"Glacial Theories"

Quarterly Review. July 1863.

by Dean Trench in his valuable work on the Miracles of our Lord.*

We would, in conclusion, once more draw attention to the great dearth of trustworthy information on the subject of the Natural History of Palestine. Of the geology of that country we know next to nothing, so that here is a wide field full of fruitful promise. What fish swim in the Jordan and in the Sea of Galilee? How far is the ichthyological fauna of Palestine identical with that of the fresh waters of Syria, described by Heckel ('Süsswasser-Fische Syriens, in *Abbild. u. Beschrieb. neuer u. seltener Thiere u. Pflanz. in Syrien*,' &c., von Kotscky Fenzl. Heckel u. Redtenbacher, 1843)? We recommend these remarks especially to the consideration of Mr. Tristram, to whom we are already much indebted for his contributions to our knowledge of the ornithology of the Holy Land; and we trust he will forgive us if, in the cause of science, we venture to express a hope that his health will shortly again require a little change of air, and that Palestine will be the country visited.

ART. III.—1. *Voyages dans les Alpes, précédés d'un Essai sur l'Histoire Naturelle des Environs de Genève.* Par M. de Saussure, Professeur emérite de Philosophie dans l'Académie de Genève. Vols. I., II., III., IV. Published in 1779, 1786, 1796. Neuchâtel.

2. *Théorie des Glaciers de la Savoie.* Par M. le Chanoine Rendu, Chevalier du Mérite Civil. From the 'Mémoires de la Société Royale Académique de Savoie.' Vol. X. 1841.
3. *Essai sur les Glaciers, et le Terrain Erratique du bassin du Rhone.* Par M. Charpentier, Directeur des Mines du Canton de Vaud. 1841.
4. *Système Glaciaire, ou Recherches sur les Glaciers.* Par MM. Agassiz, Guyot, et Desor. 1847.
5. *Travels through the Alps of Savoy; with Observations on the Phenomena of Glaciers.* By Professor James D. Forbes, F.R.S., F.G.S., &c. Edinburgh. 1843.
6. *Occasional Papers on the Theory of Glaciers.* By Professor James D. Forbes. 1859.
7. *Various Memoirs on Glacial Theory, in the Transactions of the Royal Society*, 1862, *and of the Cambridge Phil. Society.* By W. Hopkins, M.A., F.R.S., F.G.S.

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* 3rd edit., p. 433.

8. *The*

8. *The Glaciers of the Alps.* By Dr. Tyndall, F.R.S., Professor of Natural History in the Royal Institution of Great Britain, and in the Government School of Mines. 1860.

WHOEVER has been in the habit of contemplating the beauties of the works of nature or of art must necessarily feel the importance of regarding them from the best points of view. In the grander scenes of nature, for instance, when seen from an ill-chosen point, the mountain may appear too dominant, the expanse of water too large, or the distance too insignificant; and, however grand or beautiful each object may be individually, the general effect may be unsatisfactory to the cultivated eye. We must seek for that point in which every object appears in its due proportion, and helps to produce that general harmony in which the highest beauty of nature and art essentially consists. And so it is with science. In the earlier periods of the development of any complicated branch of knowledge, its several parts will frequently appear more or less disjointed, out of keeping with each other, and wanting in that more perfect harmony which is the surest test of truth in science, as well as the highest result of the beauties of external nature. Still Time, the great arbiter in such matters, gradually asserts his influence, and a period arrives at which we may be enabled to form at least a fair approximate estimate of the relative merits of the various conclusions in an advancing science. Such appears to us to be at present the state of Glacial Science. We have hitherto abstained from taking much part in the discussion of the subject, notwithstanding the popular and scientific interest which has been justly attached to it, not merely on its own account, but also on account of its important bearings on certain conclusions of geologists. We believe that premature criticism has been bestowed upon it; and if we now present a view of it somewhat different from those which have hitherto been more frequently advocated, we trust that we shall be regarded as doing so, not from partiality or prejudice, but on account of the present more perfect development and altered phase of the science.

Most of our readers will probably have formed some more or less distinct conception of a glacier; but we think it advisable to preface our examination of the various views which have been put forward on the subject, by a very general and brief description of those curious masses of ice and of the mode of their formation. If a mountain be of sufficient elevation, the temperature in its higher portions may be always below the freezing temperature, in which case the aqueous vapours which rise in the atmosphere above the summit of such a mountain, and subsequently fall upon it, must usually assume the form of snow; and thus it is that continual additions of snow are supplied to these elevated regions. At the same time there are several causes which restrict this increase within determinate limits. Thus the high winds which frequently play about the tops of the mountains are often the means of scattering thence a certain quantity of snow, and depositing it at lower levels. The frequency of avalanches, also, in the higher regions is well known to the mountain traveller; and another cause for the diminution of these elevated masses of snow is to be found in the radiating heat of the sun, which thaws a certain superficial portion of the mass on which it falls, and thus affords an effective aid in counteracting the tendency to indefinite accumulation.

The water produced by this melting sinks into the porous mass of snow; and, since the temperature of the mass must necessarily, at the elevations we are here contemplating, be less than that of freezing, the percolating water will be, at least, partially frozen, and will constantly tend to convert the mass of snow into ice, and thus to give it greater firmness and consistency. Pressure and certain other causes may also assist in the work of consolidation, till at length the mass is found to assume its final character of transparent solid ice.

In the higher regions of a mountainous chain there are usually many precipitous peaks and ridges with deep intervening continuous valleys, or more circumscribed circus-like hollows. The great majority of these have openings by means of which their drainage is more or less perfectly effected. If the ridge of a snow-mountain have this character, it is manifest that the snow which covers it will always tend to accumulate in the valleys, as being more sheltered than the neighbouring heights. Now it is found by observation that the masses of ice and snow thus accumulated do not remain quietly at rest, but creep slowly forth from their original cradles through the drainage valleys above mentioned. These latter valleys are in general nearly in the lines of quickest descent on the mountain side, and it is along them that the glacial masses of ice and snow descend from the higher regions in which they originate. Here, then, we have another and effective cause in constant action to limit the accumulation of snow and ice on summits of mountains on which they are formed, and to establish the equilibrium between the growth of the whole glacial mass in the higher and colder regions and its waste in the warmer regions below.

When a range of mountains is so high that its summits are covered

covered with perpetual snow, the line above which the snow never disappears is called the *snow-line*. The summer temperature at any point of it never exceeds 32° (Fahr.). The elongated portions of the general glacial mass which protrude below this line in valleys descending along the sides of the mountain, as above intimated, are more especially termed *glaciers*. The larger of them (those of the *first order* or *primary glaciers*) vary from four or five to ten or twelve miles in length. These are not essential limits, but they hold approximately in the Alpine glaciers, with which we are more familiarly acquainted than with any other. It is on *glaciers*, as thus defined, that the greater portion of recorded and systematic observations have been made.

A limit is imposed to the linear extension of glaciers, by the rate at which the ice melts as it descends into the warmer regions below the snow-line. In these regions the causes of decay, estimated by their whole annual effect, will predominate over those of production, and the glacier becomes thinner as it descends, till, at its lower extremity, the thickness vanishes and the glacier ceases. This dissolution of the mass takes place, in a greater or less degree, on its lower and on its lateral surfaces, as well as on the upper one, though it is at the latter surface that the greater part of the melting is produced. This process, it will be observed, is not altogether continuous during the whole year; for in the winter it will evidently be entirely arrested on the upper surface, which, at that season, becomes deeply buried in snow. The annual effect in each successive year will, nevertheless, be that due to the predominance of destructive causes. The whole mass is thus in the course of years reconverted into water, which then rushes down the valley with seeming impatience to regain the ocean from which it parted perhaps some two or three centuries before.

In contemplating a snowy mountain, we are led almost unconsciously to regard the enormous accumulation of ice and snow which gives to its summits their characteristic aspect, as being no less typical of all that is unchangeable than the rocks themselves on which it rests. We see, however, from the brief preceding explanations, that this perpetual snow, as it is termed, is rather a type of perpetual motion than of constant rest. It will be seen, in following the mutations of any constituent particle of the glacial mass, that its conversion from water into aqueous vapour, its ascent above the tops of the highest mountains, its conversion successively into snow and into glacial ice, and its final reconversion into water, and descent to the level from which it rose—that all these mutations form, in fact, one of those numerous cyclical or periodical processes by which Nature, in all her regions, unites the beauty and variety of changing aspects with a real stability capable of almost infinite duration.

It has been stated above that a glacier properly so called is the elongation below the snow-line of the general glacial mass which occupies the highest valleys and receptacles of mountains of sufficient elevation. A primary glacier will frequently originate in a single glacial receptacle above the snow-line, or it may proceed from two or more such receptacles, these partial glaciers uniting afterwards to form one principal glacier, precisely as two streams from different sources may unite to form one principal river. Thus the main glacier of the Aar, the scene of M. Agassiz's researches in the Bernese Alps, is formed by the junction of two great tributaries or affluents proceeding from separate sources, and termed, from the mountains in which they respectively originate, the Finsteraar and Lauteraar glaciers, the former being on the right, the latter on the left in descending. The whole forms a rough representation of the letter Y. The length of the resulting glacier, from the point of junction of these two principal tributaries to the lower extremity of the glacier, is nearly five miles, and its greatest width, which is at the junction, is upwards of three quarters of a mile. There are also many minor tributaries to this glacier, most of which unite above the junction to form respectively the two great tributaries, while, below the junction, four distinct lateral tributaries swell the united glacier by flowing into it from the valleys, along its precipitous flanks. The glaciers from Mont Blanc at Chamouni, the scene of Principal Forbes's more detailed observations, are also among the most important of the Alpine glaciers. Other glaciers on the southern side of Mont Blanc, the glacier of Zermat descending from Monte Rosa, the glacier of the Rhone and others, will be recollected as among the principal primary glaciers which have most occupied the attention of glacialists. The same general description is applicable to them all.

The term *primary* is generally used, as we have used it above, to denote the glaciers of large dimensions. There are also *secondary* glaciers, the horizontal extent and thickness of which are much smaller than those of a primary glacier. The inclination of the beds on which they rest is usually much greater than in the larger glaciers, and they are generally restricted to higher localities on the sides of the mountains. We are not aware of any series of accurate observations having been made on these smaller glaciers. We would recommend them to the notice of future observers. It would not only be curious to observe how

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far the different glacial phenomena may be modified by their peculiar conditions, but it is also very possible that they might afford valuable tests, whether at rest or not, of the truth of particular theories of glacial motion.

The inclination of the surface to the horizon in large glaciers usually varies from 2° or 3° to 8° or 10°. As a general rule the surface is most rough and dislocated where the inclination is greatest and most irregular. In many glacial valleys there are also steep escarpments, over which the ice is precipitated, and broken into thousands of enormous fragments, forming one of the wonders of Alpine scenery. The re-cementing of these fragments into one continuous mass of glacial ice at the foot of an ice-fall was, till recently, one of the most mysterious of glacial phenomena.

When we look down on the surface of a glacier from a considerable height, the minor inequalities of its surface become scarcely sensible. We may generally observe, however, even on the smooth portions of the surface, certain transverse lines, rare in the centre of the glacier, but more numerous in its two marginal portions, in each of which these lines are respectively nearly parallel ; and as they proceed from the flanks on either side towards the central portion, they incline towards the upper extremity of the glacier, instead of being perpendicular to its axis. These are the *crevasses*, gaping, vertical fissures, often large enough to present the most serious impediments to the progress of the traveller across them. They are rarely longitudinal in the elongated or canal-shaped glaciers, but in certain cases where the valley becomes suddenly divergent in its descent, the crevasses become also divergent, like the rays of a fan. The glacier of the Rhone, at its lower extremity, presents the best and most familiar example of crevasses of this latter kind. The theoretical explanation of all these phenomena belongs to the mechanics of glacial motion.

There is another group of objects, very striking in a bird's-eye view of the surface of a glacier. We allude to the long, dark, continuous lines of *débris* nearly parallel to the axis of the glacier, and stretching frequently from points near its upper extremity to its final termination. To the eye situated as above supposed, they appear free from all local asperities, following in graceful curves all the flexures of the valley. They consist of an aggregation of rocks and smaller detrital matter, the rocks varying from small pebbles to angular blocks of many tons in weight. These are the *moraines*. One is almost invariably found on each side of the glacier, and close to the bounding walls of the valley; they are the *lateral* moraines. Another moraine,

moraine, and usually the largest, is observed to coincide very nearly with the axis of the glacier, and is called the *median* moraine. In large glaciers there are frequently also other smaller moraines intermediate and parallel to those above mentioned. The glacier of the Aar furnishes, perhaps, the best examples of existing moraines with which we are well acquainted. Not far below the junction of its two great tributaries, as many as six or seven may be distinctly recognised. They are laid down with great accuracy in the map of this glacier, in Plate III. of the Atlas which accompanies the last work of M. Agassiz on glaciers, the *Système Glaciaire.* It should also be stated that aggregations of large blocks and smaller débris are usually found at the terminations of glaciers in front of the ice itself, and extending more or less completely across the valley. They are the *terminal moraines.*

The motion of a glacier is slow and persistent during all seasons, but slower in winter than in summer, and varying generally at different times and in different places, from a few inches to twenty or thirty inches a day. Moreover, in an elongated canal-shaped glacier, the axial portions move faster than what are termed the lateral or marginal portions. Also, the more superficial parts of the glacial mass move faster than the inferior parts. These inequalities of motion show that a glacier, in its aggregate mass, has a power of changing its form, so as to admit of these irregularities of motion, as well as to enable it to adapt itself to all the irregularities in the form and dimensions of the valley along which it descends. This property of the general glacier we call its *pliability.* It has been the subject of much earnest discussion.

The motion of the glacier enables us to account very clearly for the existence of central moraines. The lateral ones are manifestly due to the various blocks and débris which fall down the precipitous sides of the glacial valley on the glacier beneath, by the onward motion of which they are carried forward, sometimes the whole length of the glacier, and deposited in its terminal moraine. If, however, the lateral moraine belonging to one flank of a large tributary glacier meets the corresponding flank of another tributary, with its moraine (as at the junction of the two great tributaries of the Aar glacier), the two moraines necessarily unite, and move forward along the central line of the resulting glacier as its central moraine. A similar explanation applies to the moraines which are intermediate to the median and either lateral moraine. They all arise from lateral and usually smaller tributaries to the general glacier or to its principal affluents. If a lateral moraine, for instance, be formed in the

the upper portion of a large tributary, and a smaller tributary pour down its contents into the larger one, the lateral moraine of the latter, conjoined with one of the lateral moraines of the smaller tributary, will be thrust away from the side of the glacial valley, and will become one of the intermediate moraines above mentioned. A similar explanation applies to other moraines of this kind, the number of which is usually indicative of the number of minor tributaries which have helped to produce the general glacier. Many of these moraines extend to the lower extremity of the glacier and deposit their contents at the terminal one, which would thus grow incessantly, were it not that large portions of it are constantly removed by the current of water, frequently a powerful one, which issues from beneath the glacier at its extremity. Portions also of the lateral moraines work down to the bottom of the glacier, and are finally pushed forward to its extremity.

The powerful agency of glaciers, in transporting blocks of enormous magnitude from their original sites to points many miles distant, will be easily understood from what precedes. The recognition of this operation of transport as the daily employment, as it were, of nearly all glaciers, has led to some highly interesting conclusions in geology.

When the traveller descends from the high point of view from which we have supposed him to survey the glacier, and begins to traverse its surface, he becomes sensible of the rugged route along which he has to make his way. He finds that many of the crevasses, which appeared to him like so many narrow well-marked lines, are really deep yawning fissures, over which it is frequently impossible to pass without bridging them over by some artificial means. The large central moraines, also, which appeared like even dark longitudinal stripes on the glacier, he finds to be frequently elevated ridges of 20 or 30 feet in height. This elevation does not arise simply from the accumulation of the blocks and débris of the moraine, but partly also from an icy ridge which underlies them, and which has been formed by the protection against the wasting effects of sun and rain, afforded by the débris to the ice beneath it. *Glacier tables*, formed by large single blocks poised on pedestals of ice, are produced in a similar manner. Also the less dislocated portions of the glacier surface present, especially on sunny days, a beautifully bright effect, arising from the innumerable rills of water produced by the superficial melting of the ice. These rills sometimes form, by their confluence, considerable rivulets, which, of course, precipitate themselves into the first crevasse that crosses their course, thus making their way to the bottom of the glacier, whence the water is finally discharged from its lower extremity. The volume of water thus discharged in the winter is small, as might be expected; but in the warmer summer months is sufficient, in the case of a large glacier, to form at once a river of considerable magnitude.

It is impossible to overestimate the sublimity and beauty of these glacial masses, surrounded by their mountain accompaniments, whether we see them intruding themselves, as it were, at their lower extremities, into the fertile valleys of the lower Alps, and increasing by contrast the beauty of the summer verdure there, or whether we contemplate them in their solitary grandeur in the remoter portions of their higher regions. It was in 1841 that M. Agassiz may be said to have established himself on the glacier of the Aar, just below the junction of the two primary tributaries above described, for the purpose of observing the phenomena which the glacier might present to him. He there erected for himself, and two or three scientific friends who accompanied him, the tent which soon became so well known as the Hôtel Neuchâtelois, where, in that and two or three subsequent years, he received, with characteristic courtesy and hospitality, a large number of the philosophers of Europe. This glacier affords peculiar advantages for observations on glacial phenomena, and it was for this reason principally that M. Agassiz selected it. Nor should we conceive a continued summer residence on so accessible a glacier, and one which may be so easily traversed in any direction, as otherwise than very enjoyable. During the day-time, when the weather was fine, we have seen its whole surface alive, as it were, with innumerable gurgling rills of water, which, with the brightness of the snowy mountains, gave, even amidst the surrounding desolation, an animation to the scene which dissipated all feeling of loneliness. At sunset this scene is often suddenly and singularly changed. On the disappearance of the sun's rays, the surface-melting of the glacier, with every rill resulting from it, is immediately arrested, and, if the atmosphere is sufficiently serene, all is reduced at once to almost perfect stillness. The silence becomes imposing. Every little rill being hushed, there is sometimes literally not a sound to be heard, save that of the distant avalanche, occurring just often enough to make one the more sensible of the intensity of the silence. Such scenes offer, indeed, an adequate reward to every energetic traveller for all the effort he can make, and all the fatigue he may encounter, in seeking them.

We have no intention of entering into the earlier history of glacial science. We can do little more than mention the names of such glacialists as Simler, Scheuchzer, and Grüner, who, with others

others of inferior note, collected a considerable number of facts respecting the phenomena and topography of glaciers. Scarcely any facts, however, were accurately observed, and a great part of their theories were formed with very little knowledge of physical and mechanical principles. But De Saussure's work, 'Voyages dans les Alpes,' was of a far higher order than any which had preceded it. The author was a Swiss philosopher fond of physical science, and a devoted admirer of his native mountains. He resided at Geneva, and availed himself of his proximity especially to Mont Blanc to make visits to that mountain, and also to the other Swiss mountains, almost every summer for upwards of twenty years. He commenced his observations in the year 1760. They were not restricted to glaciers, but were equally extended to all those numerous physical, geological, and topographical facts which that region presents to the notice of the philosophical traveller prepared to appreciate at once the true value of the principles and laws by which Nature works, and the beauty of those varied and magnificent scenes which, in a country like Switzerland, she always presents to us. The results of all the long-continued observations of this philosophical traveller are embodied in his work above mentioned, consisting of four quarto volumes published at different times, as additional matter was collected and arranged for each successive volume. The whole work consists of a happy combination of scientific observation and philosophical discussion, enlivened by the introduction of agreeable personal details, and charming descriptive touches of those magnificent scenes of beauty which characterise these Alpine regions, but which at that time were imperfectly known, even to the few secluded inhabitants of the lower and more accessible valleys of the district. There is something peculiarly national in this work, and the name of De Saussure is one of which his countrymen may reasonably be proud. Many of his more abstract scientific observations have been superseded, as might be expected, by more advanced and recent researches; and the region which he was the first to describe in systematic detail is now popularly known from the large influx of travellers. But it must not be forgotten that his work remained for half a century the recognised and unrivalled receptacle of the best descriptions which existed of the scenery and physical phenomena of the Alps.

De Saussure did not devote his special attention to glaciers, and does not appear to have added to the then-existing knowledge of the subject much that was absolutely new, either in observed phenomena or in abstract reasoning. The great advantage which he conferred upon it seems to have been in methodising and generalising the knowledge or suggestions of those who had preceded him, rather than in adding to it discoveries of his own. He was prepared for this task of generalisation by his large acquaintance with the general phenomena of glaciers derived from personal observation. The distinct idea that glaciers moved by sliding over their beds appears to have been first advocated by Grüner, and subsequently adopted by De Saussure; but the latter was enabled by his larger acquaintance with glaciers to give to this view a wider generality, and therefore it is that his name has become so intimately associated with what has been termed the *sliding theory* of the motion of glaciers. Again, others had described, though very imperfectly, the moraines of glaciers; but De Saussure was the first to describe them systematically, and to recognise, in some degree, the important inferences deducible from the actual positions of portions of the blocks and detritus transported from their original sites by former glaciers. At the same time, it is singular that he should not have recognised the obvious origin of central moraines in the confluence of two lateral moraines belonging respectively to two confluent tributaries, as above described. He supposed them, on the contrary, to arise from a continual convergency of the lateral portions of the glacier towards its axis in the course of its onward motion—a conclusion entirely at variance, as we shall see, with subsequent observation.

The preceding explanations and descriptions have been designed to point out generally, and without details, the process by which glaciers are generated and maintained, and to indicate the aspect which they present to the eye of the traveller who may or may not desire to penetrate into the more hidden secrets of glacial mysteries. We believe that the pleasure which any intelligent traveller may derive from the contemplation of the external beauties of Alpine scenery may be materially enhanced by some acquaintance with the nature and constitution of these enormous moving masses of ice and snow. Those who may wish to acquire a more profound acquaintance with the subject must, of course, enter into the minuter details of observation and experiment, and must, moreover, bring to the task a considerable amount of mechanical and physical science. A portion of the remainder of this review will necessarily involve certain details more especially intended for the latter class of readers, but there will be much at the same time which may be easily understood by the more general reader, and which, we trust, may add to any interest he may already feel in glacial phenomena and glacial theories.

The internal temperature of a glacier has a bearing, to a greater or less extent, on most of the more important problems which glaciers present to us. We shall therefore consider this branch of our subject before we enter into the details on other branches of it.

We believe that M. Agassiz is the only one who has made direct experiments for the determination of the internal temperature of glaciers. A vertical bore had been made, for a different purpose, in the glacier of the Aar not far below the junction of its two principal affluents, of the depth of 60 mètres, or about 200 feet. Other bores were also made near the former one, of the depth of a few mètres. At the end of July, and a few days in the beginning of August, 1842, M. Agassiz observed the temperature in the shallower bores during fifteen days successively, at depths between 3 and 5 mètres, and found it to be invariably the temperature of freezing, neglecting very small discrepancies, in three only of the observations, manifestly due to some accidental cause. Simultaneously with these observations, M. Agassiz also examined several times the temperature indicated by the thermometer sunk to the bottom of the deeper bore of about 200 feet. He found it invariably at the freezing temperature, the zero of the Centigrade, and 32° of Fahrenheit.

These observations leave no doubt of the interior temperature having been very near 32° (Fahr.) at every point to the depth of 200 feet, during the summer months, after the snow of the preceding winter had entirely disappeared from the surface of the glacier.

Still these observations were only applicable to the summer months. In order to render them as complete as he was able to make them for the winter months likewise, M. Agassiz placed a thermometer in the glacier at the depth of $2\frac{1}{10}$ mètres, or about 7 feet, in the summer of 1842. After remaining there two years it was taken out, and showed that the minimum temperature to which it had been reduced during that time was $2\frac{1}{10}°$ (Cent.), or very nearly $3\frac{3}{4}°$ (Fahr.) below the freezing temperature. Consequently $28\frac{1}{4}°$ (Fahr.) was very nearly the lowest temperature which the glacier had acquired in two successive winters in that particular locality. M. Agassiz does not appear to have determined the winter temperature in the bore of 200 feet.

To explain the manner of determining the temperature generally at any point within the glacier, it will be necessary to state briefly the law of temperature within the superficial portion of the earth's crust, as determined by theory, and sanctioned by observation to the greatest depth (upwards of 2000 feet) to which man has been able to penetrate.

There is a very small uniform flow of heat from the interior

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parts of the earth through its outer solid crust, into the circumambient space. If the atmospheric temperature in any region of the earth's surface were constant and equal to the mean annual temperature there, the terrestrial temperature immediately beneath the surface would be the same as the constant atmospheric temperature; and at a point at any proposed depth beneath the surface, the temperature would exceed the superficial temperature by an amount increasing by 1° (Fahr.) for an increase in depth of about 70 feet. This is called the *mean* terrestrial temperature. But the atmospheric temperature changes from one season to another, and this superinduces a corresponding change in the terrestrial temperature; that change being greatest immediately beneath the surface, and decreasing with the depth till it becomes insensible at the depth of about 60 or 80 feet. Moreover, the atmospheric temperature varies from day to night, and such is also the case with the terrestrial temperature, but only to depths not exceeding one or two feet. Thus there is a *diurnal* variation of the terrestrial temperature to the depth of one or two feet, and an *annual* variation to the depth of 60 or 80 feet; while at greater depths the temperature at each point (the mean terrestrial temperature) is invariable from year to year, but is greater in proportion to the depth of the point beneath the surface.

If the upper stratum of the earth were ice (as it may be considered to be in the case of a glacier), results similar to the above would still hold true; because ice, so long as it remains *solid*, or its temperature is below 32° (Fahr.), allows heat to pass through it according to the same laws as any other solid. But there is this peculiarity in ice—that it ceases to be solid at the temperature of 32° (Fahr.). Now, it is easily proved that the flow of heat from the earth's interior is more than sufficient to raise the temperature of the lower surface of any considerable glacier, under ordinary conditions, to the above temperature. A part only, therefore, of the transmitted internal heat is employed in producing this effect; the remainder is employed in melting the ice at the lower surface of the mass, whence it necessarily follows that no considerable glacier can be frozen to its bed. The *mean* temperature of the glacier will vary frqm 32° (Fahr.) at the lower, to a temperature at the upper surface which depends on the atmospheric temperature, and is, in the middle region of the Alpine glaciers (as deduced from M. Agassiz's observations), between 1° and 2° (Fahr.) below freezing. It will be somewhat lower near the upper, and somewhat higher near the lower end of the glacier. Hence the *mean* internal temperature can never differ much from 32° (Fahr.) The actual temperature

perature will be subject to annual and daily variations, like those described in the terrestrial temperature; but these variations will penetrate only to still smaller depths than in the earth itself, nor will they ever exceed a few degrees. Consequently, the internal temperature of a primary glacier will be approximately uniform, especially in its lower portions.*

There is also another cause which must help in producing the approximate uniformity of the interior temperature. M. Agassiz made a number of experiments on the glacier of the Aar, proving a considerable infiltration of water through the small pores and crevices of the ice ;† and though Professor Huxley failed, in certain more limited experiments on the Mer de Glace, to obtain the same result, it would seem very difficult according to all existing evidence to doubt this infiltration as a general fact. If it does take place, the water must enter the glacier at a temperature of 32° (Fahr.), and must constantly tend to raise the interior temperature to that height. The winter cold, within the small depth to which it penetrates, will, more or less, counteract this tendency; but below that depth the temperature must ultimately rise to 32°, and remain constant. This is consistent, it will be observed, with the temperature observed by M. Agassiz at the depth of 200 feet.

These resulting temperatures as above stated are deduced from accurate solutions of the problem, and admit of no ambiguity or appreciable error. They do not appear to us to have been always attended to in speculations on which they have an immediate and important bearing.

We shall now direct the attention of our readers to that property of ice by virtue of which it is capable, at a certain temperature, of what is called 'regelation.' The discovery of this property, and the recognition of its applicability to the explanation of certain glacial phenomena, of which no adequate explanation had been previously given, constitute a most important epoch in the history of glacial science. It rescued our glacial theories from much of the vagueness and indeterminateness which till that time had hovered about them, and assisted greatly in placing the science on that basis of accurate investigation and exact experiment to which, in some of its most important points, it had no previous pretension.

In the month of June, 1850, Mr. Faraday exhibited an experi-

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* The solution of the above problem will be found in the 'Philosophical Magazine' for January, 1845, vol. xxvi. See also the memoir 'On the Theory of the Motion of Glaciers,' in the 'Transactions of the Royal Society' for 1862. Read May 22nd, 1862.

† 'Système Glaciaire,' chap. ix.

ment at an Evening Meeting of the Royal Institution, in which he showed 'that when two pieces of ice with moistened surfaces were placed in contact, they became cemented together by the freezing of the film of water between them; while, when the ice was below 32° (Fahr.), and therefore *dry*, no effect of the kind could be produced. The freezing was also found to take place under water; and, indeed, it occurs even when the water in which the ice is plunged is as hot as the hand can bear.'*

It was a generalisation of this simple but curious fact, that suggested to Dr. Tyndall the experiments which have so largely affected the state of glacial science. In the above experiment the two blocks of ice not only cohered to each other, but became so perfectly united that it was no longer possible to recognise their plane of junction. Now it occurred to Dr. Tyndall that if *two* pieces were capable of thus uniting, any number of pieces must equally unite if placed under similar conditions; and consequently that we might expect that an indefinite number of indefinitely small fragments, under a pressure which should secure the requisite contact of contiguous particles, at the temperature of 32° (Fahr.), would coalesce into one continuous mass of transparent ice. The conclusion was tested by the following experiment:—

Two cubical blocks of seasoned boxwood had each a cavity hollowed out on one of its sides, such that when these two sides were placed in contact, the contour of the one cavity exactly corresponded to that of the other; and the two cavities together formed a lenticular vacant space between the two blocks of wood. A ball of ice was placed in this vacant space, not of the same form as the cavity itself, but of something more than sufficient bulk to fill it when forcibly pressed into it. The two blocks were then placed under a hydrostatic press, and a pressure applied to them sufficient to crush the ice and make it assume the form of the cavity in which it was placed. In this process the ice was of course broken into atoms; but when turned out of the mould, within the few seconds of time necessary for that operation, it had been *regeled* into a perfectly continuous and transparent lump of ice. The regelation appeared to have been effected almost at the instant that the crushing was completed.

This is the simplest form of the experiment, and exhibits most satisfactorily the result of the process which is called *regelation*. The ice is not squeezed like a soft substance, but cracked, split, and broken into thousands of pieces, which, brought into contact by the pressure, are again united into one continuous

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* 'Glaciers of the Alps,' p. 351.

mass by the process of regelation. We are not here speaking of the nature of this process—of the molecular actions which may be involved in it. We are appealing merely to the *result* of that process as an observed fact; and the fact itself may manifestly be made the base of our speculations, without our knowing the *modus operandi* of the process, just as we may reason upon the facts or results of crystallization, notwithstanding our ignorance of the physical process by which those results are produced. We are the more anxious to point out this distinction because we imagine that we discern a disposition on the part of some glacialists, in the application of regelation to the explanation of the motion of a glacier, to depart from the *facts* or *results* of regelation, with which we are acquainted, to the *modus operandi*, with which we are not acquainted. The term 'regelation' has been objected to as seeming to indicate the nature of the process by which the effect above described is produced; but it must be distinctly understood that when we speak of the 'property of regelation' as characterising ice at the particular temperature of 32° (Fahr.), we mean simply that property in virtue of which ice at that temperature is capable of being broken and fractured, and instantly reunited into a continuous mass, as above described. We shall see in the sequel the great importance of this property of ice, in the theory of the motion of glaciers.

We may add that Dr. Tyndall has varied the above experiment in several ways, as may be seen by referring to his 'Glaciers of the Alps,' p. 346, or to his Memoirs in the Transactions of the Royal Society.

The *modus operandi* in the conversion of snow into the compact ice of the lower glacier, is intimately connected with the internal temperature of the mass. In the colder glacial regions the falling snow is usually dry, and consists of fine granules; but when the atmosphere is more moist, and its temperature little exceeds that of freezing, the snow is flocculent. During the winter a thick covering of snow is deposited on the glacier; but, below the snow-line, the whole of this snow, together with the superficial portion of the pre-existing glacier beneath it, is dissolved by the heat of the following summer. Above the snow-line, on the contrary, a part only of the previous winter's snow is dissolved, and the other part remains as a permanent addition to the glacier, thus forming an annual stratum which may or may not be afterwards recognisable as distinct from similar strata above or below it. When the summer warmth begins to predominate in these higher regions, the superficial snow is melted by the sun's rays, though the atmospheric temperature may be considerably below 32°. The water thus produced

produced sinks into the porous mass of snow, the temperature of which will necessarily be below—and in the highest regions considerably below—32°(Fahr.). This percolating water will therefore become partly frozen, as above intimated, the depth to which the infiltration proceeds depending on circumstances. The portion of the last winter's snow which remains at the end of the summer thus becomes changed into a granular mass, while the mass immediately below it will also be further modified in like manner. The more superficial portion of the whole mass thus transformed becomes granular, and is called *névé;* it becomes more and more consolidated as the depth increases, till it finally assumes the character of compact glacial ice. We should expect the mass thus formed to be stratified, but that its indications of stratification would be feeble. It is in this manner that the glacial mass increases above the snow-line, to compensate for the waste below it.

In the higher regions in which glaciers originate, the minimum superficial winter temperature will frequently be much less than that determined, as above stated, by M. Agassiz in the middle region of the Aar glacier, though the winter covering of snow will tend to equalise these temperatures in different localities. Whatever effect, however, may be produced by a lower atmospheric temperature in the higher glacial regions, the tendency of the infiltrated water, as above explained, must always be to raise the temperature to that of freezing in the lower and far greater part of the mass into which the winter cold never penetrates. Allowing this influence of infiltration, the lower portion of the glacial mass will have the same temperature in these higher and colder regions as in the milder middle and lower regions of the glacier ; but the portion affected by the winter temperature will be generally colder and its depth greater where the mean external atmospheric temperature is the lowest, and especially in winter.

The conversion of snow into névé, and subsequently into consolidated ice, has been a subject of frequent discussion. The views of all the earlier glacialists, and of some also of the later ones, were founded on conceptions more or less erroneous respecting the internal temperature of glaciers. Pressure is a cause, as well as temperature, to which this conversion has been attributed (p. 78). M. Agassiz has described his experiments and stated his views more explicitly than any other glacialist in Chapter V. of his 'Système Glaciaire.' He probably erred in attributing too much importance to the interior temperature. Principal Forbes, in his earlier speculations, appears to have recognised congelation, due to the winter temperature, as the effective cause

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in producing the transmutation we are speaking of; but he afterwards rejected this idea, and adopted the opinion that it was due to pressure alone; for, in 1846, he writes, 'I am satisfied, then (and it is only after long doubt that I venture this confident expression), that the conversion of snow into ice is due to the effects of pressure on the loose and porous structure of the former.' To the operation of direct pressure he adds that of the 'kneading or working of the parts on one another,' due to a difference of motion of two contiguous particles and the consequent friction between them.

Dr. Tyndall has stated his views on this question in his 'Glaciers of the Alps' (p. 249-251). He appears to consider direct pressure as the principal cause of the solidification of the ice, aided, perhaps, by congelation in the colder portions of the mass.

None of these views appear to be sufficiently based on determinate conceptions of the interior temperature of the glacial mass. If the mean annual atmospheric temperature be several degrees less than 32° (Fahr.), the temperature during the later winter and earlier spring months will be considerably below the freezing temperature generally, at depths not exceeding that to which the winter cold is able to penetrate. In that part of the mass, therefore, congelation must necessarily attend infiltration, and must probably be a more efficient cause than pressure, which, in the more superficial portion of the mass, must be comparatively small. In its lower portion, on the contrary (if we allow the full effect of infiltration there), the temperature must be very nearly that of freezing, and congelation will proceed very slowly, while the pressure will become comparatively large and efficient. It appears to us that both the causes here spoken of must be effective, but more especially in different parts of the mass.

The process of regelation could not, of course, be even tacitly alluded to in any of the explanations above mentioned previous to that given by Dr. Tyndall, since he was the first to discover its importance in glacial questions; nor even in his own explanation do we see any explicit allusion to its probable efficiency in the consolidation of the névé into compact ice. But it does appear to us that it is by means of this process that pressure is enabled to produce a particular kind of consolidation in ice at the freezing temperature which it is incapable of producing at any lower temperature. In fact, we do not see how we can do otherwise than recognise the efficiency of this cause, so far as we recognise the temperature of 32° in the greater portion of the mass.

When the glacial mass passes from the state of névé to that of the proper glacial ice, it does not necessarily become a homogeneous hard transparent mass, but is frequently found to consist of alternate layers of two apparently different kinds of ice, one of which is of a dark bluish colour, and transparent, the other of a dull white colour, and opaque. These layers usually vary in thickness from the fraction of an inch to one or two inches, or upwards. Their continuity is more or less perfect for considerable distances, and their position, in the great majority of cases in which their development is most complete, approximates to verticality. The colour of the whiter layers is found to be due to the presence of a great number of small air-bubbles contained in them; the blue layers derive their greater transparency from the comparative absence of these bubbles. The structure is usually designated as the *ribboned*, *laminar*, or *veined* structure of glacial ice. These laminæ appear to be developed as the ice consolidates from its state of névé, and may be regarded as a general property of the ice in its consolidated form, however different its development may be in different parts of a glacier, and however much that development may seem to depend on local conditions.

Whatever may be the physical cause of this peculiar structure, there seems to be no doubt of its being, in many cases, gradually developed during the transmutation of the névé into compact ice; and it appears to be equally certain that the structure, so far as regards the positions of the bands and their degree of development, may be suddenly and entirely changed when the cause producing the change is sufficiently energetic. The most complete proof of this latter statement is found in the structure immediately at the bottom of the ice-falls which form such striking features in the external aspect of a glacier. The structure in such localities is always finely developed, the veins are nearly vertical and transverse, their intersections with the surface of the glacier running nearly in straight lines across it, in directions perpendicular to its axis. This appears to be universally true, whatever may have been the degree of development of the structure, or the positions of the veins in the glacier immediately above the fall. There can be no doubt, therefore, as to the structure originating at the bottom of the fall, so far as it is distinguished by the characteristic positions of the veins as above described. When we examine the glacier at points more or less remote from the fall, we find the nearly straight transverse lines of structure converted into elongated loops, with their vertices directed towards the lower end of the glacier, and the question arises whether these loops are the original transverse lines of structure,

ture, distorted into lengthened curves by the more rapid motion of the axial portion of the glacier; or whether they are altogether new structural lines resulting from the action of causes similar to those at the foot of the fall, their effects being modified by the change of conditions under which they act? This is a question which we shall discuss in the sequel. It may here be sufficient to remark that the positions of the veins and structural curves on the face of the glacier are generally such as might be anticipated, supposing them to be transmitted from the locality in which they originated, but to be elongated and deformed, as above described, by the unequable motion of different parts of the glacier.

In a canal-shaped glacier the elongated curves of structure will thus become more nearly parallel to the sides of the glacier in its marginal portions, as they move onward from the fall. Dr. Tyndall has appropriately designated the structure in those portions, the *marginal structure.* The laminar structure is also strongly developed on large glaciers beneath their central moraines, which arise, as above explained, from the junction of two of the lateral moraines of two large tributaries, as on the glacier of the Aar. In such cases the veins are vertical and longitudinal, and such as would result in the united glacier from the marginal veins of the tributaries, when those veins should be nearly parallel to the sides of their respective tributaries. This has been called the *longitudinal structure.* From the foot of the great fall of the Rhone glacier, and in some other glaciers, the forms of the valleys are such that the ice moves from them in radiating lines, and the curves of structure consequently expand into curves of an approximately circular form. Most Alpine travellers will have remarked the striking feature they form on the glacier of the Rhone, between the fall and the terminating circular contour of the glacier. The Mer de Glace is also one of the well-known glaciers which exhibits the different varieties of this structure in great perfection.

We have already indicated the way in which the névé may become more or less distinctly stratified, and all glacialists probably agree in the belief that stratification may be frequently recognised in that portion of a glacial mass. There has been, however, great difference of opinion as to the permanence of any visible stratification in the consolidated ice of the lower portions of glaciers. M. Agassiz regards it as a permanent and pervading character of all glacial ice, derived from the original stratification of the névé. Principal Forbes, on the contrary, considers it to exist only in the névé, all indication of it disappearing in the true glacial ice. He cites the Talèfre glacier in support of his assertion. But these two observers did not agree as to what appearances

ances were to be regarded as really indicative of stratification. Dr. Tyndall refuted Principal Forbes's opinion by the discovery in several localities of the coexistence of stratification and a well-developed veined structure in the same mass. It should, however, be remarked that, after diligent search on the glaciers of Mont Blanc and Monte Rosa, he found comparatively few instances of this coexistence; and the inference from the observations of the two last-named glacialists would seem to be, that though the two phenomena in question do sometimes coexist in the same mass, the external proofs of their coexistence are comparatively rare. M. Agassiz's views on this subject are more complicated. We shall recur to them shortly.

We may now direct attention to certain bands, called *Dirt Bands*, which have been remarked on the surface of a few Alpine glaciers, and which appear to be in some way associated with the laminar structure just described. They form elongated loops, similar to the above structural curves, defined by a slight dirty tint, very feeble, but sufficient, when seen under favourable circumstances, to distinguish them from the whiter intervening spaces. Their darker colour is caused by a small quantity of sand and dirt spread along them on the surface of the glacier. They were first observed and described by Principal Forbes on the Mer de Glace. He was able to enumerate eighteen of them between Trélaporte and the lower extremity of the glacier, with the average distance of about 700 feet between their vertices, measured along the axis of the glacier. Sixteen or seventeen years afterwards Dr. Tyndall recognised the same number within the same limits; whence we may infer that this mean distance between them is determined by some law, and not by merely accidental circumstances.

It is to the two observers above mentioned that we owe our principal knowledge of these bands on the Mer de Glace. Principal Forbes supposes the glacier to consist of alternate portions of more and less porous ice, each portion being bounded by an internal surface which coincides with a surface of one of the laminæ of the veined structure, and that the bands arise from the fact that the dirt, diffused by the winds or other superficial causes over the surface of the glacier, adheres to the porous more than to the harder portions of the ice. The defect of this view is that it leaves the hypothesis of the alternate occurrence of zones of greater and less porosity entirely unsupported by observation or theory. It amounts to little but the assertion of the fact of the coincidence of the bands and superficial curves of lamellar structure.

At the foot of the ice-fall of the glacier du Géant, Dr. Tyndall

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found these bands restricted to the ice which had been precipitated down the fall. He observed there also a number of large transverse ridges [*] or *rucks* of the glacier, which Principal Forbes seems also to have observed,[†] and which he appears to refer, as we think, to their right mechanical cause—a periodical augmentation of the enormous pressure *à tergo* arising from the more rapid flow of the ice down the fall when liberated by the approaching warmth of summer. Dr. Tyndall also observed that snow was still remaining on the sides of those ridges least exposed to the sun's rays, and that this snow was the receptacle of a considerable quantity of dirt conveyed thither by external causes, and retained by the snow, to be finally deposited on the surface of the glacier. He regards the porosity of the ice immediately beneath the bands as merely superficial, and to be the effect of the bands, and not their cause, as asserted by Principal Forbes, being produced, he supposes, by the sinking down of the particles of dust into the surface of the ice, in consequence of the greater heat which they imbibe from the sun's rays.

Dr. Tyndall's theory of these bands requires confirmation by more extended observation, but it involves no difficulty which appears to us at present so great as that involved in Principal Forbes's hypothesis of the existence of alternate zones of more and less porous ice in the glaciers in which these bands are observed.

Both these theories of the dirt-bands involve the superficial origin of the dirt which colours the bands, and are in this respect opposed to the views of M. Agassiz, so far, at least, as we comprehend those views. The latter glacialist appears to refer most of the alternating bands or laminæ of blue and white ice, above described as the veined structure, to the original stratification of the névé. He states, as the result of observation, that many of the stronger blue veins in the consolidated glacial ice are accompanied by fine particles of sand and dirt which lie intermediate to those veins and the contiguous whiter ones. He seems to conceive the veins thus distinguished to be derived from the stratification of the névé, of which, in fact, they are to be regarded as the continuation into the compact ice of the middle and lower glacier. So far, too, as we understand our author, the laminæ of blue and white ice intermediate to the stronger laminæ above mentioned, belong also to the stratification which he represents as pervading the whole mass of the glacier. He regards the real veined structure as a comparatively superficial and local phenomenon, in

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[*] 'Glaciers of the Alps,' p. 369 *et seq.*

[†] 'Occasional Papers,' p. 40 (1844).

in which the laminæ may or may not coincide with those layers which he asserts to belong properly to the stratification; but we are unable to see any distinct cause to which the real veined structure, according to his views, is to be referred. Again, the author of this theory is bound to explain how the original strata of the névé could assume the varied but regular positions of the blue and white veins in the lower parts of the glacier, and especially the transverse and vertical position which they uniformly assume at the foot of an ice-fall, after the ice has been broken into innumerable fragments. In all this the failure is so manifest as to be condemnatory at once of the theory. We may, however, remark that the facts respecting the re-formation of the laminar structure at the foot of an ice-fall were then far less perfectly known than at the present time. Still, the confusion and inadequacy of the generalizations and conclusions appear to us to be inconsistent with the care and detail with which the observations themselves were evidently made, and also with the care with which many of the curves of structure are delineated by trigonometrical admeasurements on the map of the Aar glacier contained in the Atlas which accompanies the *Système Glaciaire.* It is very desirable that that glacier should be again carefully and impartially examined in reference to its laminated structure, with the additional light which has been thrown on the subject since the period when M. Agassiz's observations were made. Those observations were evidently conducted with great care, and might, we doubt not, be brought into harmony with the observations of other glacialists, instead of standing, as they do now, in perplexing antagonism to them, both as to facts and as to interpretations.

We have already alluded to the experiments on infiltration, made respectively by M. Agassiz and Professor Huxley. The former states that he had ascertained by observation that the coloured infiltrating fluid passed through the compact ice in which he conducted his experiments, *entirely along the capillary fissures* (*Systèm. Glac.*, p. 173). These fissures are described as dividing the mass into small angular fragments, without any recognizable order of form or arrangement (p. 163); and it is stated that when a lump of ice from the interior of the glacier was exposed to the external atmosphere, it was easily disintegrated by the separation of these angular fragments from each other. It is to the absence of all regularity in the forms and relative positions of these fragments that we would here more especially direct the attention of our readers, as indicating the absence of all tendency in the general mass to yield in one direction more than another to any forces which may be exerted to tear and rupture it it by extension. This conclusion is important as regards the theories of glacial motion which we shall have to discuss. So far as relates to those parts of a glacier in which, according to Prof. Huxley, no infiltration takes place, and in which, therefore, there can be no such fissures as are here contemplated, the same question as to the difference of cohesion in different directions cannot arise, unless it should be that certain determinate planes of crystallisation may give a predominant tendency to the mass to cleave in some particular direction. We cannot ascertain, however, that any such tendency has been detected in ice; and we may the less expect it to exist in glacial ice than in any other, from the manner in which that ice passes by a gradual process of consolidation from snow to the compact ice of the lower glacier. It may also be remarked as somewhat singular that glacial ice, even where the veined structure is most completely developed, should indicate no tendency, while unweathered, to cleave along the veins rather than in directions transverse to them.

M. Agassiz also made a number of experiments and observations respecting the interior structure of glacial ice;* and Dr. Tyndall has subsequently made somewhat similar experiments on common or lake-ice.† Such experiments are highly interesting to the physicist, but at present they seem to have too uncertain a bearing on our glacial theories for the critics of such theories to dwell upon them, even if our space would admit of our doing so. We would only remark that these experiments reveal nothing, especially in reference to glacial ice, to justify the inference of there being any of the greater tendency above alluded to, to cleave in one direction rather than another.

Before we discuss the different theories which have been propounded to account for the observed motion of a glacier, it will be necessary to define accurately what we mean by the *viscosity* or *plasticity* of a body, since this property has been especially appealed to in the glacial theory which, till a late period, occupied so large a share of public attention in this country. It will be understood that we allude to the *Viscous Theory.* And here we may first premise that all exact definitions of such terms as *solidity*, *viscosity*, *fluidity*, *elasticity*, and the like, must necessarily be *mechanical*, since all the properties of bodies denoted by such terms indicate a power, greater or less, of resisting the tendency of external forces to change the form of a body, or, what is equivalent, to change the relative positions of its com-

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* 'Système Glaciaire,' p. 163 *et seq.*

† 'Glaciers of the Alps,' p. 354.

ponent particles with reference to each other. Again, we can pursue no exact reasoning, and make no accurate calculations in which the properties here spoken of are involved, without some determinate *measures* of those properties, and such measures can only express the mechanical capability which the body may derive from each of them, to resist a given amount of force acting under given conditions—*i. e.*, the measures of these properties, as well as their definitions, must be *mechanical.*

We may also here remind our readers that a body or mass of any substance is said to be in its natural state, or in a *state of no constraint*, when it is acted on only by the mutual attractions of its component particles, and not by any extraneous forces whatever. Extraneous forces tend, of course, to move a body from one position in space to another; but that is a fact with which we are not here immediately concerned. Another effect is to change the form of the body, and thereby to bring it into a *state of constraint;* and when spoken of with reference to this effect, these forces are frequently termed *constraining forces.* Again, when a body is held by extraneous forces in a state of constraint, certain internal or molecular forces are called into action, by virtue of which the body has a tendency to regain its natural form, and will regain it more or less completely if the constraining extraneous forces be removed. These internal molecular forces may be conveniently designated as *forces of restitution.* Thus if a body be extended or compressed in any directions, or twisted and contorted, corresponding forces of restitution, of greater or less magnitude, will be called into action, tending to restore, in a greater or less degree, the unconstrained form of the body. In such a case the body is said to be more or less *elastic*, the *elasticity* called into action in each case being measured by the greater or less tendency of the body to recover its original form. Certain substances exert a great force of restitution whenever they are deformed or placed in a state of constraint, whether by extension, compression, or torsion. Such bodies are said to have great *elasticity;* and when, moreover, they require a very large force to fracture them by extension, or crush them by compression, they are called *solid* bodies. If the force required for this purpose should be indefinitely large, the body would be said to be *perfectly rigid;* but this is a state to which bodies in nature can only approximate, but never attain. In solid bodies, too, the *cohesive power* to resist extension or tension, and the *resisting power* to resist compression, must, from the above definition, be great; and the *extensibility* and *compressibility* will be small.

Again, we may conceive the form of a homogeneous substance to to be altered without altering its volume, and so that its specific gravity shall remain uniform and unaltered. In such case, if no force of restitution, or, therefore, no elasticity be called into action, tending to restore the substance to its original form, the body is said to be *plastic*. Such bodies may possess great power of resistance to any compression of their volume, but have usually a small cohesive power. The distinctive character is, that they will retain any arbitrary form which may be given to them consistently with the preservation of their volume and uniformity of their specific gravity. Thus a lump of clay sufficiently softened by moisture, or a piece of wax sufficiently softened by heat, are *plastic* substances. The essential definition of *viscosity* is the same as that of plasticity, except that the term is usually applied to substances which approximate more nearly than plastic bodies to a state of fluidity. Thus, if the wax cited as a plastic body were still further softened by heat, it would be called viscous rather than plastic. In both cases the constituent particles are moveable *inter se* without changing the whole volume of the substance, or necessarily exciting any force of restitution; but in bodies termed viscous there is less cohesive power than in those usually termed plastic, and consequently the molecular relative displacements are more easily effected.

A substance like india-rubber may be cited as having a property intermediate between solidity and plasticity. When it has been extended, compressed, or angularly contorted, it will return, after the removal of the constraining forces, almost exactly to its original form—*i. e.*, its *elasticity* is great, and so far it resembles a solid body. On the other hand, its *extensibility* is great, and it might seem in this respect to approximate more to a plastic than to a solid body. Its great elasticity, however, destroys all approximation to real plasticity. Such a substance is more conveniently designated as an *elastic body*, the term *elastic* being here understood to indicate the combination of great elasticity with great extensibility or compressibility.

The importance of exact definitions of such terms as solid, plastic, viscous, &c., can only be understood when we come to analyse and compare the different theories of glacial motion. But before we proceed to the review of that part of our subject, it will be desirable to recapitulate the principal observed facts respecting the motion of glaciers.

It has been already stated that all primary glaciers move onwards with a slow but persistent motion. This general fact was known to the earlier glacialists; but it is to later observers, especially to M. Agassiz and Principal Forbes, and more recently to Dr. Tyndall, that we owe our detailed knowledge of the motion in question. We can here do little more than state the principal results arrived at, and make a few remarks on points of precedency of observation and other collateral matters which fall more, perhaps, within the province of the critic or historian of science, than in that of the scientific philosopher. The principal observed facts respecting the motion of primary glaciers are the following:—

1. In an elongated glacier the axial portion moves faster than its marginal portions, as above stated; but the point of maximum velocity in a line perpendicular to the axis of the glacier, though usually near to the axis, is frequently not upon it. When the course of the glacier, for instance, curves more rapidly than usual to the left, the point of maximum velocity will be thrown towards the right side of the glacier, and *vice versâ.* Also the velocity along the line of maximum velocity varies at different points, according to local circumstances of the inclination of the valley, its width, or particular impediments. On the Mer de Glace it seems to vary generally from about 20 inches a day in the higher portion to about 30 inches in the lower part of the glacier.

2. The ratio which the velocity in the extreme marginal portion bears to the maximum velocity in the same transverse section is very variable. On the Mer de Glace it appears to vary in many places from about one-third to one-half. In particular localities, however, it may be much less, in consequence of local obstacles along the sides; but in such cases the marginal portions are much broken and fissured transversely. At no great distance from the lateral boundaries of the glacier, the motion usually becomes much more equable.

M. Agassiz has given an account, in chap. xii. of his 'Système Glaciaire,' of observations which he made on the Aar glacier by means of a great number of stakes placed originally in a straight transverse line across the glacier, the positions of which were observed for three or four successive years. The curves assumed in these different years by the straight line on which the stakes were originally placed, are delineated, on the beautiful map contained in the Atlas accompanying the above work. The motion of the mass is thus presented, as it were, to the eye. We would also refer our readers to the account which Dr. Tyndall gives in his 'Glaciers of the Alps' of similar observations made at six or seven places entirely across the Mer de Glace. Principal Forbes likewise made a number of more insulated observations on different glaciers, showing the generality of the law above stated respecting the relative velocities of the axial and marginal portions of the glacier; but we are not aware of his having made observations at a number of points, in any locality, extending entirely

entirely across a glacier, as in the observations of M. Agassiz and Dr. Tyndall.

3. A primary glacier slides over the bed of the valley containing it.

4. As the axial portion of a canal-shaped glacier moves faster than its lateral portions, so the superficial portion moves faster than the lower one.

When Principal Forbes first put forth his Viscous Theory, he manifestly regarded that part of the motion of the whole mass which depends on the *sliding* of its lower surface as insignificant (if, indeed, it existed at all) compared with the excess of the motion of the upper surface over that of the lower one, or that due to the *pliability* (p. 82) of the mass, to whatever cause that pliability might be due. Others, on the contrary, while admitting both the sliding and the pliability as *veræ causæ*, thought that the former was probably more efficient than the latter, and urged the necessity of determining their relative influences by actual observation.* The first observation for this purpose was made by Principal Forbes at the terminal face of the Glacier des Bois, at Chamouni. He found that, of the whole motion of the upper surface of the glacier, the part due to the *sliding* of the mass was rather more than one-half; that due to its *pliability* being consequently rather less than one-half.† Dr. Tyndall, by similar observations, in 1857, on the flank of the *Glacier du Géant*, obtained the result that the latter of the above causes was there somewhat more efficient than the former. The mean of these results would assign nearly an equal efficiency to each of the causes above mentioned.

The important fact, however, that glaciers do slide is not dependent alone on this limited evidence; for every valley which we believe to have been a glacial valley either in remote or more recent periods, bears evidence, in the striated and rounded surfaces of its rocks, to the sliding of the glacier formerly contained in it. Exactly such are the striating and rounding effects that recent glaciers are producing, and no glacialist, we imagine, now doubts the sliding motion here asserted.

5. The motion continues during the winter, but is slower during that season than during the warmer months of summer. The clearest observations we have on this subject are those made by Dr. Tyndall at midwinter on the Mer de Glace, and described in his 'Glaciers of the Alps,' p. 294.

The observations by which the greater relative velocity of the axial

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* 'Phil. Mag.,' 1845. Mr. Hopkins's third letter on the 'Motion of Glaciers.'

† 'Occasional Papers,' p. 173. It may appear singular that Principal Forbes never referred to this result except as a proof of the *viscosity* of glacial ice.

axial parts of a glacier was determined, did not certainly involve much ingenuity either in their conception or execution. Still, the fact is a cardinal one in the motion of a glacier, and this is probably the principal reason why the claim of precedency in the mere fact of making these observations has been sometimes insisted on with undue urgency. In this country Principal Forbes, whatever might be the reason, was generally regarded as the first who determined explicitly by observation the true relative velocities of which we are speaking, while M. Agassiz was scarcely considered to have had any share in the matter. Under these circumstances, Dr. Tyndall did nothing more than simple justice to the latter observer, in making known the facts of the case to English readers. We quote Dr. Tyndall's own words:—*

> 'The facts, then, so far as I have been able to collect them, are as follows:—M. Agassiz commenced his experiment (for determining the relative velocity in question) about ten months before Professor Forbes, and the results of his measurements, with quantities stated, were communicated to the French Academy about two months prior to the publication of the letter † of Professor Forbes in the "Edinburgh Philosophical Journal." But the latter publication, in announcing in general terms the fact of the speedier central motion, was dated from Courmayeur twenty-seven days before the date of M. Agassiz's letter from the glacier of the Aar.'

Should our readers be in the humour to compare small things with great ones, they will see in the case just stated an analogy with that of the predictions of a new planet by M. le Verrier and Professor Adams. The latter was the first to make the prediction; the former was the first to publish it. The scientific world has justly refused to give exclusively to either of these astronomers an honour to which the other had an equal claim. The same kind of equal justice will be done, we doubt not, to the glacialists of whom we have been speaking.

A glacier presents to us a great physical problem in its first formation, in the peculiar characters of glacial ice, and in the transformations which it undergoes between the first conversion of the matter composing it into snow, and its final reconversion into water; and it also presents to us a great mechanical problem in the phenomena of its motion. The majority of glacialists, even in recent times, have probably been interested in the subject more on account of the physical than the mechanical questions involved in it; and it may, perhaps, be asserted as probable that the principal importance which has been usually attached to the

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* 'Glaciers of the Alps,' p. 273.

† 'Occasional Papers.' Letter dated July 4, 1842, p. 9.

latter questions has been found in regarding them as subservient to the physics of the subject, rather than in the solution itself of a great mechanical problem which Nature here presents to us. This preference would seem to us to be associated—partly, perhaps, as a cause, and partly as an effect—with the fact that few persons previously versed in abstract mechanics have directed their attention to glacial phenomena. A great number of observations have been made on the motion of glaciers, but there are very few glacialists who have professedly regarded the subject under its mechanical aspect, and endeavoured to bring to bear upon it the fundamental principles, with the exact reasonings and methods, of mechanical science. This led to loose and inaccurate methods of treating the mechanical problems of the subject, and to fundamental hypotheses too indeterminate to be made the foundation of a sound glacial theory. But, some years ago, the Viscous Theory, as it is termed, was received with that degree of confidence which scarcely admitted, without manifestations of impatience, the claims of free discussion, though still a certain number of scientific men always regarded it with that reserve which has been since, as we conceive, well justified by the discovery of regelation. It was this important discovery which aroused many glacialists to the conviction that glacial theory might be made to rest, not on an unproved hypothesis like that of the viscosity of glacial ice, but on the results of accurate experiment and exact investigation.

It has already been stated that Grüner was the first to suggest that the motion of a glacier was due simply to gravity, which urged it down the valley containing it, as it urges the descent of a body in ordinary cases down a plane sufficiently smooth and sufficiently inclined to the horizon; and, moreover, that this view obtained a considerable circulation in consequence of its adoption by De Saussure, with whose name it became associated under the appellation of the *sliding theory* of De Saussure, though he neither seems to have made any material addition to it nor to have removed the difficulties which it appeared to involve. Nearly forty years afterwards, several Swiss observers directed their attention to glacial phenomena, after the subject had remained nearly dormant for a considerable period. They rejected De Saussure's theory in favour of what was called the *Dilatation Theory*, according to which a glacier was propelled onwards by the expansion of its mass due to the freezing of the water contained in its internal pores. It is now entirely exploded, as being inconsistent with the interior temperature of a glacier, and as leading to a resulting motion not in accordance with that now established by observation. A few years after, Principal Forbes proposed his *Viscous Theory*, which, after reigning dominant in this country for fifteen or sixteen years, has recently had to submit to the rivalry of a theory which distinctly recognises the sliding of glaciers, and is based on the property of *regelation* instead of that of *viscosity*, on which the Viscous Theory was made to rest. We shall principally direct our attention to the two last mentioned of these theories.

The two leading objections against the sliding motion of glaciers were (1) that it appeared impossible that a solid glacial mass should slide at all down an irregular valley of which the inclination to the horizon should not exceed 3° or 4°; and (2) that if the mass were once to begin to move in that manner, it would necessarily move, like any other body descending an inclined plane, with an accelerated motion, and be finally projected from the mouth of its mountain valley, like an avalanche, into the plain beneath.*

These objections were apparently very formidable. The following simple experiment was devised to test their real weight:—

> 'A mass of ice was placed on a flat rough slab of sandstone, so arranged that it could easily be placed at any proposed inclination to the horizon. When the inclination was about 20°, the ice descended with an accelerated motion, as in ordinary cases; but at smaller inclinations it descended with a *slow uniform motion*, which, for inclinations not exceeding 9° or 10°, was, *cœteris paribus*, *proportional to the inclination*. The velocity was increased by an *increased weight*.' †

The motion was sensible, it seems, for an inclination of not more than half a degree, and would doubtless have been so, especially with an increase of the weight, for still smaller inclinations. The motion was due to the melting of the ice immediately in contact with the slab, for when the temperature of the air was below 32° (Fahr.) the motion was no longer sensible.

The difference between this case and the ordinary case of motion down an inclined plane, with which it has been confounded, may be easily explained. In the latter case the retarding force of friction is found experimentally to be independent of the

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* Principal Forbes expresses these objections in much the same form as in the text. He says: 'The main objection, however, is this, that a sliding motion of the kind supposed, if it commence must be accelerated be gravity, and the glacier must slide from its bed in an avalanche. The small slope of most glacier-valleys and the extreme irregularity of their bounding walls are also great objections to the hypothesis.'—'Occasional Papers,' p. 249; also published in 1855 in the 'Encyclopædia Britannica.'

† Memoir 'On the Theory of the Motion of Glaciers,' 'Transactions of the Royal Society;' read May 22nd, 1862. Also the 'Phil. Mag.' for January, 1845, and the 'Transactions of the Cambridge Phil. Soc.,' 1847.

velocity; provided always the two surfaces in contact are strong enough in their texture not to be injured by the pressure and friction between them, or by any other cause. Now, in the case of the glacier, we have explained that the temperature at its lower surface must be 32° (Fahr.), or that at which the ice there must be in a state of slow dissolution, as the necessary effect of the heat supplied from the earth beneath it. Hence, at the instant when an indefinitely thin stratum of ice at the lower surface is melted, the glacier loses its hold on its rocky bed, and is impelled by its own weight to move by an indefinitely small step onwards. It is then again obliged to wait, as it were, till the next indefinitely thin layer is melted, and so on for the consecutive steps of its motion, which, the successive intervals being infinitely small, becomes the uniform motion of the mass. The proper dynamical analogy is derived from the descent of a body in water. The body soon acquires such a velocity that the retarding force of the resistance of the water becomes equal to the accelerating force of gravity, and the body then begins to move uniformly with the velocity acquired. This velocity is called the *terminal velocity*. The uniform velocity of the glacier is its terminal velocity. The details may be seen in papers referred to in the second footnote of p. 106.

It appears from the experiments just described that the velocity of the sliding mass was increased, *cæteris paribus*, by increasing its weight,—*i.e.* the force urging it forwards was thus increased more than the resistance to the motion. Now, if any local obstacle should be opposed to the motion of a glacier, the mass would accumulate behind the obstacle; and it follows from what precedes, that the force urging the glacier forwards would be increased by the additional weight more than the resistance of the obstacle would be increased by the additional pressure or friction upon it produced by the accumulated mass. Consequently, supposing the supply of ice from the source of the glacier to be, as it is, unlimited, the glacier must, in the course of time, overcome the obstacle opposed to it, as certainly as that a river would ultimately overcome any local dam opposed to its progress. The same argument might be urged if the glacier were frozen to its bed; but since the adhesion of the particles of the ice to the bed of the glacier is undoubtedly proved by the preceding experiments to be far less than their adhesion to each other, the accumulation required in the case now supposed would probably be immensely greater than in the actual case in which the action of the bed of the glacier exercises so little power upon it to arrest entirely its motion.

We may state that the glacial mass is here supposed to have

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a degree of pliability which enables it to adapt itself to the different dimensions of its valley; but so far as the above results are concerned, it is immaterial whether the pliability be derived from actual breakings, crackings, and regelation, or from any assumed viscosity.

We can thus account then demonstrably for that part of the motion of a glacier which depends on its sliding over its bed; and, to obtain the whole motion, we have to add to this part of it the motion which results from the pliability of the aggregate glacier. Admitting this property, we at once deduce from it, without any particular calculation, the more rapid motion of the axial part of the glacier compared with its marginal parts, and that of the upper as compared with the lower surface of the mass. To this extent the problem presents no difficulty, and we have not *data* sufficient to work it out more completely. The real stumbling block in the theory has consisted in the apparent impossibility of reconciling this pliability of the aggregate glacier with the obvious characters of hardness and brittleness which belong to compact glacial ice. It is manifest that in consequence of the motion of a glacier, as above described, some parts must be extended, some compressed, and others distorted in a degree apparently quite inconsistent with the hard, crystalline structure of ice, and the preservation of its continuity. It is in the explanation which is given of this difficulty that the fundamental difference between the *Viscous Theory* and that which we may term the *Sliding and Regelation Theory* consists.

The question was answered according to the Viscous Theory, by the bold assertion that ice was really viscous. It was very difficult to ascertain without ambiguity what distinct property of matter was indicated by the term *viscous*, for no definition was ever given of it; but it is certain that those who accepted the theory generally understood the term in question in the sense in which it is ordinarily applied to tar, treacle, soft wax, and such like substances, to which it is strictly applicable according to the definition we have given of it above (pp. 99-101). It could only be in this sense, too, that it could be received by those who regarded the theory of the viscosity of ice as one of those truths which are caught by the eye of genius long before they become visible to the vulgar eye. Still the explanation given, as far as we can understand the subject, appeared to be little more than that glacial ice, in the mass, was pliable because it was viscous, and viscous because it was pliable. It was to be expected that many would object to a theory which assigned the pliability of a glacial mass to no distinctive property of matter which the author of the theory could define, and who believed that some peculiar property of ice remained to be discovered which should afford a more intelligible explanation of this pliability than the vague and misty one which was put forward by the Viscous Theory. It would seem impossible to deny that this expectation has been fully justified by the discovery of that very distinctive property of ice at the freezing temperature, which enables it, after being crushed to a fine powder, to resume its original texture and character of a transparent and continuous crystalline substance, as above described.

Principal Forbes must necessarily have been aware of the accusation of vagueness under which the Viscous Theory always laboured, and it is much to be regretted that he did not avail himself of the opportunity afforded by the publication of his 'Occasional Papers' to remove all ambiguity in his fundamental definitions. But instead of this we find the following remarks, intended as an appeal to the reader in favour of the Viscous Theory,* in which the Principal claims the credit of having laid the foundations of a *true theory* of glaciers, provided we admit the following postulates: 'First, that the limited plasticity of ice, which, when ice is exposed in the glacier to a peculiarly violent strain, necessitates the formation of an infinity of minute rents, is really a part of the Viscous Theory.' But the kind of cracking and fissuring here intimated appears to us to belong to what can only, with any regard to the accuracy and distinctiveness of scientific language, be called *solid* bodies. If the term *plastic* (which appears to be now preferred to *viscous*) were to denote a property of substances which yielded in the manner implied in the above quotation, the whole crust of the globe might, in the same sense, be said to be plastic. In that sense it designates no distinctive property of ice, or of any other substance. Secondly, our author requires us to admit, for the establishment of his claim, 'that the reconsolidation of the bruised glacial substance into a coherent whole may be effected by pressure alone acting upon granular snow, or upon ice softened by imminent thaw into a condition more plastic than ice at a low temperature, and that the terms "bruising and attachment," "incipient fissures reunited by time and cohesion," were equivalent in 1846 to the phrase "fracture and regelation" applied in 1857.' But here it must be remarked, that the bruising and breaking of the glacier was obvious to every one, as well as its reunion into a continuous mass; but many refused to believe that this reunion took place either in consequence of the property of viscosity in ice, or as the

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* The whole passage will be found in the Introduction to 'Occasional Papers,' p. xvi.

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mere direct effect of pressure acting for lengthened periods of time. In fact, it was proved by Mr. Faraday[*] that pressure is not necessary for this reunion. Intimate *contact* is the essential condition, pressure being required in the ordinary cases of glaciers to produce that contact, as well as in experiments like Dr. Tyndall's, where the reunion is to be effected between a very large number of very small angular fragments, among which the contact required to produce a regelated *continuous* mass can manifestly be practically obtained only by a sufficient amount of pressure. Nor is *time*, in the sense in which it must be understood, we conceive, in the above quotation, required for this reunion, whether the fissures be great or small, for the process is shown to be sensibly *instantaneous*. How then are we to concede the points demanded by the author of the Viscous Theory? If there be more cogent reasons for allowing them than we can find, it would, we think, have been more conducive to the establishment of the truth to state them explicitly, than to leave others what we believe to be the hopeless task of discovering them.

Let us now consider somewhat more in detail the manner in which the internal constraint of a glacial mass, considered as a hard and brittle *solid*, may be relieved consistently with the sensible preservation of its continuity. For the more simple elucidation of the problem, conceive a rod of any material which is solid, according to our previous definition of the term, and suppose it to be acted on by two equal stretching forces at its two extremities and in the direction of its length, and by equal compressing forces at opposite points along its sides, and in directions perpendicular to its length. The beam will remain in equilibrium, but will be slightly elongated and transversely compressed. If these forces continue to act, and the stretching force be sufficient to overcome the cohesion, the beam will soon become so elongated that minute disruptions of its continuity will take place, as a consequence of its extension, so that it will be on the point of being torn asunder. But before the actual dislocation should be completed, let us conceive some physical cause to be called into action which should instantly restore the continuity of the rod and the original state of its molecular constitution, so that if the stretching force were removed, the rod would have no tendency to return to its original length. That force, however, being continued, instead of immediately breaking the rod, will, on account of the supposed reconstruction of the molecular constitution, produce another elongation in it similar to the first; and thus, by successive elongations and reconstructions,

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[*] 'Glaciers of the Alps,' p. 351.

we may suppose our rod, though formed of a *solid* material, to be indefinitely elongated without being broken asunder, precisely *as if* it were perfectly plastic.

Now, there is only one solid substance known for which this continued alternate breaking and reconstruction of continuity and structure are possible; and there is only one condition under which this alternate process is possible with that particular substance. The substance is ice; the condition is, that its temperature must be 32° (Fahr.); and the process of reconstruction is that of which we denote the result by regelation. We consider our imaginary beam analogous to the ice of the glacier. The latter is broken by extension, or its structure may be broken down by compression, but the continuity and structure rise again, restored by regelation.

This explanation completely reconciles the pliability of the glacial mass with the obvious brittle and unyielding character of a hard specimen of glacial ice, by means of an experiment entirely independent of all glacial accumulations of ice, or of the phenomena attending them, and proving the existence of a peculiar and distinctive property of ice, on which the whole explanation rests. The Viscous Theory only explains the difficulty by an appeal to the phenomena which constitute the difficulty itself.

Most of our readers will be aware that there has been of late considerable discussion respecting the priority of the recognition of that *pliability* of glacial masses of which we have been speaking. M. Rendu, the late Bishop of Annecy, wrote an essay on the 'Théorie des Glaciers de la Savoie,' which was printed in Vol. X. of the 'Mémoires de la Société Royale Académique de Savoie, 1841.' Principal Forbes has made not unfrequent references to this essay, but it still remained till recently almost unknown to the glacialists of this country. It was not so much from any incompleteness, we conceive, in these references, as from the fact of most of the quotations being insulated from each other, that they entirely failed to convey to the reader any adequate idea of the essay itself; and it was not till the publication of Dr. Tyndall's 'Glaciers of the Alps' that it became at all appreciated in this country. More copious extracts from it than had before appeared are given in this work, and what is, perhaps, equally important, they are given in more continuous order,* and not in that insulated form in which they had previously appeared. In this essay it is clearly seen that the author had formed a distinct conception of the unequable motions of different parts of a glacier; of its accumulation in particular loca-

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* 'Glaciers of the Alps,' p. 299.

lities, and its attenuation in others; and, in fact, of all the principal phenomena which indicate a certain pliability in the general glacial mass, sufficient to enable it to mould itself to the local and temporary conditions to which it may be subjected, and to flow on in a manner analogous to that of a river. In Chap. VIII. he remarks:—

> 'Il y a entre le Glacier des Bois et un fleuve une ressemblance tellement complète qu'il est impossible de trouver dans celui-ci une circonstance qui ne soit pas dans l'autre. Dans les courants d'eau la vitesse n'est pas uniforme dans toute la largeur ni dans toute la profondeur; le frottement du fond, celui des bords, l'action des obstacles font varier cette vitesse, qui n'est entière que vers le milieu de la surface.'

Again, the author says (Chap. X.):—

> 'Je l'ai dit, les glaciers d'écoulement sont des fleuves d'eau solide; tous les phénomènes des fleuves s'y retracent avec une fidélité qui suffirait pour faire soupçonner leur usage: ils s'élargissent ou se rétrécissent selon la nature des bords.'

Few persons, we imagine, after reading these simple quotations, will doubt the priority of M. Rendu in the recognition of the fact that the motion of a glacier was analogous to that of a river. But it may be said, and something of the kind has been asserted, that this recognition was little more than a vague idea in his mind, which probably never assumed a form sufficiently definite to make it worthy of notice. If it had been so, and he had speculated no farther without seeing the formidable difficulty which had to be encountered in any attempt to reconcile the rigidity of ice with the pliability of a glacier, it might perhaps have been justly said that he had made only an accidental and faltering step in our knowledge of glacial movements. But let us take another quotation from his memoir. He says (p. 84, Vol. X.):—

> 'Il y a une foule de faits qui sembleraient faire croire que la substance des glaciers jouit d'une espèce de ductilité qui lui permet de se modéler sur la localité qu'elle occupe, de s'amincir, de se renfler, de se rétrécir, de s'étendre, comme le ferait une pâte molle. Cependant, quand on agit sur un morceau de glace, qu'on le frappe, on lui trouve une rigidité, qui est en opposition directe avec les apparences dont nous venons de parler. Peut-être que les expériences faites sur de plus grandes masses donneraient d'autres résultats.'

This quotation shows that he saw the difficulty before him, looked it, as it were, full in the face; felt that the scientific weapons of that day were insufficient to vanquish it; obeyed the call of sound philosophy, and stopped. Dr. Tyndall has well observed

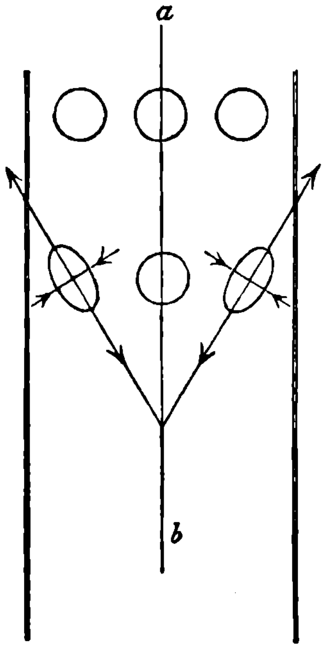
observed that he stopped where many have since stopped also, till more effective means were discovered of overcoming the difficulty in question.

And here, also, we feel ourselves called upon to observe how difficult it is, by means of a few extracts, to produce the conviction derived from the perusal of the whole memoir here spoken of, as to the caution and modesty of the author's philosophical character. Some of his views are such as have not been sanctioned by advancing science, but they are always put forth, when doubtful, with that care and reserve which, we think, appertains to the highest philosophy, and which assuredly, in the case before us, increases our confidence in the author's clearness of view on points of greater certainty. No mere extracts, however favourably chosen, could have given us the same conviction of the strength of M. Rendu's claim to priority in the case we have been discussing, as the entire perusal of his memoir. Scientific justice calls, we think, for the recognition of the Bishop's claim to the clear perception of the *pliability* of a glacier, while Principal Forbes appears to have had a stronger conviction of its importance. If the latter had subsequently established his Viscous Theory, he might well have afforded to M. Rendu the inferior merit of recognising this mere pliability of the aggregate glacier; but those who cannot admit that the term *viscosity* was ever intended to denote a property of ice equivalent to that clearly expressed by *regelation*, will scarcely regard the Principal as having laid the real foundations of a true theory of glacial motion in the Viscous Theory.

It has already been explained that the mass of a glacier will be subject to certain internal tensions and pressures due to the more rapid motion of its axial portions. The weight of the mass, the form and inclination of the glacial valley, and particular local causes, may also exert a great influence on these internal forces. When the sides of the valley are parallel, or when they are widely divergent, there are certain results, obtained by the mathematical solution of the mechanical problem thus offered to us, which are directly applicable to the actual cases of glaciers. In those cases, also, in which we are concerned with more irregular valleys, producing more irregular external forces, though, from the want of sufficient data, we may not be able to calculate the amount of the effects produced, we can often ascertain their nature and character, which is usually all that can be practically useful.

Let us first suppose the glacial valley to be elongated and of the simplest form, with parallel sides and a uniform inclination; and suppose, also, the upper and lower surfaces of the glacier to move

move with the same velocity. This will not be exactly true, but will lead to no sensible error so long as we restrict ourselves to those upper portions of the mass to which alone we can commonly penetrate. Now we have already seen that, in consequence of the more rapid axial motion, the mass will be extended in some directions and compressed in others, and it has been distinctly proved by accurate investigation,* that in the case before us the internal *tension* at any point of the mass will be the greatest in a direction pointing towards the upper extremity of the glacier and outwards towards its nearest side, and inclined to the axis at an angle of 45°. To explain the nature of the problem to which we would here direct the attention of our readers, as well as certain of the results deducible from it, we shall borrow a very simple elucidation of it from Dr. Tyndall.† In the annexed figure, *a b* represents the axis of a regular trough-shaped glacial valley, like that above described. The glacier may be represented by a quantity of any plastic or viscous substance partially filling the trough when placed in a horizontal position. Conceive three equal circles to be stamped on its surface near *a;* then if the end *a* of the trough be slightly elevated, and the opposite end at *b* be open, the viscous substance contained in it will flow in the direction *a b*, and it is found that the circle stamped on the axis retains its circular form, while the two lateral circles are transformed into the ovals represented in the figure, the longest axis of each oval being inclined at an angle of 45° to the axis *a b* of the trough, while their shorter axes are perpendicular respectively to the longer ones. This manifestly proves that the longer axis of each oval is a line of maximum extension compared with any other line through the centre of the oval, the shorter axis being in like manner a line of maximum compression. In other words, supposing the mass to have cohesive power, the longer axis of each oval must be in a direction along which the *tension* at the centre of the oval will be greater than in any other direction through that centre; and, likewise, the shorter axis of each oval must be that in which the *pressure*



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* Memoir 'On the Theory of Glaciers,' 'Phil. Transactions,' 1862.

† 'Glaciers of the Alps,' p. 383.

will be the greatest. At any point on the axis *a b* there will be neither tension nor pressure resulting from the motion of the mass. These conclusions are in exact accordance with those arrived at long ago by an exact mechanical solution of the problem.

We are hence enabled to explain the formation of the large crevasses, and their general positions; for it is manifest that when the greatest tension becomes greater than the cohesive power, a crevasse must be formed perpendicular to the direction of that greatest tension; *i. e.* it must be formed along the minor axis of each oval in the case elucidated by the figure. Consequently, the crevasses in the two marginal portions of the glacier respectively will converge towards each other as they proceed towards its higher end (*a*). In the cases of *converging* valleys, the more general solution of the problem shows that the lateral crevasses will always converge towards each other, as just described; but will make angles greater than 45° with the axis *a b*. If on the contrary the valley rapidly *diverge*, the crevasses will diverge as the lines of motion of each part of the mass diverge with the valley itself. The lower extremity of the Rhone glacier presents a most striking example of these diverging crevasses.

It should be remarked that the directions of the crevasses above determined, are those in which they will be originally formed. They remain open for a certain time, and then close up, and the ice on opposite sides of them is regeled into a continuous mass. During this time the more rapid central motion constantly tends to bring them, in parallel-sided or convergent valleys, more nearly to perpendicularity with the axis *a b*. Still they are observed to lie within the angular limits above stated, with few, or, perhaps, no exceptions. The exact positions in which large fissures will be formed may doubtless depend materially, in many cases, on local conditions; but this will not usually prevent a dominant general cause from impressing a dominant general character on the resulting phenomena. We have seen, too, that glacial ice appears to have no greater tendency to cleave in one direction than another, so that the directions of the crevasses must be determined by external causes, and not by the internal structure of the glacier.

We have already spoken of the curious phenomena of the veined structure in glacial ice (p. 94). It appears to be closely associated with the directions of greatest pressure above explained. Wherever it exists in the same locality with crevasses, the directions of the latter are stated to approximate very generally to perpendicularity with the superficial curves of structure. This law is usually observable in the marginal portions of glaciers, in which alone the more regular crevasses generally exist to any great extent; and it therefore follows that, in those localities, the surfaces of structure must be perpendicular, at every point, to the directions of greatest pressure. We can also prove this law to hold in cases where regular crevasses are either non-existent or comparatively very rare, as at the bottoms of ice-falls and along the axial portions of a glacier. In the first case, there must necessarily be an enormous longitudinal pressure from the accumulation of ice *à tergo;* and in the latter case, the axis of the glacier (as finely illustrated on the Aar glacier) is often indicated by a great central moraine, formed by the junction of two great tributaries. In such instances, the ice of the two tributary streams is forced into the same bed, and must usually produce an enormous transverse pressure in the united glacier. In the first case here cited, the structural curves are directly transverse, and in the second they are entirely longitudinal, and are consequently in both cases perpendicular to the directions of greatest pressure. All other observed cases lead to the same inference.

Some time ago Dr. Tyndall made certain experiments, which, together with others made by Mr. Sorby, led him to suppose that the cleavage structure in rocks was due to the great pressure to which they had been subjected, the planes of cleavage being perpendicular to the directions of maximum pressure. This suggested to him the idea that the veined structure might also be due, in like manner, to pressure. The analogy between the two cases is manifest; but as the theory of rock-cleavage is uncertain, that of the veined structure, so far as it rests on this analogy, must, *à fortiori*, be so likewise. Dr. Tyndall has also made experiments on the liquefaction of ice by pressure, which afford an additional presumptive proof in favour of the theory above mentioned. We must refer the reader to the 'Glaciers of the Alps' (p. 408) for an account of these ingenious experiments. Though we may not yet regard this phenomenon of the veined structure as unequivocally accounted for by the analogy and experiments here spoken of, it seems not improbable that they may lead in the path towards the right solution.

Principal Forbes also, as is well known, put forward, many years ago, his theory of the veined structure. He conceived that, as different parts of the glacier move faster or slower than the adjoining parts, two contiguous particles moving along adjoining parallel lines must generally be moving with different velocities; and thus, if in contact at any proposed instant, the one having the greater velocity would slide past the other, and in time get separated from it. Thus, suppose the velocity of every particle in a vertical plane parallel to each side of a

regular

regular canal-shaped glacier to move with the same velocity, and suppose the axial parts of the mass to move the fastest; then will every particle in one of these planes tend to slide past the neighbouring particle in one of the adjoining planes; and thus there will be a tendency to make the whole of one of these planes slide on the surface of the adjoining one, and thus also to break the cohesion between them. It was in this presumed bruising and rupturing along these parallel planes that the author of this theory considered the veined structure to originate. His first idea was that a greater facility was thus afforded for the infiltration of water between those bruised laminæ, and that this infiltrating water became frozen by the winter cold, and formed the more compact and transparent ice of the laminæ. A real physical cause was thus assigned for the veins, but it was entirely inconsistent with the internal temperature of the glacier, into which, as above explained, the winter cold does not penetrate many feet. The idea was afterwards abandoned, but I am not aware that the author substituted for it any other physical cause. The veins appear to have been attributed only to the bruising of the mass, as above described, and therefore to a mechanical rather than to any determinate physical cause.

Principal Forbes did not determine the positions of the planes or surfaces of the veins, as above, by the simple consideration of the relative motions of contiguous particles, which, in a canal-shaped glacier, would give the marginal lines of structure necessarily parallel to the sides,—a direction from which they are often observed to deviate very considerably. His explanation was, that a *drag* towards the centre of the glacier, in consequence of its more rapid motion there, caused an oblique motion of the marginal particles. This explanation was founded on a demonstrable mechanical error;* and the Ripple Theory, by which he attempted to explain his conclusions, has now been proved to be entirely fallacious.† Again, it has been stated that under the great central moraine of the Aar glacier the veined structure is very finely developed where there can be no difference of motion in adjoining particles. It is also impossible, in our opinion, to give any real explanation of the positions of the surfaces of structure near the foot of an ice-fall, consistent with this theory.

If the surfaces of structure be considered as due to the actual difference of motion of contiguous particles, the problem becomes only a geometrical one, and we conceive it to have been shown demonstrably that the positions of the veins or surfaces of

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* See References in second footnote, p. 106.

† 'Glaciers of the Alps,' p. 398.

structure

structure could not coincide in that case with their observed positions.* It is impossible, we think, to accept this theory if Principal Forbes's 'differential motion' of two contiguous particles means the actual difference between their instantaneous motions; and yet, if it do not mean this actual difference, it is inconceivable to us what intelligible meaning can be assigned to it.

Priority in the observation of this phenomenon of the veined structure, immediately after Principal Forbes had remarked it in 1841, was made a subject of controversy. M. Agassiz stated himself to have previously observed it; but in his 'Système Glaciaire' (p. 208) he claims for M. Guyot the credit of having first distinctly noted this structure in 1838 on the *Glacier du Gries.* In support of this claim he gives a quotation from a communication made by that observer to the Swiss naturalists at Bâle in the year just mentioned, and which is now placed in the archives of the Society of the Natural Sciences at Neuchâtel. The quotation is too long for insertion here, but we may cite the following passage from it as in itself conclusive. M. Guyot says that, being on the Glacier du Gries,—

> 'Je vis sous mes pas la surface du glacier entièrement couverte de sillons réguliers de 1 ou 2 pouces de largeur, creusés dans une masse à demi-neigeuse, séparés par des lames saillantes, d'une glace plus dure et plus transparente. Il était évident que la masse du glacier étoit ici composée de deux sortes de glace, l'une, celle des sillons, encore neigeuse et plus fusible, l'autre, celle des lames, plus parfaite, cristalline, vitreuse, et plus résistante, et que c'était à l'inégale résistance qu'elles présentaient à l'action de l'atmosphère qu'était dû le creux des sillons et la saillée des lames plus dures.'

It was at once admitted, we believe, by Principal Forbes himself and all other glacialists, that the evidence in favour of M. Guyot's priority of discovery was established. Principal Forbes's claims, as regards these phenomena, do not rest on the precedency due to his observations, but on his recognition of the importance of this peculiar and curious structure as a general character of glacial ice.

The difficulty of explaining the adequacy of the forces acting on a glacier to enable it to overcome the numerous and apparently insurmountable obstacles to its motion, has always been one which has been more or less experienced by most glacialists. A prevailing idea has been that the lower portions of a glacier are crushed simply by the weight of the superincumbent mass—that the cohesion of those portions is thus destroyed and the mass

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* Memoir in the 'Transactions of the Royal Society,' 1862, p. 725.

pushed

pushed outwards, where it meets with the fewest obstacles. And yet the Peak of Teneriffe, for example, does not crush its basal strata into atoms, and thrust out its own foundations. If it were possible that the weight of that mountain could be suddenly superimposed on terrestrial rocks which had been solidified under a comparatively small pressure, it seems probable that those rocks would be thus crushed into powder, if sufficiently brittle; but Nature does not work in this manner. She educates the back, as it were, to prepare it for the load it has to bear, by the slow and gradual superposition of the superimposed weight. And similarly if a stratum of ice, frozen under the mere pressure of the atmosphere, could be placed under the weight of a glacier at a temperature below 32° (Fahr.), it would be instantly crushed into powder, and its cohesive power so far destroyed as to make it capable of being thrust outwards on a horizontal plane by a comparatively small vertical force. But if the temperature should be exactly 32° (Fahr.), as in the lower parts of a glacier, the structure and cohesion of the crushed ice would be immediately restored by regelation, and it would be, at least, an apparent contradiction to suppose that the ice would be again crushed by the pressure under which it had just before been regeled and consolidated. We doubt whether any mass of ice producing a pressure within the limit of regelation (if there be such a limit) could squeeze out its lower portions on a horizontal plane, so as to produce any continuous motion like that of a glacier. It is the resolved part of the force of gravity parallel to the bed of the glacial valley (always inclined to the horizon) which we conceive to be the force really effective in urging onwards every part of the glacier.

Principal Forbes appears to have been impressed with the difficulty of assigning an adequate cause for the crushing effects which he supposed to be produced in the interior of a glacier, and by which the cohesion was destroyed and its motion facilitated, as if it were viscous. He says that a considerable quantity of water is constantly percolating through the minute fissures of the mass, or held by them in capillary suspension, and that this water 'exercises a tremendous hydrostatic pressure' to push onwards the whole mass in the direction of least resistance.* Now, we feel ourselves bound to assert that this conclusion is founded on an entire misconception of the mechanical action of this internal water. Admitting the existence of the capillary fissures, filled with water throughout the glacier, what would be the con-

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* 'Occasional Papers,' p. 165. See also a Memoir in the 'Transactions of the Royal Society for 1846,' Part III.

sequence of the supposed enormous hydrostatic pressure in these minute internal tubes and fissures? The answer is obvious: the water would rush forth from every crevice on the exterior surface of the mass. No such *hydrostatic* pressure, therefore, can exist. In fact, if the water be held at rest in capillary crevices, it will transmit no hydrostatic pressure whatever, but will simply, by its own weight, increase the weight of the mass. Again, if the water flow through these minute fissures with a *steady* motion (and such its motion must be very approximately), it will produce no hydrostatic pressure at all. The truth of both these assertions may be strictly proved,* and is, in fact, sufficiently obvious to any one familiar with such investigations. We have here an example of the incautious appeals which have been made to mechanical principles in the solution of certain glacial problems.

It is to the small adhesion of the lower surface of the glacier to its bed, that the enormous power of the internal forces to crush and dislocate the general mass is due. The smallness of this adhesion in a glacier presents a case similar to that of a long beam in a horizontal position, supported principally by forces acting at its two extremities. The more exclusively this force is thrown on these extremities, the more likely will be the beam to break by its own weight. And thus will the glacier be the more likely to be dislocated when the principal forces opposing its motion act along its flanks, while the axial portions are comparatively little impeded by the small friction on the lower surface of the mass.

The subjects we have been discussing involve a degree of complexity which may render it desirable, for the clearer comprehension of them, that we should give a brief summary of the contributions which different glacialists have made since the time of De Saussure, to our knowledge of glacial facts and glacial theories. We have already spoken of Rendu's Memoir, and of the claim which it establishes for him of having been the first to recognise clearly and distinctly the pliability of a glacier, and that it moved, speaking generally, *as if* ice were a viscous substance, and in a manner resembling that in which the water of a river moves. Guyot's claim to having been the first to observe and to describe clearly the veined structure, we conceive to be unequivocally established. Agassiz has probably done more than any other man to diffuse a general interest in glacial subjects throughout the scientific world. He was enabled to accomplish

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* Memoir 'On the Theory of Glaciers,' 'Transactions of the Royal Society.' Read May 22, 1862.

this by his high reputation and wide acquaintance among men of science, and the esteem in which he was held by them, as well as by his zealous activity as an observer, although his physical theories were never received with much favour. His second work, the 'Systême Glaciaire,' remains the most copious deposit of accurate and careful observations which we possess on many glacial phenomena, as his map of the Aar is by far the finest topographical record we have of any glacier and its superficial phenomena. Principal Forbes's 'Travels in the Alps' is also a work full of interesting matter relating to Alpine glaciers generally, and his sojourn among them. His researches were unwearied, and he acquired and communicated to us a large amount of general and detailed knowledge of glacial phenomena. For this, and for the general interest with which he helped to invest the subject, we consider the scientific world to be greatly indebted to him. The prevailing defect of his observations is that they are subordinated too much to two dominating ideas, the viscosity of glacial ice and his supposed origin of the veined structure; and therefore it is that his observations, though extending generally over a wider range than those of M. Agassiz, are less valuable in many cases where greater detail and minuteness are essential. We think that for many years imperfect justice only has been meted out to the 'Systême Glaciaire' in this country, and that our estimate of the claims of its author, as well as those of some few other foreigners, may have been perhaps, if we may use the expression, somewhat too insular. There is scarcely any part of Principal Forbes's speculative theories to which we can assent, and it is quite certain that much of his mechanical reasoning is altogether erroneous. Mr. Hopkins was the first to explain the sliding of glaciers and their unaccelerated motion. He has also applied accurate methods of investigation to the solution of many of the mechanical problems which glacial theory involves. Dr. Tyndall in recognising the necessity for precise definitions, and for exact modes of research both in the mechanical and experimental branches of the subject, has afforded excellent aid to the advance of glacial theory. He has done good service also in the observations he has made; but it is in the substitution of a determinate and beautiful experimental result for a hypothesis unfounded on any determinate property of matter that he has rendered the greatest service in this department of science. The results of regelation explain exactly what glacial theory required to be explained; but they do not effect this through the medium of viscosity. Regelation does not *use* viscosity, but *supersedes* it, and renders not merely the word itself, but any definite idea which has ever been attached to it, useless in all

exact

exact reasoning on the subject. We cannot refrain from appealing in the name of exact science, and on behalf of the rightful claims of exact philosophers, against the merging of the definite into the indefinite, or the *Sliding and Regelation Theory* into the *Viscous or Plastic Theory*.

In what we have said on regelation we have been anxious to point out that the value of the actual results of regelation is little diminished for the glacialist by our ignorance of the exact *modus operandi* by which those results are produced. The theory of gravitation might be advanced if some astute philosopher could prove that gravity was only the effect of some still simpler property of matter; but Physical Astronomy, in the sense in which that term is used at present, could scarcely be thereby rendered more complete than it is; and so, though the process of regelation may hereafter be explained, the discovery of the results of that process will not the less constitute a decided and independent step in glacial science, and one which, we believe, will always hereafter be recognised as such.

We have already mentioned the name of M. de Charpentier, but we should not do justice to him if we did not recognise his claim as having been one of the first glacialists, though preceded several years by M. Venetz, to direct attention to the former great extension of the Alpine glaciers, as manifested by the enormous masses of blocks and débris, which have evidently been derived from distant localities, and the transport of which he attributed to the agency of glaciers. But this is a subject which our space will not allow us to discuss; and, in fact, it may rather, perhaps, be regarded as belonging to the wide domain of Geology than to the more restricted one of Glacial Theory. In the later history of our planet it has opened to us a new and interesting page which has yet been but imperfectly deciphered, and which can only be truly interpreted by the combined efforts of the geologist and the glacialist. We confess that we are not without apprehension that many geologists may be disposed to accept theories in which the action of glaciers is the leading agency, without due regard to those mechanical and physical principles to which the motion of glaciers must in all cases be subordinated. We shall take one important point to elucidate our meaning. We have seen (p. 79) that below the snow-line the thickness of a glacier decreases from year to year, principally by the melting away of its superficial portion. Suppose the thickness of a glacier at any assigned point of its course to be 1000 feet, and to diminish 10 feet annually. Let us further suppose the glacier to move at the rate of 400 feet a year. Then for every 400 feet in the length of the glacier, measuring towards its

lower

lower extremity, there will be a decrease of 10 feet in the thickness, which will consequently be reduced to zero at the distance of 40,000 feet, or about 7½ miles, which would be the length of the glacier below the point at which its thickness has been supposed to be 1000 feet. The wasting of 10 feet annually in thickness is very nearly the estimate of M. Agassiz, founded on careful experiments, made on the glacier of the Aar, near the junction of its two great tributaries, and at the height of some 8000 feet above the sea. The motion of 400 feet in a year is greater than the mean annual motion of the Aar glacier, and less than that of the Mer de Glace. It may be taken as a sufficiently near approximation to the mean motion of the Alpine glaciers.

Let us take an actual example, analogous to the imaginary one above given. Erratic blocks exist on the flanks of the Jura opposite the mouth of the valley of the Rhone, at the height of at least 3000 feet above the Lake of Geneva, and it is universally allowed that they must have been transported by some means or other from different localities in the valley of the Rhone. The favourite theory at present appears to be that their transport was effected by a glacier which descended the whole length of the valley just mentioned, and thrust itself across the central Swiss valley, to deposit its burden of blocks on the sides of the Jura in the form of a terminal moraine. We have here no intention of discussing the truth of this theory; we wish simply to point out some difficulties which, it would appear, have not engaged the attention of those who advocate it. According to Charpentier, the highest lines of erratic blocks may be distinctly traced along the sides of the Rhone valley, their elevation on either side of it at Martigny being about 2500 feet, and at the mouth of the valley 2300 feet above the river. We take these heights as indicative of the depth of the ancient glacier between the two places just mentioned. Now let us conceive the conditions as to the motion of the glacier and the rate of its wasting away to be the same as in the imaginary case above taken, or very nearly the same as in the Aar glacier at the present time; and let us also suppose, to make the analogy complete, that the valley of the ancient glacier was continued beyond its present termination. It then follows, from a calculation like the above, that it must have extended some 15 miles beyond the mouth of the present valley. But this ancient glacier, instead of continuing along a trough-shaped valley, must have debouched into the open plain of Switzerland, and thus have been at liberty to diverge in nearly all directions within a semi-circle, like the glacier of the Rhone from the foot of its fall. Consequently, the external surface

exposed

exposed to the dissolving influences of the sun's rays, and of the atmosphere, would be much increased, and the thickness of the glacier would be reduced to zero long before its remoter boundary had attained a distance of much less than 15 miles. And here it will be observed, that the temperature of the Swiss valley is tacitly supposed to be reduced to that of the middle region of the Aar glacier, at an altitude above the sea of about 8000 feet. Nor would that, as our calculation tells us, be cold enough to secure the protrusion of the glacier, as above supposed, to the flanks of the Jura, a distance of 50 or 60 miles. In fact, it would be necessary that the temperature at the level of the Lake of Geneva should be lower than that of the snow-line, *i. e.*, lower than the present mean temperature in the Alps at the height of about 10,000 feet. It would be useless to talk of this enormous depression of temperature being produced by any peculiar disposition of land and sea. The only conceivable terrestrial cause to which it could be chiefly referred, must be the natural elevation of the whole region to the amount just stated. Then the glacial mass in the Swiss valley would not melt away, as it would below the snow-line, in its transit to the Jura, which it would reach provided the fall between the mouth of the Rhone valley and the top of the Jura chain were sufficient to secure its motion in that direction. This fall would require the Alps to be raised some 4000 or 5000 feet more than the hills of the Jura. Mont Blanc would thus become nearly 30,000 feet high, while all the lower regions surrounding it would be raised to an elevation of 10,000 or 12,000 feet above the level of the sea—consequences which might well alarm the boldest catastrophist, and dispose us to search carefully, before we finally admit them, for some simpler mode of transporting erratic blocks from the Alps to the Jura.

The great difficulty which besets all theories involving an extreme extension of ancient glaciers in Western Europe, arises from the apparent impossibility of assigning any adequate terrestrial cause, except that of extreme elevation, for the enormous depression of temperature in these temperate latitudes, which such theories tacitly demand. Terrestrial causes for considerable variations of climatal temperature have been assigned, depending on the influences of warmer or colder ocean currents, and of possible changes in the disposition of sea and land; but it would be futile to attribute to such causes the immense depression of temperature required in a case like that discussed above. But our immediate object is not to discuss the various causes by which terrestrial temperature may be affected, but to remind geologists of the physical impossibility so clearly indicated by established glacial

facts

facts and theories, of any prolongation of glaciers beneath the snow-line, beyond those limits which may be consistent with the extent of such prolongation calculated as in the preceding example. At present we have only to recommend that in framing the theories which erratic blocks may suggest to us, we should endeavour to bring them into strict accordance with the mechanical and physical principles which govern the motions of existing glaciers, as well as with all associated geological phenomena, and thus to establish that harmony of which we have spoken in the commencement of this review, as the final and most perfect test of scientific truth.

ART. IV.—1. *The Empire. A series of Letters published in 'The Daily News' in* 1862 *and* 1863. By Goldwin Smith. Oxford and London. 1863.

2. *Lectures on Colonization and Colonies, delivered before the University of Oxford in* 1839, 1840, *and* 1841. By Herman Merivale, A.M., Professor of Political Economy. New Edition. London. 1861.

3. *Reports of the Past and Present State of Her Majesty's Colonial Possessions, transmitted with the Blue-books for the Year* 1860.

4. *Twenty-first General Report of the Emigration Commissioners.*

5. *Letter to the Right Hon. Benjamin Disraeli, M.P., on the present Relations of England with the Colonies.* By the Right Hon. C. B. Adderley, M.P. New Edition. London. 1862.

THAT it should even be made a matter of question by any, whether Great Britain shall retain her colonial possessions, is something new and strange. But since there are men among us, and men of accomplishment and ability, who take the negative side, and who would resent the imputation that their words are no more than the casual effusion of a passing and thoughtless grumble, we must require them and others to bestow somewhat ampler reflection upon the subject.

The arguments of those who, like Mr. Goldwin Smith, inculcate the necessity of dismembering the colonial empire are obvious and simple, and based on the narrowest possible view of a few facts, excluding from consideration many facts of far greater importance. These arguments are generally stated as follows: Colonies 'do not pay.' They are useless for the purposes of commerce, and too costly for the purposes of power. Since the recognition of the principles of Free-trade by the leading statesmen of the great parties, they are superfluous for the