, and I was surprised to find, that the thermometer almost invaria-

bly remained about zero (32° F.) On one occasion only, I saw it ascend to + 1° (+33°,8 F.) M. Agassiz made the same observations, and obtained the same results. I ought to add, that these observations were made at a height of only one foot above the surface. It was impossible for us, from want of shade, to place the thermometers in a higher position, so that we could not determine, in an exact manner, the influence of the radiation of the snow. In the sun, the heat was excessive; and therefore we not only made no use of our cloaks, but, in order to be more at our ease, took off our coats and waistcoats. The necessity of keeping on our double veils was, in these circumstances, a real punishment. I attempted several times to remove them for a few moments, in spite of the advice of the guides, and afterwards I could not too much repent having done so, if it was to that cause that I had to attribute the miseries of the succeeding night.

"Agassiz rejoined us at the Hospice of the Grimsel about four o'clock. I had, in the mean time, prepared a bowl of punch, by means of the essence which we always took care to have in our possession; and I need hardly say, that this drink, invented expressly for icy regions, appeared to us a real luxury. When we were all seated round the table, in the small low apartment, which serves as a shoemaker's workshop, we experienced a lively satisfaction in recalling the most trifling events of the day; and, proud of our success, we formed a thousand projects for the future. It was then, among other plans, that we conceived, for the first time, the idea of attempting the ascent of the Jungfrau. Jacob had prepared the supper, which, like that of the preceding evening, consisted of rice soup, salt mutton, and chamois steaks. This last dish was not, I must allow, very juicy; but as it was of chamois, we were obliged to regard it as delicious.

"We soon went to bed, in order that we might be able to start at a very early hour in the morning; but we had scarcely reposed an hour or two, when I experienced the most violent pain in the face. My head seemed on fire, and I felt my cheeks swelling and my face cracking. In vain I sprinkled myself with cold water—I suffered the agony of a martyr. Agassiz awoke a few minutes afterwards with a deep sigh. I am in great pain, he said; my lips feel as if they were torn in pieces; what can be done to assuage this suffering? For a moment we thought of going out and immersing ourselves in the snow, but reflecting that such a remedy might produce serious consequences, we resolved to endure our misery till the morning. It was a terrible night for us. Towards the morning the pain gave us a little respite; we reposed a few hours, and when we rose, we could not restrain our fits of laughter, on looking at each other. Are you aware that you have the appearance of a *cretin?* said Agassiz. Have the goodness to ask for a looking glass for yourself, I replied. Our faces were coloured purple, and horribly disfigured; I could scarcely open my eyes, so great was the swelling of my eyelids; and Agassiz had his lower lip excessively swollen and pendant. Nevertheless, we decided on starting the same day. Our thermometers gave us the same results as on the preceding day; that is to say, the thermometrograph, at a depth of five feet in the snow, indicated—3° (+26.6 F.), and the thermometer in the air stood at +2° 5 (+ 36.5 F.), at eight o'clock in the morning."

It is apparent from this description, that the water which existed in the neighbourhood of the Grimsel was spring-water; and what proves that the glacier of the Aar had not moved for a long time, is the fact that the snow was not gathered together at its extremity. We also observed the same continuity of the snow at the extremity of the glacier of Rosenlaui, and our guides assured us that the snow is never seen collected together by the ice in winter, as is the case with the gravel and the soil in summer, when glaciers are advancing. Every thing, therefore, concurred to convince me, that glaciers do not advance in winter. The glacier of Rosenlaui, furnishes me with another proof of the same thing, and in the following manner: This glacier terminates at the edge of an abyss into which the torrent, escaping from its extremity, is precipitated in a cascade. Such a circumstance could not but be very favourable for my researches. I found, as I had foreseen, the internal edge of the glacier exposed; not a drop of water escaped from it, for, if there had been only a few drops, these would have found their way to the edge of the uncovered hollow. I am well aware that it may be urged, that perhaps this

water loses itself in the interior of the rock, by flowing into a crevice or a cavern before reaching the extremity, and that, consequently, the absence of water at the terminal edge does not prove its total absence, any more than in the case of other glaciers. But supposing that it were so, the glacier ought at least to be moist at its bottom; for, assuredly, if the melting caused by the earth took place in winter in spite of the external temperature, it is not a glacier descending so low as that of Rosenlaui, and consequently surrounded by a warmer atmosphere than many others, which ought to be the exception to the general rule. Now, I convinced myself that the whole bottom of the glacier was frozen to its bed, and consequently that all movement was impossible. I believe myself therefore entitled to affirm as a thing demonstrated, *that glaciers are stationary in winter*, that the water which escapes from them is spring-water, and that it is not at all the result of the melting caused by terrestrial heat. For, as has been already remarked, if that melting really took place, it ought to occur in all glaciers, and not a single one ought to be dry.

I took advantage of this winter excursion to make another experiment relative to the formation of polished surfaces,—an experiment which, I believe, will not be devoid of interest for future researches. It is known that I attribute polished and striated surfaces to the action of ancient glaciers which formerly covered the localities; and I do so, because they are completely identical with those met with under existing glaciers, and along their flanks. At first a pretence was made of not assigning any importance to these polished surfaces, which were attributed to various causes; and M. de Charpentier himself, in his last work, seems to have allowed himself to be intimidated by the criticisms of opponents, for he says, "If there were no other facts to adduce in favour of the diluvian glaciers, but these marks of attrition and these furrows, the observation of M. Mousson (who attributes them to currents), might be well founded."* But since that time facts have spoken for themselves; and they have been more eloquent

* J. de Charpentier, Essai sur les Glaciers, p. 203.

than the most beautiful theory could be. There is now no one who ventures to dispute the importance of a phenomenon so generally distributed; but some geologists, refusing to adopt the glacial theory, have pretended that the polished surfaces, on which glaciers of the present day repose, are not the result of the action of glaciers, but have been produced by a cause anterior to their existence. This opinion has nothing probable in it, because, in the Alps, the phenomenon is too intimately connected with glaciers to be regarded as independent of them; but it would be of importance to have an experimental proof of this. As all the glaciers of the Bernese Oberland are at present in a state of increase, I conceived the idea of making an accurately determined mark at a place which I supposed would soon be invaded by the ice. For this purpose, I caused a corner of the glacier to be removed; and it was by this means that I assured myself that the intermediate bed of mud and of gravel was so frozen as to be incorporated with the rock. We succeeded, however, in uncovering the surface of the rock; and we then cut out in the polished rock, at a place whose position was accurately ascertained by means of fixed points, a triangle, having a base of about a foot, from which we removed the rock to the depth of half an inch, having rendered the surface as rough as possible. If, as I have no doubt, it be really the glacier which polishes the rock, this triangle ought to be repolished and striated within a certain number of years. We have no idea of the time required by a glacier to polish its bed, but it is probable that the duration of that time varies according to the weight of the mass, and according to the nature of the rock; and, as the bed of the glacier of Rosenlaui consists of a black limestone sufficiently susceptible of being acted on, we may hope to obtain a result more speedily than if the rock had been gneiss or granite.

I had hoped that the glacier would soon have invaded anew the place we had uncovered in the month of March; but I was a little disappointed when I visited our glacier with Messrs. Forbes and Heath in the month of August following, to see my triangle still exposed. The glacier had advanced, it is true, but on its left side, while the right flank had re-

mained stationary. We cut out a second but smaller triangle under the ice itself, at a distance of 17 feet 5 inches from the first, and I hope to find both covered by the ice next summer.

Another objection which has been made to my theory is the following. If, as you say, the temperature of the glacier is constantly at 0° (32° F.) and under it, how can it be conceived that the water should remain liquid and penetrate into the interior of the mass? Will it not freeze by the simple contact of the ice? The action of the dilatation ought, therefore, to be confined to the surface, and ought not to produce any effect on the inferior beds of the glacier. This reasoning is undoubtedly just, but those who have made use of it as an objection here maintain a physical necessity which does not exist in nature, when they suppose that the infiltrated water, which acquires the temperature of the glacier by being introduced into it, must necessarily freeze when its temperature descends below zero. Because all ice has a temperature at least as low as 0°, it does not follow also, that water cannot exist below 0°. It will suffice to mention here, the experiments of Professor August,* which have demonstrated that water can be preserved in a liquid state, *in vacuo*, at − 12° (+ 5° F.), and even at − 13°5 R (+ 3°87 F.). According to that author, no shock, however violent, can produce congelation under these circumstances at a temperature of − 2°.5 R (+ 28°.6 F.) Now, may it not be the same with the water which penetrates into the interior of the glacier? But science ought not to rest satisfied either with possibilities or probabilities, when we have to do with a phenomenon accessible to our researches; and the question can only be determined in a definitive manner by direct experiment. I had, in other respects, so much the more interest in obtaining information on this subject, because, last summer, having reached no deeper than 25 feet in my boring attempts, the results which I obtained might, to a

* Poggendorff's Annalen, vol. lii. p. 184; and extract in Bibliothèque Universelle de Genève, 1841, p. 191.

See also Professor Kries in Poggendorff's Annalen, 1841, No. 4; and in Edinburgh New Philosophical Journal, vol. xxxii. p. 198.—Edit.

certain extent, have been attributed to the influence of the temperature of the external air. I therefore went a second time, in the month of August last, to the same glacier of the Aar on which I had lived during the preceding summer. On this occasion I remained more than a month including some more or less distant excursions (from the 8th August to the 10th September), having been accompanied by several friends who were naturalists, some of whom had been with me during the previous years. Messrs Forbes* of Edinburgh, and Heath of Cambridge, likewise spent nearly three weeks with me at the Hôtel des Neuchâtelois. Afterwards, my friend M. Escher de la Linth, took an active part in our labours. I passed 27 days altogether on the glacier, during which time I succeeded in sinking the piercer to a depth of 140 feet. As it is the first time that an attempt has been made to penetrate to a great depth into the interior of a glacier, it may not perhaps be uninteresting for my readers to be informed of the mode of procedure followed, and the results obtained.

The attempts which I made the previous year, had shewn me that a glacier could not be so easily pierced as one would at first suppose. I looked forward with pleasure to being able this year to observe the phenomena presented by the glacier at its contact with the rock. I communicated my new projects to M. Koehli, engineer at Bienne, and we were convinced that we should employ iron piercers like those used in the construction of artesian wells. M. Koehli was good enough to entrust me with his own piercer, which is 150 feet long. It was, he said, the greatest depth which I could reach by piercing with the hand; while a larger piercer would have required considerable scaffolding, and an outlay of money which would greatly have exceeded my pecuniary resources.

* Notwithstanding the painful disputes which afterwards arose respecting the phenomena in the structure of glaciers, whose discovery was claimed by Professor Forbes, I am far from regretting our residence together on the glacier; and if any thing could console me for the vexation I experienced on this subject, it is the thought that the visit of that gentleman to the Hôtel des Neuchâtelois, has contributed to diffuse more widely in England and Scotland the knowledge of the mechanism of

As, however, the thickness of glaciers is generally estimated at from 80 to 100 feet, I hoped to reach the bottom of that of the Aar with this boring apparatus. In order not to be stopped by unforeseen difficulties, I took with me M. Koehli's foreman, who was entrusted with the direction of the boring operations. The piercer was composed of ten bars, an inch in diameter and fifteen feet long; the cutting portion of the various *fleurets* was three inches, three inches and a half, five inches, and six inches in diameter. All the bars, as well as the other instruments connected with the piercer, such as the keys, *cuillers*, ropes, &c., were carried by men from Meyringen to the Hospice, and thence to our hut on the glacier, that is to say, to a distance of ten leagues from Berne. I established myself, along with my companions, under the same block which had sheltered us the preceding year, and which is now known to the scientific world by the name of the *Hôtel des Neuchâtelois*. It is a large block of mica-slate, forming a part of the moraine which descends from the flanks of the Schreckhorn, at a league higher up. Its length is 41 feet, its breadth 30 feet, and its height 19 feet. One of its angles projects in the form of a roof on the south-west side. It was this place which we again chose on this occasion as a shelter. I had sent some days previously two of my guides to the glacier to prepare our abode. They found the walls which had been constructed the previous year completely dislocated by the movement of the glacier. The space, however, which they contained, would not have been sufficiently large for our encampment of this year. The hut was therefore rebuilt, and arranged in such a manner as to shelter eight persons. The kitchen was prepared in front of the entrance to the hut, and the store-room at the side, under another block. The guides constructed for themselves a second hut on the left side of the glacier, at the distance of half a league. It was not, like our own one, built on the ice, and, in this respect, it was more solid and less precarious; but ours had the advantage of being situated in the middle of the glacier, at the most favourable point for our observations.

As soon as we were fairly settled in our hut, I commenced the boring operations. In the previous year I had tried two kinds

of instruments, a piercer with two teeth, and one with a *fleuret* having four teeth; but I soon perceived that the latter instrument had the disadvantage of grinding the ice too violently, which often rendered it extremely difficult to draw it out. This year I had only brought simple chisels of different diameters, and another instrument having the form of an inverted funnel, with circular and angular edges, which our miners call a *couronne*, and of which they make use in piercing very soft substances. At first, and to a depth of 40 feet, this instrument worked very well, but afterwards it became necessary to give it up; for, although its edges were very blunt, yet the weight of the rods becoming always greater, caused it to be too firmly fixed in the ice, and rendered its withdrawal a matter of infinite difficulty. We therefore exchanged it for simple chisels.

I had remarked that the piercer acted better when the hole was filled with water than when it was dry; and, accordingly, I took care to place the bore in communication with one of the numerous rills of water which flowed over the glacier. This had, moreover, the advantage of enabling the borers to dispense with the necessity of emptying the hole every minute; for all the splinters of ice which the piercer detached at the bottom of the hole, came up of themselves to the surface, merely owing to their smaller specific gravity, and they were carried away by the current. But this proceeding had likewise its inconvenience; for, as I wished to make daily observations on the temperature of the interior of the glacier, I was obliged to cause the bore to be emptied every evening, which was a pretty long operation.

In this manner, a depth of 70 feet was reached. The piercer then began to become too heavy, and the chief borer proposed to me to construct a *trépied* above the hole, and to fix to it a pulley, on which a rope was to be passed, to which the piercer should be attached. He hoped by this means to bore more rapidly, and with less trouble. The operations were on this account suspended for several days, during which my people went to the valley of Hasli, to procure young firtrees for the scaffolding. When the *trépied* was prepared, they transported it to the glacier. It was now intended to

proceed with renewed ardour; but what was our surprise, when we were about to introduce the piercer into the hole, to find that it would no longer enter. We then remarked that the hole had become contracted by half an inch, and we had no other course left but to recommence anew. We required three days to reach the depth of 70 feet, at which we had left the bore. For my own part, instead of regretting this loss of time, I rejoiced at the circumstance, for I had thus obtained the most manifest proof of the dilatation of the glacier to a considerable depth. The contraction was not merely superficial, but extended as far as the bore did; and it could not be the result of the water congealed along its walls, for care had been taken to remove the little streamlets, and I had caused the few inches of water which accumulated at the bottom of the hole, in consequence of transudation, to be taken out every evening.

One day the workmen felt the piercer escape from their hands, and fall down two feet. They were then at a depth of 110 feet. It was evidently an internal cavity which had been encountered, and a certain quantity of air-bubbles were soon seen arriving at the surface, after having traversed the column of water which filled the bore. Unfortunately I was not near at the time, and the bubbles of air, whose nature it would have been interesting to ascertain, could not be collected. But the fact is of itself not the less important, because it furnishes us with the proof that there are cavities in the interior of glaciers at very considerable depths, and in places where the surface does not exhibit any trace of a large and deep *crevasse*. The splinters which were detached by the piercer, and which ascended to the surface, had the same appearance and the same hardness as if they had been detached from the wall of a *crevasse*. In two other cases, bubbles of air rose from the bore, without, however, the piercer descending suddenly. I have also seen them frequently ascend to the surface of hollows in the ice (*baignoires*), filled with water.

I was provided with three of Bunten's thermometrographs, which I placed every evening in the following manner: one in the great bore; a second in the hole less deep, generally at a depth of 15 feet; and a third in the air. I took care to close the openings of the two holes, so as completely to prevent the external air from influencing the temperature of the air contained in those holes. During the four weeks that these observations were continued, the temperature of the air at night never descended below − 6°.5 (+ 22°.1 F.); the minimum has even been + 2° (35°.6 F.) during the night, on one occasion. Of course, the streamlets of water on the surface of the glacier then continued to flow, and did so even when the temperature was at + 1° (33°.8 F.), or even at 0 (32° F.). In the interior of the glacier, the thermometrograph sometimes indicated 0 (32° F.), and then the bore remained moist, and water was accumulated at the bottom; sometimes it descended to − 0°.3 (31°.46 F.), and even to − 0°.5 (31°.1 F.); the sheath of the thermometrograph, or the cord which held it, was, in such cases, sometimes frozen to the bottom of the bore; and I was obliged to disengage it by pouring boiling water down along the cord. On the morning of the 15th of August, the thermometrograph, which had been during the night at a depth of 60 feet in the glacier, was at 0 (32° F.); the external air had been at − 3° (+ 26°.6 F.). On the 16th it was at the same, while the air had been at + 1° (33°.8 F.); on the 17th the thermometrograph indicated − 0°.5 (31°.1 F.), and the temperature of the air had descended to − 2° (+ 28°.4 F.). I quote these three observations because they were made at the same depth, during an interruption of the boring. I intend publishing afterwards all the observations made at different depths. I shall merely now add, that, on the night between 31st August and 1st September, I placed two thermometrographs in the same hole, the one at a depth of 15 feet, and the other at 125 feet, and that I found both at 0 (32° F.) in the morning. The result was the same during the day of the 1st September, and during the nights of the 1st and 2d and 2d and 3d September, at the depths of 15 and 120 feet; while the maximum of the external air during the day was + 8° (46°.4 F.), and the minimum of the two nights + 0°.7 (+ 33°.26 F.). During the day of the 3d September, I found the temperature at 0 (32° F.), at 15 and 120 feet in the same hole, the external air being at + 3° (37°.4 F.).

I had observed, that whenever the cold of the night was not very great, water was infiltrated into the bores; and on several occasions I found in the morning my thermometrographs submerged, although I had taken care to empty the bore completely the preceding evening, and although the introduction of water from above was prevented by little drains, and by the manner in which the aperture was closed. I saw at once the importance of this fact for the theory of dilatation, and I proceeded carefully to examine this accumulation of water. The following is the method I adopted. After having thoroughly emptied the bores, I measured from time to time the quantity of water that had been accumulated, by plunging into the hole a sounding line, which I kept stretched in such a manner as to rub against the sides of the hole as little as possible. The length which I found to be moist when I withdrew it, gave me the depth of the accumulated water. The following observations were made simultaneously in two bores, of which the one had a depth of 30 feet, and the other of from 120 to 140 feet. The first of these holes, of equal diameter throughout, had been bored with a *fleuret* of $3\frac{1}{2}$ inches in diameter; the large hole, on the contrary, went on decreasing in diameter from above downwards, having been bored to a depth of 92 feet with a *fleuret* 6 inches in diameter, from 92 to 110 feet with a *fleuret* of 5 inches, and beyond 110 feet with a *fleuret* of $3\frac{1}{2}$ inches.

Quantities of water accumulated in the small and large bores.

DATE.	SMALL BORE at a depth of 30 feet.	LARGE BORE at first at 120 feet, and afterwards at increasing depths to 140 feet.	Temperature of the air.	
			Minimum.	Maximum.
	ft. in.	ft. in.		
Night of Aug. 31 —Sept. 1.....	0 6	6 0	+0°.3 (32°.54 F.)	.....
Day of Sept. 1 ...	9 10	29 6	.....	+8°.7 (47°.66 F.)
Night of 1st—2d	15* 0	12 0	+0°.7 (33°.26 F.)	.....
Day of 2d.....	2 6	10 0	.....	+2°.5 (36°.5 F.)
Night of 2d—3d..	0 6	3 6	+0°.7 (33°.26 F.)	.....
Day of 3d.....	2 8	20 0	....	+3°.0 (37°.4 F.)
Night of 3d—4th	0 3	3 0	+1°.0 (33°.8 F.)	.....
Day of 4th.....	3 4	18 0	.....	+3°.6 (38°.48 F.)
Night of 4th—5th	1 8	4 0	−4°.0 (+24°.8 F.)	.....

* During this night the small bore remained open, and some water was introduced from the surface, whereas the large bore was closed.

It results from this table, that the quantity of water accumulated in the two bores was not only different, but was proportional to the surfaces of the bores; whence we conclude that it is by no means introduced from the opening at the top, but that it is exuded by the sides. If it were otherwise, the small bore would not on every occasion have contained a quantity of water so small compared with that accumulated at the same time in the large bore. The contrary would even have taken place, because the small bore was narrower than the large. The fact of the quantity of water being unequal on the different days is also not without its importance, for it proves to us that no fissure or canal existed which could place the bores in communication with any reservoirs of water; for, in that case, the accumulation of water would have been proportional to the interval of time comprised between the observations. There is only one conclusion to be drawn, which is, that the glacier imbibes water in unequal proportions at different hours of the day, at different external temperatures, and according as the air is dry or humid; or, in other words, that the glacier should be regarded as a spongy body of ice, which has imbibed a larger or smaller quantity of the water that circulates in its interior.

It is true, that, during the five days when the above observations were made, the temperature fell only once below 0 (32° F.) during the night, and that it remained most frequently above 0. We have had other nights during which the minimum has been at + 2° (35°.6 F.). We must not, however, proceed too rapidly to draw from this inferences adverse to the theory of dilatation, and above all we must not thence conclude that the glacier only imbibes as much water as there is at the surface. The state of the atmosphere no doubt influences the quantity of water which circulates in the glacier, but this influence is not such as to prevent water existing in the interior of the mass even when the temperature falls temporarily under 0 (32° F.). During the night of the 4th and 5th September, the temperature of the air fell to − 4° (+ 24.8 F.). On the evening of the 4th it snowed, and on the morning of the 5th the glacier was covered with six inches of snow, and nevertheless we have seen that, notwithstanding the cold, the

small bore collected that night 1 foot 8 inches, and the large bore 4 feet of water. The snow was perfectly dry, and did not begin to melt until 6 o'clock P.M.

If these facts were not sufficient to prove that water circulates in a liquid state in the interior of the glacier, I could cite others not less conclusive. I have often seen water exuding along the smooth walls of the *crevasses;* and it sufficed to wipe the moist place, to see a little drop of water rise to the spot over which I had passed my handkerchief. Lastly, it will afterwards be seen, that, on my descent to the bottom of a crevasse 120 feet deep, I found the walls of ice bristled in a multitude of places with small icy stalactites, four or five inches long, which evidently proceeded from the drops of water exuding at all these points.

It appears to me to be demonstrated, that water is diffused in four ways in a glacier:—

1. By hollows open at the surface, to which numerous rills of water resulting from the melting of the superficial ice contribute their supply. These hollows, opening widely to the surface, are *crevasses*, apertures of cascades, *baignoires*, and vertical tubes, at the bottom of which there are small fragments of rock. It is not necessary to give any other proof of this mode of diffusion than the following fact: Whenever the night has been very cold, the surface of these pools of water is frozen, but the water beneath is lowered by two or three inches, and covered by a second and thinner crust of ice reposing immediately on the water; sometimes, indeed, the water of cavities of small extent has entirely disappeared, although the plates of horizontal ice which cover these hollows indicate that there must have been water to the depth of a foot and even more. I have never, however, seen large bath-shaped hollows (*baignoires*), full of water, lower their level more than a few inches in the course of a night. I ought to add, that I have never met with a *crevasse* which went to the bottom, except at the extremity of the glacier.

2. By internal canals which circulate in the whole mass, like arteries of very various sizes, and which are seen terminating here and there on the walls of the *crevasses* and on the terminal edge of the glacier, where they sometimes form little

cascades and even jets similar to that of a fountain, as was well seen last year in the glacier of the Rhone; but most frequently their opening is very small, and is hardly a line in diameter; they then give rise to small stalactites, of which I shall afterwards speak.

3. By capillary fissures, which divide the whole mass into a quantity of angular fragments of various sizes. We may assure ourselves of this by pouring, as I have done, coloured liquids into cavities hollowed out at the surface of the glacier.

4. Lastly, it seems to me probable that the vertical bands of blue compact ice, alternating with bands of white porous ice, and which I shall discuss immediately, maintain a continual infiltration of water in the mass of the glacier wherever this structure is observable.

The demonstration of the presence of water in the entire mass of the glacier, at all depths, is the most important of all the facts obtained during my residence last year on the glacier of the Aar; and I am so much the more interested in bringing this clearly out, because it is upon the absence of water at great depths, that many authors, and especially Mr Hopkins, in his essay entitled Theoretical Investigations of the Motion of Glaciers, found, in a great measure, their reasoning against the glacial theory. Without pretending that all the difficulties can be removed by the discovery of this single fact, it will at least be conceded that it has enabled us to advance a step further towards the solution of the problem.

I now come to a no less important fact, that of the lamellar structure of glaciers. This remarkable phenomenon, which Professor Forbes has described extremely well in the Edinburgh New Philosophical Journal,* but of which he has erroneously claimed the discovery, and has assigned to it a generality which the facts he has himself observed did not at all justify, was for the first time observed in 1838 by Professor Guyot of Neuchâtel, on the glacier of the Gries, at a height of 7500 feet; the first naturalist made it the subject of a highly interesting communication to the Geological Society of France,

* For January 1842, vol. xxxii. p. 84.

assembled that same year at Porrentruy. I had myself, likewise, remarked this phenomenon on the Glacier des Bois, during an excursion which I made at the same period, and afterwards on the glacier of the Aar; but it presented itself nowhere with that striking distinctness we witnessed last year; and it is on this account that, like several other facts which continued observations could alone elucidate, it is only mentioned in a vague manner in my *Etudes sur les Glaciers.*

During the months of August and September 1841, this phenomenon was so well developed in the glacier of the Aar, that it could not fail to strike every observer, especially in the environs of the Hôtel des Neuchâtelois, at a height of 7500 feet, and at a distance of two leagues from the extremity of the glacier. I shall now endeavour to describe it, and to give an account of the researches carried on with a view to ascertain its extent and modifications.

It is known that in general the ice of glaciers differs from ordinary ice, by having a network of capillary fissures, which seems to penetrate its whole mass, and is the result of the transformation of the névé into ice. But this ice is, nevertheless, of great compactness, and though rough at its surface where it is not covered by a moraine, that appearance is only superficial; and it suffices to remove the external crust, to find the ice as compact as under the moraine, and at every place where it is sheltered from evaporation. If, then, we examine this ice, we find that it is by no means uniform, but, on the contrary, that it is composed of laminæ or vertical bands of variable breadth, being generally from ½ to 1, 2, and 3 lines, but sometimes also from 1 to 2 and even from 4 to 5 inches broad. One set have a bluish tint, and a very compact and homogeneous texture; and the others have a white colour, their ice being less hard, and having a snowy appearance, in consequence of the quantity of bubbles of air with which it abounds. The whole may be compared to a mass of glass, composed alternately of dull bands with air-bubbles, and bands which are perfectly transparent. This particular arrangement of blue bands alternating with white ones, is particularly apparent in the walls of the *crevasses*. The bands pass

from one wall to the other, and can be traced for spaces of hundreds of yards, always preserving a perfect parallelism. They are to be seen descending in this manner into the mass of the glacier, as far as the eye can reach into the *crevasses*, sometimes to a depth of ten and fifteen metres (yards). Their direction is generally parallel to the axis of the glacier; but they are not always rectilinear; for I have seen on the glacier of Aletsch and elsewhere, contortions and dislocations of different kinds, and M. Guyot remarked the same thing in the Gries glacier. Although the observations made at the beginning of our residence on the glacier of the Aar induced me to believe that this lamellar structure extended to a large portion of the mass of the glacier, I was nevertheless desirous to assure myself, in a direct manner, that it did so, and with this view, I descended into one of those hollows or pits in the glacier, where most of the streams which trace a serpentine course on its surface are swallowed up. I was thus enabled to follow the blue and white bands as far as the accumulation of water at the bottom of the hole permitted me to descend, that is to say, to a depth of 120 feet.

The following is a short account of the descent, which my travelling companions afterwards denominated my *descente aux enfers*:—

It was towards the termination of our residence on the glacier; we had finished our boring, and were preparing to depart, when, while discussing, according to custom, the phenomena we had observed, one of the party remarked that it would perhaps be easy to descend without danger into some one of the pits of the glacier, and that perhaps some unexpected appearances might thus be observed. All were of this opinion, and without delay we commenced looking for a pit suited to the purpose. These pits, as I have remarked in my *Etudes sur les Glaciers*, are probably old crevasses, which a small stream of water has prevented from being completely closed; so that instead of being of an elongated form, they are, on the contrary, for the most part circular, and the rivulet, far from contracting them, tends to enlarge them more and more, especially when it is considerable. We found at some distance

from our hut one of these pits, which seemed well adapted for our object; it had an opening of eight feet in diameter, and appeared to penetrate vertically to a great depth. I resolved to descend into it; but to accomplish this it was necessary to begin by turning off the stream by cutting another bed for it in the ice. We set all hands to work, and when the new bed was formed, I sent my men to procure the *trepied*, which had been used for the boring operations, and placed it over the pit. A board on which I was to sit was fixed at the end of the rope, and I was secured to that rope by a strap, which passed under my arms, so that my hands were left free. In order to protect me from the water, which we were not able to turn off completely, the guides covered my shoulders with the skin of a goat, and placed on my head a cap made of a marmot's skin. Thus accoutred I descended, provided with a hammer and a staff. My friend Escher was to direct the descent, and for this purpose he lay down on his face, with his ear hanging over the side, in order the better to hear my directions. It was agreed that so long as I did not ask to come up, I should be allowed to descend as far as the distance at which M. Escher could distinctly hear my voice. I reached a depth of 80 feet without encountering any obstacle, observing attentively the lamellar structure of the glacier and the small stalactites of ice of which I have already spoken, and which were attached on all sides to the walls of the pit. These stalactites were from 2 to 5 or 6 inches long, and only a few lines in diameter; and they were bent like hooks fixed in the walls. It was evident that they were produced by an exudation from the walls of the pit; for if they had resulted from the water falling from the surface of the glacier, they would not have been so uniform nor so equally distributed over the whole surface of the sides. Those which were really derived from the cascade of water from above were much larger, were more closely united to the wall of ice, and were, moreover, limited to one of the surfaces of the passage. The bands of blue ice became perceptibly broader as I descended; they were less sharply marked than above, and the remainder of the mass, of an inferior degree of whiteness, was less distinctly contrasted with the intermediate deeper coloured laminæ. At a

depth of about 80 feet I encountered a ridge of ice which divided the pit into two compartments, and I endeavoured to enter the widest; but I could not penetrate more than 5 or 6 feet, because the passage became divided into several narrow canals. I caused myself to be raised up, and managing so as to make the rope deviate from the vertical line, I got into the other compartment. I had observed in descending, that there was water at the bottom of the pit, but I supposed it to be at a very great depth; and as my attention was especially directed to the vertical bands, which I continued to trace, thanks to the light reflected by the brilliant walls of the ice, I was very much astonished when I suddenly felt that my feet were immersed in water. I immediately directed myself to be drawn up, but the order was misunderstood, and in place of ascending, I found that I was descending. I then uttered a cry of distress, which was heard, and I was raised up before being obliged to have recourse to swimming. It seemed to me as if I had never in my life encountered water so cold. Fragments of ice floated on its surface, which no doubt were broken portions of stalactites. The walls of the pit were rough to the touch, and this was doubtless caused by the capillary tubes.

I should have wished much to remain a longer time to examine the details of the structure of the ice, and to enjoy the unique spectacle presented by the blue of the sky, as seen from the bottom of the abyss; but the cold obliged me to ascend as soon as possible. When I reached the surface, my friends told me of their anxiety for my safety when they heard my cries, and that they had experienced the greatest possible difficulty in drawing me up the pit, although they were eight in number. I had, however, reflected but little on the danger of my position. Perhaps, if I had known it previously, I would not have exposed myself to it; for, if one of the large pointed cones of ice lining the walls of the cavity had been detached by the rubbing of the rope, and had struck me in its descent, my destruction would have been certain. I would, therefore, advise no one to repeat the experiment, unless it should be for some important scientific purpose.

I was thus enabled to follow out the lamellar structure of the glacier, not only as far as the first interruption of the canal, that is to say, to a depth of about 80 feet, but even, although less distinctly, to the bottom of the hole. I therefore consider myself entitled to conclude that the laminæ traverse completely the glacier properly so called, becoming more and more blended in its mass, whereas they exhibit very different phenomena in the higher regions, as I shall afterwards notice. Everything leads us to believe that the phenomenon of the lamellar structure is connected with the infiltration of the water into the mass of the glacier; and as we have seen, by the experiments mentioned above, that water exists in a liquid state as far as we have yet been able to penetrate, and that, on the other hand, the lamellar structure extends also, according to all appearance, through the entire mass, it must be by observing in a continued manner the mode of the infiltration of the water in the ice, especially at places where the lamellar structure begins to present itself, that is to say, at the limit between the névé and the glacier, that we can hope to attain the explanation of this important phenomenon. I propose to prosecute these observations during the sojourn I expect again to make this year on the glacier of the Aar. At the same time I beg those naturalists and natural philosophers who may visit other glaciers, to direct their attention to this subject, being persuaded that they will deduce important results, not only for the study of glaciers, but for physics in general.

I ought here to mention an experiment which I made with the view of ascertaining the modifications to which the different bands of this ribboned ice are subjected by the influence of the air. At a spot where the lamellar structure was very distinct, I uncovered a space about a foot square under the moraine, taking care to clean the surface well. We observed attentively the action which the air exercised on this surface newly exposed. At first the blue bands began to grow pale insensibly; fissures seemed to be formed in their interior; but I rather think that they merely became visible by the displacement of the air contained in the mass, and which is often

n bursting out in small bubbles at the surface. At the end of

half an hour the blue bands could only be vaguely distinguished from the white ones; the surface had assumed a very rough aspect; some hours afterwards it had even become entirely porous to the depth of about an inch; the lumps which were formed had become mobile, without, however, being disaggregated; the differences of tint had then completely disappeared, and the ice appeared composed of unequal but homogeneous fragments. Nevertheless, by moistening largely with water this unequal surface, the ribboned arrangement and the tints of defined colour reappeared immediately, and anew gave to the ice the lamellar aspect which it had originally. When the influence of the atmosphere was prolonged for a considerable period, the white bands at last became completely disaggregated to the depth of about a foot, and were then transformed into small grains perfectly similar to those of the *névé*, whereas the blue bands only appeared between these beds of grains as projecting edges soiled by the dust which lodges on glaciers. It is in the white bands, which are originally hard, but have become granular in consequence of the external temperature and the melting, that the small rills of water collect, which wind over the surface of the glacier. But if the glacier should be inundated by a violent rain, on the following day its blue and white tints at the surface contrast anew with a striking intensity. This, at least, was what we saw take place last year, and it remains to be ascertained to what extent this reiterated phenomenon of disaggregation and of congelation is reproduced at different seasons, and under influences of various kinds, and what are the circumstances which render the whole more or less apparent at the surface of the glacier. It is probable, according to what has just been stated, that the white bands are a *névé* impregnated with water and congealed, whereas the blue bands appear to me to be bands of ice of liquid water transformed directly into homogeneous ice.* It is difficult to determine

* It would be interesting to ascertain if the same thing takes place in ice masses, containing piled up ice which has been sprinkled with water.

the cause which determines this linear and continuous disposition of these vertical laminæ, but it seems to me that the first impulsion of this structure is caused by a molecular movement of the névé soaked with water, and purified by the running of the water according to the line of the greatest slope. If this were once established, it would not be at all extraordinary, that, in the lower part of the glacier, and notwithstanding the changes its course might undergo, this original disposition should be maintained by the direction which it would impress on the water that infiltrates into its mass by penetrating from the surface to the interior. It is at least only in this way that I can explain the more or less considerable obliquity of the laminæ on the outside of the moraines, and at the edges of glaciers where the largest quantity of water circulates; and it is only by this means that we can explain the sinuosities and the contours that they form, when the masses are displaced from their natural direction during the long course of their descent from the high regions towards the extremity of the glaciers. It is well worthy of remark, that the stratified fields of snow of the highest slopes of the Alps do not present any trace of this lamellar structure, and that in the *névé*, where it passes into glacier,* this structure is superficial, or only penetrates a few feet into the mass; it is only in the main course of the glacier that it seems to me to be well developed and to penetrate very deeply; at the lower extremity it gradually disappears. It still remains to be determined if traces of it exist at the inferior surface of the mass of the glacier.

* I only wish to describe in this article the facts relative to glaciers properly so called, because these are more immediately connected with the theory of progression by infiltration. It would have led me too far to have detailed the new observations which have been made on the fields of snow of the highest regions, and on the various phenomena presented by the *névé*. M. Desor and I have endeavoured to delineate the limits of these three regions (the fields of snow, the *névé*, and the glaciers), on a map of the glaciers of the Bernese Oberland, which accompanies the narrative of our ascent of the Jungfrau, by M. Vogt. (Jent and Gassmann, Soleure, 1842.)

The movements of glaciers, and more especially those of the lower glacier of the Aar, have for several years been the object of my continued attention. In my *Etudes* (p. 150), I have stated the progress of this glacier from 1827 to 1840, which, after taking into account the corrections lately made by M. Hugi, as to the distance of the hut from the *Abschwung*, would amount to about 220 feet per annum. In August 1840, the Hôtel des Neuchâtelois was 2457 feet from the angle of the rock called the *Abschwung*, which separates the branch of the Finsteraar from that of the Lauteraar. I took care to inscribe this measurement on one of the walls of the block. The following year, on arriving at the Hôtel des Neuchâtelois, my first proceeding was to measure the distance anew, and I then found it to be 2623 feet. The block had therefore advanced 166 feet. This new measurement was likewise inscribed on the block, and I am sure that on again visiting the glacier of the Aar this year, I shall find that the block has advanced about an equal distance. This is what any traveller may ascertain who happens to be at the glacier before me. If it be kept in mind that this annual progression takes place at a point where the glacier is very little inclined, inasmuch as its slope is scarcely 3°, and that at the same time the upper masses, and especially the branch of the Lauteraar, are very uniform, and do not present any trace of a rapid movement, it will be understood that a sliding movement, such as is supposed by many authors, is not at all probable. It suffices, moreover, to have seen the localities, in order to be convinced that at that point the glacier could never have advanced otherwise than by a slow and continuous movement, caused by infiltration and the daily congelation of the water which penetrates the whole mass.

These measurements ought further to establish, in another manner, the evidence of the movement by dilatation. It has been reasonably said: If it be the infiltrated water which, by becoming dilated, occasions the progression, it would not only be necessary that the whole mass should advance, but that the distance between two given points on the glacier itself should also be augmented; the same thing ought to occur

here as in a mass of dough, which is made to ferment, and all of whose parts become dilated. Now, this is precisely what takes place in glaciers, as I have been enabled to infer from the distance which separates the block under which we lived, from another large block situated lower down, and known by the name of Hugi's Hut (*Cabane de Hugi*). The two huts are placed on the summit of the great medial moraine, consequently in the middle of the glacier, and under precisely similar conditions. When I established myself on the glacier in the month of August 1840, the distance from the *Hôtel des Neuchâtelois* to the *Cabane Hugi* was 1890 feet; in the month of August 1841, the distance amounted to upwards of 2000 feet; hence it had increased by about 110 feet.* On the other hand, we have just seen that the distance of the *Hôtel des Neuchâtelois* from the *Abschmung*, had been augmented during the same year by 166 feet in 2457; whence it results, that the progression was about the same in the two spaces (166 feet in 2457, and 110 in 1890). Last year, before quitting the glacier, I took, along with my friend M. Escher, other measurements, which will serve, I hope, to confirm these results. We ranged in a line, at three different places, a series of stakes, whose positions corresponded exactly with fixed points on both sides of the glacier. As these stakes are all inserted to a depth of 10 feet, I hope to find them still erect this year. It is probable that they will no longer be in line, and those which have advanced the farthest in the direction of the slope of the glacier, will indicate the portions of the glacier which have progressed most rapidly.

But the glacier was not only dilated in the direction of its slope, it was also considerably swollen, especially in the branch of the Finsteraar, at some distance from our hut. This swelling was so visible, that on approaching it for the first time, in the month of August last, every one was struck, the guides

* I cannot give these numbers exactly to a foot, owing to the deficiency of our means of measurement; but if there be an error, it cannot, at all events, be considerable, as we measured the distances with a rod.

as well as ourselves; and I believe that in calculating the swelling at about 10 feet, we are far under the truth. It was quite close to the place where, in the preceding year, I had inserted two poles in the bores, the one to a depth of 9 feet, and the other to a depth of 20, for the purpose of ascertaining if the glacier really rejected foreign bodies. I burned with impatience to discover what had become of my two poles. We found both erect; but having measured them, I ascertained that they had been elevated 7 feet; so that the longest was only sunk to a depth of 13 feet, and the other would scarcely stand in the hole, which was not more than a foot and a half deep. The contact of the wood had also sensibly enlarged the two holes, more particularly the second.

At first sight the matter seems quite simple, and it might be said that it was the level of the glacier which had been lowered by the melting. But we must not forget that these poles were planted on the portion of the glacier which had become most swollen since the preceding year, and this necessarily complicates the question, for how can we conceive at once a swelling of 10 feet and a lowering of 7 feet? Ablation had, nevertheless, actually taken place; and if, notwithstanding this ablation, the glacier became swollen as it progressed, that could only be because the augmentation of volume, resulting from the congelation of the water accumulated in the interior of its mass, had been more considerable than the volume of ice removed at the surface by the melting and the evaporation. A similar occurrence ought to present itself whenever the summer is rainy, as in 1841, for then the evaporation is less considerable, while an enormous mass of water is infiltrated daily into the glacier. We found, in fact, all the glaciers of the Oberland in a state of increase; and in all places they threatened to invade the pastures.

The experiment which I have now mentioned was also made by M. Escher de la Linth on the glacier of Aletsch; and the results he obtained were still more striking as to the rapidity of the disappearance of the ice. M. Escher, with the view of verifying the opinion I had expressed on the metamorphism of glaciers, proceeded in the course of the month

of June last year to the glacier of Aletsch, taking with him an ample provision of stakes, which he had caused to be prepared in the Valais. He described, by means of these stakes, planted at equal distances from one another, a triangle placed in relation to several fixed points at the sides; and he hoped by this means to arrive at a knowledge of the movements of the glacier, not only in the direction of the longitudinal, but also in that of the transversal axis. In order that the winds and storms might not knock over his stakes, he sank all of them to a depth of four feet. What was his surprise when, having gone about the middle of August to visit his triangle, he found that most of the stakes were down, and that those which were still erect did not penetrate more than half-a-foot into the ice! Twelve days afterwards (the 28th August), when I ascended the same glacier on my route to the Jungfrau, I found only one of the stakes erect. Thus two months were sufficient to remove a bed of 3½ feet in thickness from the glacier of Aletsch; and notwithstanding, M. Escher assures us that no difference in the level of the surface was perceptible.

These experiments were so important that I resolved to continue them. Foreseeing that, in the following year, the bore which I had pierced to a depth of 140 feet would be closed, or at least so much contracted as to be no longer of any use to me, I determined to employ it for a similar experiment. I therefore took fourteen wooden cylinders, each a foot long, and having a somewhat smaller diameter than that of the bore. I numbered them carefully, and having sunk No. 1 to the bottom of the hole, I covered it with a bed of gravel of 9 feet. I then introduced cylinder No. 2, which I covered in the same manner with a column of gravel of 9 feet, and so on in succession; so that in this manner the entire bore contained all the fourteen cylinders, separated from one another by columns of gravel 9 feet long. The fourteenth was 1½ foot under the surface of the ice on the 6th September 1841. As the position of the hole was exactly determined and easy to find, we can easily ascertain what has been the amount of ablation in a given time. Supposing that this ablation

is on an average only 5 feet in the year, and that the glacier continues to advance in the same proportion—that is to say, about 200 feet annually—the cylinder No. 1 ought to reach the surface in about 28 years, and at a distance of 5600 feet from the place where it was inserted. In this calculation the inequality of the rapidity of the beds is certainly not taken into account, and which, according to the most recent researches, must be less considerable than I at first supposed.

The second bore, having a depth of 30 feet, was employed for an experiment from which I expect still more important results. We have seen, from the general view I have already given of the temperature of the interior of the glacier, that this temperature varied but little during the whole period of our residence on the glacier. It was important to ascertain if this was also the case during the colds of winter, when the glacier is covered with a thick bed of snow. I therefore resolved, with this view, to make use of my three minimum thermometers. I placed two in the bore, and suspended the third freely in the air at the top of the *trepied*. In order to preserve these instruments from the influence of the water, and that I might not have difficulty in raising them afterwards, I had a tin-case made, which was 2½ inches in diameter and 24 feet in length, and which I sank in such a manner that its upper end was a foot below the surface of the ice. I introduced the first thermometer into the bottom of the case, and placed the second at 12 feet higher up; so that the latter was at a depth of 13 feet, and the first at a depth of 25 feet, in the glacier. In order, moreover, to protect my instruments from the possibility of being pressed by the walls of the glacier, I took care to include each of them in a thick sheath of felt. Each instrument is coated with tallow in its sheath, and the tin case which contains both is also equally well closed, so as to prevent the access of water. By this means I hope this year to obtain in these lofty regions the minimum temperature of the glacier, at depths of 25 and 13 feet, and that of the air at 12 feet above the surface. I intend going to obtain the results whenever the snow has disappeared from the surface of the glacier.

MODE OF LIVING ON THE GLACIER.

I cannot conclude this article without saying a few words on the kind of life we passed at the *Hôtel des Neuchâtelois*; for I do not suppose that a correct idea can be formed of it without possessing an exact knowledge of glaciers. At first sight there is something extraordinary in the notion of a prolonged sojourn in the midst of a sea of ice, at a height of 7500 feet. It may naturally be imagined that the cold must be excessive, especially at night, and I have frequently been asked how I managed to escape being frozen. The fact is, that it freezes there almost every night, and the thermometrograph often indicated in the air — 4° and 5° (and + 24.8 and 23° F.), and even — 6½° (+ 22°.1 F.). But we were well provided with coverings; and as our abode was sufficiently small, the respiration of five or six individuals sufficed to maintain a tolerable temperature.

Notwithstanding its pompous name, the *Hôtel des Neuchâtelois* is, in reality, but a very small hut about 12 feet long by 6 broad, and 4 high, where its height is greatest. I have already said, that this cabin is situated on the moraine; it has pure ice for its foundation, on which the broad stones of the moraine have been placed so as to form a sort of flooring. A bed of herbs gathered on the sides of the glacier served as a mattress; and to protect ourselves from moisture, we took care to make use of a double covering of wax-cloth. The latter is a precaution which it is important to take, and which I cannot sufficiently recommend to those who wish to live on glaciers; for there, as elsewhere, humidity is much more to be dreaded than cold. As our hut was merely formed of a dry stone wall, we endeavoured to guard against violent winds, by stopping up the interstices with bunches of grass. It nevertheless happened frequently, in spite of our precautions, that a hurricane (*Guxen*) blew fearfully through the wall. As, however, we were generally fatigued by our exertions during the day, we did not sleep the less soundly.

It was the rainy and snowy nights only which were really

disagreeable; for as the large block which served as a roof was fissured throughout notwithstanding its enormous thickness, the water penetrated by the fissures, and streamed along its lower surface. Whenever one of these little streamlets encountered an inequality, a cascade was formed which awoke in an annoying manner those who happened to be under it. Sometimes one, and sometimes another, was then seen rising up, and seizing a candle, endeavouring with his finger to give another direction to the troublesome rill. But soon recovering its first direction, it would proceed to moisten the person to the right or left, and thus rouse him by dropping provokingly into his ear or mouth. The unfortunate individual would then get up in his turn, and try to correct the course of the water, or probably send it to sprinkle his companion near him. I remember one night when the rills of water and the cascades were so abundant, that all change of direction was useless; and seeing that it was impossible to shut an eye, we began to amuse ourselves at the expense of our cascades, by communicating to them all sorts of directions. In place of sleeping, we pursued hydrographical studies.

In order to inure ourselves to the cold, several of the party adopted the habit of bathing the body every morning in iced water, in a large tub which the guides placed every evening before the door of the hut, and which in the morning was often covered with ice half an inch in thickness. At first this practice seemed severe, but we soon became accustomed to it, and did not wish to give it up; for after the first disagreeable sensation was surmounted, we were sure to feel warm, and could wear our ordinary dresses with impunity; whereas those who dreaded these icy baths, and did not make use of them, shivered around us enveloped in their cloaks.

Our chief guide, Jacob Leuthold, who was also at the same time our chief cook, arrived between four and five o'clock to prepare breakfast, which generally consisted of a cup of chocolate. When we had finished, the pot was replaced on the fire for the breakfast of our guides, which was cheese-soup. Our first occupation was to visit the thermometrographs and the barometers; and when the sheath of one or other of these

instruments was frozen to the walls of the hole, it became necessary to employ hot water to detach it; an operation which took up a considerable time. Except on rainy days, the boring could not be commenced before eight o'clock, for it was necessary to wait till the rills of water again began to flow; the work was then carried on till midday. Those who were not occupied with the borers made an excursion to some neighbouring summit, or visited one of the numerous moraines which descend from the flanks of the valley; and as I had taken with me a landscape painter, M. Bourkhart of Neuchâtel, to delineate the most remarkable phenomena of this *mer de glace*, in a scientific and picturesque point of view, I often accompanied him to point out the places most worthy of attention. Most frequently, it was not so much the special object we followed, as the unexpected observations we made, which gave importance to these excursions; and at last we did not fix on any particular plan, but simply walked to some summit, or to some lateral glacier, with the anticipation of reaping an ample harvest of new observations. The numerous facts of detail which were collected in the course of these various excursions, cannot come within the compass of a mere article in a journal; but I intend soon to publish a summary of them in a supplementary volume to my *Etudes sur les Glaciers;* and I hope that they will contribute more and more to increase the interest which attaches to glaciers, by initiating naturalists into the minute history of a natural phenomenon, which, with all its apparent uniformity, is yet so splendid and so varied. What indeed can be more interesting than this series of metamorphoses, to which frozen ice is subjected, from its fall on the high summits in the form of snow or of small hail (*gresil*), to its transformation into those masses of compact ice which descend like immense solid *coulées* into the midst of our forests, and of our cultivated fields! What can be more worthy of attention than the study of the red snow, that microscopic creation which extends as a rose-coloured tint over immense spaces of the névé, in places where traces of organic life could hardly have been expected to exist! What, moreover, can be more curious than to be able to follow for great distances the junction of the glacier and the rock, to see in some degree the former in the act of polishing and furrowing the walls of its bed, and to study the various effects which all this produces on the rocks, according to their hardness and the nature of their composition! Lastly, let me say, that it was here that we first observed the remarkable phenomenon of the *niveaux des roches polies et moutonnées*, which will enable us henceforth to determine the greatest thickness of the ancient sheet of ice which covered our country at any given point in the Alps, and that with a precision so much the more rigorous, because the traces the ice has left of its action on the rocks on which it has operated, are indelible, and form a striking contrast with the rugged and angular rocks which surmount them.

But let us return to our hut. Mid-day approaches, the whole party are re-assembled round the kitchen fire, and each one brings with him an appetite with which, for sharpness, that experienced on the plains cannot be compared. Although, therefore, the fare was but little varied, all agreed that it was a real enjoyment to dine in the open air at the *Hôtel des Neuchâtelois*, round the large flat block of gneiss which served as our table. We had little else to eat but mutton and rice, but whether it is that the mutton of these high mountains is really better than elsewhere, or that the sharp air renders the palate less fastidious, it is certain that we never tired of it. Sometimes we had for variety some goat's meat, which we likewise found excellent. A cup of coffee and a cigar were the necessary adjuncts to our dinner, and it seemed to us that both the one and the other had a more exquisite flavour under the sky of the Schreckhorn and the Finsteraarhorn. This was the hour for lively conversation, animated discussions, and the proposal of daring projects. After dinner we all returned to our occupations, one in one direction and another in the opposite; or perhaps we remained at the table to write out our notes and observations. The evening then arrived for the most part more speedily than we could have wished. After the little rills of water on the surface of the glacier began to be dried up, which, in serene days, generally took place between four and five o'clock, the boring was

stopped, the holes were emptied of water, and the thermometrographs were introduced, operations which continued till nearly seven o'clock. We then assembled anew round the kitchen; but at that time, although not less hungry than at dinner, we remained a much shorter period at table, for it was much colder, the temperature being then generally about 0 (32° F). The supper being over, we hastened to enter the hut; the light dresses of the day were exchanged for good cloaks and furs; and when night arrived, we closed the curtain which served as a door, and lighted the candle. The guides returned to their habitation on the left bank of the glacier, and all slept quietly on a place of repose which under any other circumstances would have been thought detestable.

In this manner we passed altogether about a month on the glacier of the Aar, without including the excursion to the Jungfrau, which took place on the 28th of August. During all this time we had a crowd of visitors, who were curious to see an establishment of so novel a kind.* Others were attracted by a more elevated motive, the desire to participate in our labours, or to testify by their visit the interest which they took in the investigations we were prosecuting; and, as the happiness which we experience at meeting with persons for whom we entertain a profound veneration, or with whom we are on terms of sincere friendship, was heightened by the beauty of the locality, I felt my heart beat with joy whenever I recognised a friend among the travellers who arrived along with the carrier of provisions about eleven o'clock in the morning. I am proud to be able to name among the number of those who visited me or lived with me at the *Hôtel des Neuchâtelois*, General Pfuel, governor of Neuchâtel, who, notwithstanding his age, performed on foot the arduous journey across the glacier, being unwilling to be outstripped even by the youngest; Lord Enniskillen, whose zeal does not give way before any fatigue, when the progress of science is in question; MM. Adolphe and Alfred de Rougemont, whom we are always sure to meet whenever any object of utility connected with Switzerland is concerned; my excellent friend Professor Studer of Berne; Mr and Mrs Trevelyan; Messrs Guyot, Robertson, Cole, Nicholson, Martins, Canson, &c. Lastly, I have already had occasion to mention among the number of those who took an active part in my labours, M. Escher de la Linth, as well as Messieurs Desor, Vogt, Forbes, and Heath. All shared my habitation at the *Hôtel des Neuchâtelois*; and I took care to have all their names cut on one of the surfaces of the large block which served us as a shelter.

I had intended again to have encamped this year under this same block, with which so many recollections are associated in my mind; but my guides have dissuaded me, by asserting that there would be danger for the colony in doing so, owing to the block being so much fissured. I have, therefore, given orders for the construction of a tent at the side of the old *Hôtel des Neuchâtelois*; and it is there that I hope next June and July to welcome all the friends of glaciers who may choose to pay me a visit.

* Among the tourists who visited us, several came from Grindelwald by the Strahleck; these, it may be well supposed, were much fatigued on arriving at the *Hôtel des Neuchâtelois*; and I had pleasure in receiving them as well as possible. One evening, when, being assembled together round our hut, we were amusing ourselves by observing the effects of light on the flanks of the Finsteraarhorn, we saw a numerous caravan issuing from the angle of the Abschwung. Immediately our glasses were pointed in that direction, to try if we could recognise among the travellers any one of our acquaintance. One of the party was in a *chaise à porteurs*. We all expressed our surprise at this singular mode of travelling; but what was our astonishment to discover that this traveller was a lady. Our irony was at once converted into admiration. Was it possible that a lady had crossed the Strahleck! She was certainly the first who had attempted so difficult a route. It turned out to be an English lady, Mrs C—— of Edinburgh, accompanied by her husband and nine guides. We proceeded to meet them. The lady was very young, and seemed very timid; and Mr C—— informed us that she had nevertheless performed the greater part of the journey on foot, but that her shoes had been torn, and it became necessary for her to be carried, which gave her great annoyance. I invited her to take a little repose, after which the caravan continued its route towards the Hospice of the Grimsel, where it arrived at nine o'clock.