in case the *kajak*-messenger should catch the steamer without inducing her to wait for us, I write these few lines just to inform you that we are all alive and well.

As you will know, we left the *Jason*, the Norwegian sealer, on July 17, and expected to reach the shore the next day. But in this we were sadly disappointed. Screwing ice, maëlstroms, impassable ice, where it was alike impossible to row or to drag the two boats, stopped us. One of the boats was stove in, but we got it repaired again. We drifted seawards at a speed of thirty sea miles in the twenty-four hours. Drifted in the ice for twelve days. Strove hard to get to the shore, were three times on the point of succeeding, but were as often carried out to sea again by a current stronger than our power of rowing. Were once, for a whole day and night, very near perishing in tremendous breakers of the sea against the ice-rim. After twelve days' drifting about, we managed at last to get ashore near Andretok, north of Cape Farewell, at 61° and some minutes of northern latitude. We rowed again northwards, reaching Uminik, from which point the crossing of the inland ice began on August 15.

We directed our course for Christianshaab on the western coast. Encountered severe snowstorms and had heavy ground. Estimating that it would be too late to reach Christianshaab in time for this autumn's vessel, we altered our course and steered for Godthaab, the ice-fields in that direction having, besides, been hitherto trodden by no one. After altering course, reached height of 10,000 feet, with temperature of 40° to 50° C. below zero. For several weeks we remained at an altitude of over 9000 feet. Tremendous storms, loose, new-fallen snow, enormously difficult passage. Towards end of September we reached at last the western side above Godthaab. Had a perilous descent, on ugly and very uneven ice, but got safely down to Ameralik Fjord. Managed to build a kind of boat from floor of tent, bags, bamboo reeds, and willow branches. In that frail craft Sverdrup and I rowed away, and arrived here on October 3. The four men are left at Ameralik, living there on short rations fare, but will be sent for as soon as possible.

There you have in short outline our Saga. We are all perfectly well, and everything has been in the best order. I hope that we may catch this steamer, and that instead of this letter you may see our sunburnt faces.

With many greetings, yours ever devotedly,

FRITHIOF NANSEN.

P.S.—Just now the *kajak*-men must leave, profiting by the favourable weather. They have 300 miles to make before getting to Ivigtut.

The following is a translation of Mr. Sverdrup's letter:—

GODTHAAB, *October* 4, 1888.

We arrived safely here yesterday after forty-six days' wandering from east to west. It did not prove so easy to get on shore from the *Jason* as we had expected. We got into formidable ice-screwings, and the current took us southwards and out from the shore, so that we had twelve days' very hard work before getting to land, and that 300 miles more to the south than we had intended. We began at once to work back along the coast, and this took us another twelve days, so that we did not begin our crossing of the ice before August 15. The ascent was very fortunate, as we chanced to find comparatively easy ice to climb up. We shaped our course for Christianshaab, but after getting up to 7500 feet we were attacked by a northern snowstorm. We resolved, therefore, to set our course for Godthaab, the distance being shorter, and there being a better chance of favourable winds. I may truly say that we had a hard time of it. The snow and ice were very heavy, and the weather was trying. For nearly three weeks we were up at nearly 10,000 feet, and had a temperature of −40° to −50° C. Only for four days were we snowbound. After all, we have to be thankful it was not worse. After getting down from the inland ice on the western coast, we had before us some ninety miles of barren country, of which the half lay along a fjord. We tried to cross here, but found it too hard work; so we managed to construct a kind of boat from the bottom of the tent and some bags, and in that, after four days' rowing, Dr. Nansen and I reached here, where we had the most cordial reception from all the inhabitants of the colony. Two boats have now been sent to the bottom of the fjord to fetch our comrades. The regular vessel has long since left, but some 250 or 300 miles further south there is supposed to be, at Ivigtut, a steamer loading for Copenhagen, and we are now sending a *kajak*-message in order to stop that steamer if possible. We have but little hope of that, however, and are preparing to pass the winter here. That may be very comfortable after all, but of course we would prefer getting home. I must hurry up, as we are now going to dine with the parson, and, in fact, we have not had time for anything, as since arriving here we have gone from one social party to another. You may see from that how well we are off. I was the only one of our whole party who got over all the tremendous fatigues without the smallest ailment. I am, and have been all the time, as fresh and sound as a fish.

DR. GEORG SCHWEINFURTH has started upon an Oriental journey. He is going to Arabia first, to continue his studies of the coffee-plant.

THE FOUNDATION-STONES OF THE EARTH'S CRUST.[1]

Do we know anything about the earth in the beginning of its history—anything of those rock masses on which, as on foundation-stones, the great superstructure of the fossiliferous strata must rest? Palæontologists by their patient industry have deciphered many of the inscriptions, blurred and battered though they be, in which the story of life is engraved on the great stone book of Nature. Of its beginnings, indeed, we cannot yet speak. The first lines of the record are at present wanting—perhaps never will be recovered. But apart from this—before the grass, and herb, and tree, before the "moving creature in the water," before the "beast of the earth after his kind,"—there was a land and there was a sea. Do we know anything of that globe, as yet void of life? Will the rocks themselves give us any aid in interpreting the cryptogram which shrouds its history, or must we reply that there is neither voice nor language, and thus accept with blind submission, or spurn with no less blind incredulity, the conclusions of the physicist and the chemist?

The secret of the earth's hot youth has doubtless been well kept. So well that we have often been tempted to guess idly rather than to labour patiently. Nevertheless we are beginning, as I believe, to feel firm ground after long walking through a region of quicksands; we are laying hold of principles of interpretation, the relative value of which we cannot in all cases as yet fully apprehend—principles which occasionally even appear to be in conflict, but which will some day lead us to the truth.

I shall not attempt to give you an historical summary, but only to lay before you certain facts for which I can answer, and to indicate the inductions which these, as it seems to me, warrant. If I say little of the work of others, it is not from a desire of taking credit to myself, but because it is immaterial for my present purpose who first made a particular observation and how far his inductions therefrom were correct. The acknowledgment of good work would involve repudiation of bad, and for that, so far as persons are concerned, it seems hardly fair to use the present occasion. So, in the outset of this lecture, I will once for all make a statement which I have sometimes thought of invariably using, like a prefatory invocation, "You are free to suppose that everything herein has been said by somebody, somewhere," but I will add that, as far as possible, every assertion has been personally verified.

The name Cambrian has been given to the oldest rocks in which fossils have been found. This group forms the first chapter in the first volume, called Palæozoic, of the history of living creatures. Any older rocks are provisionally termed Archæan. These—I speak at present of those indubitably underlying the Cambrian—exhibit marked differences one from another. Some are certainly the detritus of other, and often of older, materials—slates and grits, volcanic dust and ashes, even lava-flows. Such rocks differ but little from the basement-beds of the Cambrian; probably they are not much older, comparatively speaking. But in some places we find, in a like position, rocks as to the origin of which it is more difficult to decide. Often in their general aspect they resemble sedimentary deposits, but they seldom retain any distinct indications of their original fragmental constituents. They have been metamor-

[1] An evening discourse, delivered at the Bath meeting of the British Association, by Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., &c

phosed, the old structures have been obliterated, new minerals have been developed, and these exhibit that peculiar orientation, that rudely parallel arrangement, which is called foliation. Except for this some masses are fairly homogeneous, while some exhibit a distinct mineral banding which is usually parallel with the other structure. These rocks are the gneisses and schists—the latter term, often vaguely used, I always restrict to rocks which exhibit a true foliation. In some schists the mineral constituents are comparatively minute, in others they are of considerable size. In the former case we may often venture to affirm that the rock is a metamorphosed sediment; in the latter its original condition is a matter of conjecture. Rocks of the former class often appear, to use no stronger word, to lie above, and so to be less ancient than those of the latter, and beneath that comes a coarser and more massive series, in which granitoid rocks are common. In these last foliation is often inconspicuous, and the rocks in consequence are not markedly fissile.

That these rocks are older than the Cambrian can often be demonstrated. Sometimes it can even be proved that their present distinctive character had been assumed before the overlying Cambrian rocks were deposited. Such rocks, then, we may confidently bring forward as types of the earth's foundation-stones. As the inscriptions buried in the Euphrates Valley tell us the tongue of Accad in the days prior to the coming of the Semite, so these declare what then constituted the earth's crust. If in such rocks we find any peculiarities of mineral composition or structure, these may legitimately be regarded as distinctive. We have only to beware of mistaking for original those which are secondary and subsequently impressed.

In other parts of the world we find rocks of like characters with those above named, the age of which cannot be so precisely fixed, though we can prove them to be totally disconnected from and much older than the earliest overlying stratum. To assert that these rocks are contemporary with the others is obviously an hypothesis which rests on the assumption that community of structure has some relation to similarity of origin. I am well aware that attempts have been made to discredit this. But if we eliminate difficulties which are merely sophistical—those, I mean, created by the use of ambiguous or misleading terms—if we acknowledge those due to our limited means of investigation, such as that of distinguishing a rock crushed *in situ* from one composed of transported fragments—in other words, of separating in every case a superinduced from a primary structure, and if we allow for others due to the limitation of our instrumental and visual powers, I do not hesitate, as the result of long and, I hope, careful work, to assert that certain structures are very closely related to the past history of a rock, and that in very many instances our diagnosis of the cause from its effect is not less worthy of confidence than that of an expert in pathology or physiology. Resemblances of structures, different in origin, do, no doubt, sometimes occur—resemblances not seldom due to partial correspondence in the environments; but in regard to these it is our duty to labour patiently till we succeed in distinguishing them. The difficulty of the task does not justify us, either in abandoning it in despair, or in sitting down, after a few hasty observations, to fashion hypotheses which have no better foundation than our own incompetence or idleness.

As it is impossible in the time at my disposal to demonstrate the proposition, I must assume what I believe few, if any, competent workers will deny, that certain structures are distinctive of rocks which have solidified from a state of fusion under this or that environment; others are distinctive of sedimentary rocks; others again, whatever may be their significance, belong to rocks of the so-called metamorphic group. I shall restrict myself to indicating, by comparison with rock structures of which the history is known, what inferences may be drawn as to the history of the last-named rocks, which, as I have already stated, are in some cases examples of the earth's foundation-stones, while in others, if they are not these, they are at any rate excellent imitations.

Let us proceed tentatively. I will put the problem before you, and we will try to feel our way towards a solution. Our initial difficulty is to find examples of the oldest rocks in which the original structures are still unmodified. Commonly they are like palimpsests, where the primitive character can only be discerned, at best faintly, under the more recent inscription. Here, then, is one of the best which I possess—a Laurentian gneiss from Canada. Its structure is characteristic of the whole group; the crystals of mica or hornblende are well defined, and commonly have a more or less parallel arrangement; here and there are bands in which these minerals are more abundant than elsewhere. The quartz and the felspar are granular in form; the boundaries of these minerals are not rectilinear, but curved, wavy, or lobate; small grains of the one sometimes appear to be inclosed in larger grains of the other. Though the structure of this rock has a superficial resemblance to that of a granite of similar coarseness, it differs from it in this respect, as we can see from the next instance, a true granite, where the rectilinear outline of the felspar is conspicuous. Here, then, is one of our problems. This difference of structure is too general to be without significance. What does it mean?

It is more difficult to obtain examples of schist of like geological age, wholly free from subsequent modification. Apparently the structure and composition of the rock have rendered it more liable to disturbance. But those exhibited, though by no means perfect examples, may serve to indicate the structure of an Archæan schist, consisting mainly of quartz and mica. We may take them as representative of a considerable series of rocks, which are often associated in such a manner as to suggest that, notwithstanding their present crystalline condition, they had a sedimentary origin. Can this inference be justified?

How shall we attack this problem? Clearly, the most hopeful way is by proceeding from the known to the unknown. Now, among the agents of change familiar to geologists, three are admittedly of great importance; these are water, heat, and pressure. As probably almost all changes in nature, with which we have to deal, have occurred in the presence of water, but those due to it alone are generally superficial, I shall assume its presence, and not attempt to isolate its effects. But we must endeavour to ascertain the results of pressure and heat, when acting singly and in combination, in modifying rocks of a known character; admitting, however, that probably while the one agent has been dominant, the other has not been wholly inoperative.[1]

The first effect of pressure due to great earth-movements is to flatten somewhat the larger fragments in rocks, and to produce in those of finer grain the structure called cleavage. This, however, is a modification mainly mechanical. It consists in a re-arrangement of the constituent particles, mineral changes, so far as they occur, being quite subordinate. But in certain extreme instances the latter are also conspicuous. From the fine mud, generally the result of the disintegration of felspar, a mica, usually colourless, has been produced, which occurs in tiny flakes, often less than one-hundredth of an inch long. In this process, a certain amount of silica has been liberated, which sometimes augments pre-existing granules of quartz, sometimes consolidates independently as microcrystalline quartz. Carbonaceous and ferruginous constituents are respectively converted into particles of graphite and of iron oxide. Here is an example of a Palæozoic rock, thus modified. It originally consisted of layers of black mud and gray silt. In the former, this filmy mica has been abundantly developed; it is present also, as we might expect, to some extent in the latter. Observe that the original banded structure, notwithstanding the pressure, has not been obliterated. Another point also demands notice. The black lines in the section indicate the direction of the cleavage of the rock, which is, roughly speaking, at right angles to the pressure which has most conspicuously affected the district, while the microfoliation, as we may call it, appears to be parallel to the original bedding, and is thus anterior to the dominant cleavage. The two may form parts of a connected series of movements, but, at any rate, they are so far separated that the pressure which produced the one, acted, roughly speaking, at right angles to that which gave rise to the other, and the folia were developed before they were bent and torn.

Let us now pass on to examine the effects of pressure when it acts upon a rock already crystalline. Here, obviously it is comparatively unimportant whether the original rock was a true granite or a granitoid gneiss; for at present we are only concerned with the effect of pressure on a fairly granular crystalline rock. But in the resultant structures there are, as it seems to me, differences which are dependent upon the mode in which pressure has acted. They are divisible into two groups: one indicating the result of simple direct crushing, the other of crushing accompanied by shearing. In the former case, the rock

[1] Heat will, of course, result from the crushing of rock. This some consider an important factor in metamorphism, but I have never been able to find good evidence in favour of it, and believe that as a rule the rocks yield too slowly to produce any great elevation of temperature.

mass has been so situated that any appreciable lateral movement has been impossible; it has yielded like a block in a crushing-machine. In the latter, a differential lateral movement of the particles has been possible, and it has prevailed when (as in the case of an overthrust fault) the whole mass has not only suffered compression, but also has travelled slowly forward. Obviously, the two cases cannot be sharply divided, for the crushing up of a non-homogeneous rock may render some local shearing possible. Still it is important to separate them in our minds, and we shall find that in many cases the structure, as a whole, like the cleavage of a slate, results from a direct crush; while in others the effects of shearing predominate. The latter accordingly exhibit phenomena resembling the effects of a tensile stress. Materials of a like character assume a more or less linear arrangement, the rock becomes slightly banded, and exhibits, as has been said, a kind of fluxion structure. This phrase, if we are careful to guard ourselves against misconception, is far from inappropriate. The mass gradually assumes a fragmental condition under the pressure, and its particles as they shear and slide under the effects of thrust, behave to some extent like those of a non-uniform mass of rock in a plastic condition, as, for example, a slaggy glass. But we must be on our guard, lest we press the analogy too far. The interesting experiments which have been made on the flow of solids, and on rolled-out plastic substances, while valuable as illustrations, represent, as it seems to me, a condition of things which must be of rare occurrence in a rock mass, pulverized by mechanical forces only. If I am to reason from them, I must regard the rock not as a fragmental solid—if the phrase be permissible—but as an imperfect fluid; that is to say, I must consider them as illustrative of structures in rocks, which have yet to assume—not have already assumed—a crystalline condition.

Illustrations of the effects of direct crushing in a granitoid rock are common in the Alps. Those of a shearing crush are magnificently developed near the great overthrust faults in the north-west Highlands of Scotland.

In the former case, where a granitoid rock has been affected only to a moderate extent, and the resulting rock in a hand specimen would be called a gneiss without any very definite mineral banding, we find that under the microscope it exhibits a fragmental structure, the felspars are often somewhat rounded in outline, are frequently rather decomposed and speckled with minute flakes of white mica of secondary origin, and commonly seem to "tail off" into a sort of stream of microlithic mica, which has doubtless resulted from the destruction of felspar, the residual silica making its appearance as minutely crystalline quartz. The original quartz grains have been broken up, and are now represented by smaller grains, often in rudely lenticular aggregates, like little "inliers" of quartzite. The original flakes of black mica have been tattered and torn, and now appear as streaky clusters of flakelets, often less than one-sixth the original length. In extreme cases of crushing, the felspar has almost disappeared; the constituents are all reduced in size, and the rock at first sight would no longer be called a gneiss, but a fine-grained mica-schist. It has become extremely fissile, and the flat faces of the fragments exhibit a peculiar sheen, as if it had received a varnish of microlithic mica. In short, from a granitoid rock a microcrystalline mica-schist has been produced, which, however, differs markedly from the rock to which that name is ordinarily applied.

Let us now turn to a rock of similar nature, in which the effect of shearing is more conspicuous. I have selected a specimen, in which, as in the first example above, some of the felspar still remains in recognizable fragments. These, however, are commonly destitute of the "tail" of mica-microliths, and bear, at first sight, some resemblance to the broken porphyritic felspars which occur in a rhyolite. The mica, whether primary, but fragmental, or secondary, tends to get associated in undulating layers; the quartz also has a more uniform aspect and a more linear arrangement. In the most extreme cases the felspar all but disappears (though I fancy that it has here a better chance of surviving), the quartz and the mica are more and more aggregated in definite but thin bands, and the former, when viewed with crossing nicols, exhibits streaks, which, for a considerable distance, are almost uniform in tint, as if its molecules under a stress definite in direction had acquired a polarity, so that groups of these act upon light almost like a single crystal.

The effects of mechanical deformation, followed by mineral change, are also remarkably conspicuous in the case of pyroxenic rocks. Augite, it is well known, is by no means a stable mineral, and under certain circumstances is readily transformed into hornblende. This occurs in more than one way without mechanical action, but of these I do not now speak. Only of late years, however, has it been known that pressure can convert a dolerite into a hornblende-schist. Of this, through the kindness of Mr. Teall, who first proved the occurrence of this alteration in Great Britain, I can show you an example. The rock, as you see, has lost the structures of a dolerite, and has assumed those characteristic of many hornblende-schists. I say of many, because, though the rock is distinctly foliated, it does not exhibit a conspicuous mineral banding. My own observations confirm those of Mr. Teall, though I have never been so fortunate as to obtain, as he did, a complete demonstration of the passage from the one rock to the other.

It seems, then, to be demonstrated that, by mechanical deformation, accompanied or followed by molecular re-arrangement, foliated rocks, such as certain gneisses and certain schists, can be produced from rocks originally crystalline. But obviously there are limits to the amount of change. The old proverb, "You cannot make a silk purse of a sow's ear," holds good in this case also. To get certain results, you must have begun with rocks of a certain character. So that it is often possible, as I believe, to infer not only the nature of the change, but also that of the original rock. Hitherto we have been dealing with rocks which were approximately uniform in character, though composed of diverse materials—that is, with rocks more or less granular in aspect. Suppose, now, the original rock to have already acquired a definite structure—suppose it had assumed, never mind how, a distinct mineral banding, the layers varying in thickness from a small fraction of an inch upwards. Would this structure survive the mechanical deformation? I can give an answer which will at any rate carry us a certain way. I can prove that subsequent pressure has frequently failed to obliterate an earlier banded structure. In such a district as the Alps we commonly find banded gneisses and banded schists, which have been exposed to great pressure. Exactly as in the former case, the new divisional planes are indicated by a coating of films of mica, by which the fissility of the rock in this direction is increased. The mass has assumed a cleavage-foliation. I give it this name because it is due to the same cause as ordinary cleavage, but is accompanied by mineral change along the planes of division; while I term the older structure stratification-foliation, because so frequently, if it has not been determined by a stratification of the original constituents, it is at any rate a most extraordinary imitation of such an arrangement. In many cases the new structure is parallel with the old, but in others, as in the "strain-slip" cleavage of a phyllite, the newer can be seen distinctly cutting across the older mineral banding. As an example, take a rock mainly consisting of quartz and mica. Sometimes there has been a certain amount of crushing of the constituents, followed by a re-crystallization of the quartz and the formation of a pale-coloured mica. Sometimes, when the direction of the disturbance has been at right angles to the stratification-foliation, the latter is made wavy, and the mica-flakes are twisted round at right angles to their original position. Sometimes there has been a dragging or shearing of the mass, so that a considerable amount of mica has been re-crystallized along the new planes of division. To put it briefly, I assert, as the result of examining numbers of specimens, that though in certain cases the new structure is dominant, a practised eye seldom fails to detect traces of the older foliation, while in a large number of instances it is still as definite as the stripe in a slate.

We have got, then, thus far, that pressure acting on rocks previously crystallized can produce a foliation; but when it has acted in Palæozoic or later times, the resulting structures can be identified, and these, as a rule, are distinguishable from those of the most ancient foliated rocks, while at present we have found no proof that pressure alone can produce any conspicuous mineral banding. I am aware that this statement will be disputed, but I venture to plead, as one excuse for my temerity, that probably few persons in Great Britain have seen more of crystalline rocks, both in the field and with the microscope, than myself. So, while I do not deny the possibility of a well-banded rock being due to pressure alone, I unhesitatingly affirm that this at present is a mere hypothesis—an hypothesis, moreover, which is attended by serious difficulties. For, if we concede that, in the case of many rocks originally granular, dynamic metamorphism has produced a mineral banding, this is only on a very small scale: the layers are but a small fraction of an inch thick. No one could for a moment confuse a sheared granite from the Highlands with a Laurentian gneiss from Canada or with an uninjured Hebridean gneiss. For the former to attain to the condition of the latter, the mass must have been brought to a condition which admitted of great freedom of motion amongst the particles, almost as much, in short, as among those of a molten rock. Clearly, the dynamic metamorphism of Palæozoic or later ages appears to require some supplementary agency. Can we obtain any clue to it?

An explanation of broadly-banded structures was long since suggested, and has recently been urged with additional force, which avoids some of our difficulties. We know that the process of consolidation in a coarsely crystalline rock has often been a slow one; the constituent minerals separate gradually from the magma, of which sometimes so little may remain, that a rock with a true glassy base has been mistaken for one holocrystalline. The residual and still unsolidified magma would admit of a slow flowing of the mass, but there would be so little of it that the crystals already individualized, though altered in position by differential movements, would be affected by strains, and liable to fracture. Such a rock, when finally consolidated, would exhibit many phenomena in common with a rock modified by dynamic metamorphism, but would differ in the greater coarseness of its structure. This may prove to be the correct explanation of the curious foliated and banded gabbros in the Lizard district. That some crystalline rocks must have passed through this stage I am now in a position to affirm, from evidence not yet published.

Let us, however, see whether another line of investigation may not throw some light on our difficulty. I have already mentioned the effect produced by the intrusion of large masses of igneous rocks upon other rocks. These may be either igneous rocks already solidified, or sedimentary rocks. The former may be passed over, as they will not materially help us. In regard to the latter, the results of contact-metamorphism, as it is called, are, as we might expect, very various. Speaking only of the more extreme, we find that sandstones are converted into quartzites; limestones become coarsely crystalline, all traces of organisms disappearing, and crystalline silicates being formed. In clayey rocks all signs of the original sediments disappear, crystalline silicates are formed, such as mica (especially brown), garnet, andalusite, and sometimes tourmaline; felspar, however, is very rare. Fair-sized grains of quartz appear, either by enlargement of original granules or by independent crystallization of the residual silica. It is further important to notice that, as we approach the surface of the intrusive mass—that is, as we enter upon the region where the highest temperature has been longest maintained—the secondary minerals attain a larger size and are more free from adventitious substances—that is, they have not been obliged as they formed to incorporate pre-existing constituents. The rock, indeed, has not been melted down, but it has attained a condition where a rather free molecular movement became possible, and a new mineral in crystallizing could, as it were, elbow out of the way the more refractory particles. I can, perhaps, best bring home to you the result of contact-metamorphism by showing you what its effects are on a rock similar to that which I exhibited in illustration of the effect of pressure-metamorphism on a distinctly stratified rock. These are, in brief, to consolidate the rock, and while causing some constituents to vanish, to increase greatly the size of all the others. It follows, then, that mineral segregation is promoted by the maintenance for some time of a high temperature, which is almost a truism. I may add to this that, though rocks modified by contact-metamorphism differ from the Archæan schists, we find in them the best imitations of stratification-foliation, and of other structures characteristic of the latter.

One other group of facts requires notice before we proceed to draw our inferences from the preceding. Very commonly, when a stratified mass rests upon considerably older rocks, the lower part of the former is full of fragments of the latter. Let us restrict ourselves to basement beds of the Cambrian and Ordovician—the first two chapters in the stone-book of life. What can we learn from the material of their pages? They tell us that granitoid rocks, crystalline schists of various kinds, as well as quartzites and phyllites, then abounded in the world. The Torridon sandstone of Scotland proves that much of the subjacent Hebridean had even then acquired its present characteristics. The Cambrian rocks of North and South Wales repeat the story, notably near Llynfaelog in Anglesey, where the adjacent gneissoid rocks from which the pebbles were derived, even if once true granites, had assumed their differences before the end of the Cambrian period. By the same time similar changes had affected the crystalline rocks of the Malverns and parts of Shropshire. It would be easy to quote other instances, but these may suffice. I will only add that the frequent abundance of slightly-altered rocks in these conglomerates and grits appears significant. Such rocks seem to have been more widely distributed—less local—than they have been in later periods. Another curious piece of evidence points the same way. In North America, as is well known, there is an ancient group of rocks to which Sir W. Logan gave the name Huronian, because it was most typically developed in the vicinity of Lake Huron. Gradually great confusion arose as to what this term really designated. But now, thanks to our fellow-workers on the other side of the Atlantic, the fogs, gendered in the laboratory, are being dispelled by the light of microscopic research and the fresh air of the field. We now know that the Huronian group in no case consists of very highly-altered rocks, though some of its members are rather more changed than is usual with the British Cambrians, than which they are supposed to be slightly older. Conglomerates are not rare in the Huronian. Some of these consist of granitoid fragments in a quartzose matrix. We cannot doubt that the rock was once a pebbly sandstone. Still the matrix, when examined with the microscope, differs from any Palæozoic sandstone or quartzite that I have yet seen. Among grains of quartz and felspar are scattered numerous flakes of mica, brown or white. The form of these is so regular that I conclude they have been developed, or at least completed, *in situ*. Moreover, the quartz and the felspar no longer retain the distinctly fragmental character usual in a Palæozoic grit, but appear to have received secondary enlargement. A rock of fragmental origin to some extent has simulated or reverted to a truly crystalline structure. In regard to the larger fragments we can affirm that they were once granitoid rock, but in them also we note incipient changes such as the development of quartz and mica from felspar (without any indication of pressure), and there is reason to think that these changes were anterior to the formation of the pebbles.

To sum up the evidence. In the oldest gneissoid rocks we find structures different from those of granite, but bearing some resemblance to, though on a larger scale than, the structures of vein-granites or the surfaces of larger masses when intrusive in sedimentary deposits. We find that pressure alone does not produce structures like these in crystalline rocks, and that when it gives rise to mineral banding this is only on a comparatively minute scale. We find that pressures acting upon ordinary sediments in Palæozoic or later times do not produce more than colourable imitations of crystalline schists. We find that when they act upon the latter the result differs, and is generally distinguishable from stratification-foliation. We see that elevation of temperature obviously facilitates changes, and promotes coarseness of structure. We see also that the rocks in a crystalline series which appear to occupy the highest position seem to be the least metamorphosed, and present the strongest resemblance to stratified rocks. Lastly, we see that mineral change appears to have taken place more readily in the later Archæan times than it ever did afterwards. It seems, then, a legitimate induction that in Archæan times conditions favourable to mineral change and molecular movement—in short, to metamorphism—were general, which in later ages have become rare and local, so that, as a rule, these gneisses and schists represent the foundation-stones of the earth's crust.

On the other side what evidence can be offered? In the first place, any number of vague or rash assertions. So many of these have already come to an untimely end, and I have spent so much time and money in attending their executions, that I do not mean to trouble about any more till its advocates express themselves willing to let the question stand or fall on that issue. Next, the statement of some of the ablest men among the founders of our science, that foliation is more nearly connected with cleavage than with structures suggestive of stratification. In regard to this I have already admitted, in the case of the more coarsely crystalline rocks, what is practically identical with their claim, for they also assert that when the banding was produced, very free movement of the constituents was possible; and in regard to the rest I must ask whether they were speaking of cleavage-foliation or stratification-foliation, which had not then been distinguished, and I know in some instances what the answer will be. The third objection is of a general nature. To prevent the possibility of misstatement I will give it as a quotation:—"To a geologist (especially one belonging to the school of Lyell) it is equally difficult to conceive that there should be a broad distinction between the metamorphic rocks of Archæan and post-Archæan age respectively, as that the pre-Tertiary volcanic rocks should be altogether different in character from those of Tertiary and recent times." Of course in this statement much depends on the sense attached to the epithet "broad." As an abstract proposition I should admit, as a matter of course, that from similar causes similar consequences would always follow. But in the latter part of the quotation lurks a *petitio principii*. During the periods mentioned volcanic rocks appear, as we should expect, to have been ejected from beneath the earth's crust similar in composition and condition, and to have solidified with identical environment. Hence the results, allowing for secondary changes, should still be similar. But to assume that the environment of a rock in early Archæan times was identical with that of similar material at a much later period is to beg the whole question. My creed, also, is the uniformitarian; but this does not bind me to follow a formula into a position which is untenable. Other studies with which I have some familiarity have warned me that a blind orthodoxy is one of the best guides to heresy. "The weakness and the logical defect of uniformitarianism"—these are Prof. Huxley's words—"is a refusal, or at least a reluctance, to look beyond the 'present order of things.' and the being content for all time to regard the oldest fossiliferous rocks as the *Ultima Thule* of our science." Now, speaking for myself, I see no evidence since the time of these rocks, as at present known, of any very material difference in the condition of things on the earth's surface. The relations of sea and land, the climate of regions, have been altered; but because I decline to revel in extemporized catastrophes, and because I believe that in Nature order has prevailed and law has ruled, am I therefore to stop my inquiries where life is no longer found, and we seem approaching the firstfruits of the creative power? Because palæontology is, perforce, silent; because the geologist can only say, "I know no more," must I close my ear to those who would turn the light of other sciences upon the dark places of our own, and meet their reasoning with the exclamation, "This is not written in the book of uniformity"? To do this would be to imitate the silversmiths of old, and silence the teacher by the cry, "Great is Diana of the Ephesians."

What, then, does the physicist tell us was the initial condition of this globe? I will not go into the vexed question of geological time, though as a geologist I must say that we have reason to complain of Sir W. Thomson. Years ago he reduced our credit at the bank of time to a hundred millions of years. We grumbled, but submitted, and endeavoured to diminish our drafts. Now he has suddenly put up the shutters, and declared a dividend of less than four shillings in the pound. I trust some aggrieved shareholder will prosecute the manager. However, as a *cause célèbre* is too long a business for the end of an evening, I will merely say that, while personally I see little hope of arriving at a chronological scale for the age of this earth, I do not believe in its eternity. What, then, does the physicist tell us must have been in the beginning? I pass by those earliest ages, when, as "Ilion, like a mist, rose into towers," so from the glowing cloud the great globe was formed. I pass on to a condition more readily apprehended by our faculties—the time, the *consistentior status* of Leibnitz, when the molten globe had crusted over, and its present history began. Rigid uniformitarian though you may be, you cannot deny that when the very surface of the ground was at a temperature of at least 1000° F., there was no rain, save of glowing ashes—no river, save of molten fire. Now is ending a long history with which the uniformitarian must not reckon—of a time when many compounds now existing were not dissolved but dissociated, for combination under that environment was impossible. Yet there was still law and still order—nay, the present law and order may be said even then to have had a potential existence—nevertheless to the uniformitarian gnome, had such there been, every new combination of elements would have been a new shock to his faith, a new miracle in the earth's history. But at the times mentioned above, though oxygen and hydrogen could combine, water could not yet rest upon the ruddy crust of the globe. What does that mean? This, that assuming the water of the ocean equivalent to a spherical shell of the earth's radius and two miles thick, the very lava-stream would consolidate under a pressure of about 310 atmospheres, equivalent to nearly 4000 feet of average rock.[1] But on the practical bearing of this consideration I will not dwell. Let us pass on to a time which, according to Sir W. Thomson, would rather quickly arrive, when the surface of the crust had cooled by radiation to its present temperature. Let us, merely for illustration, take a surface temperature of 50° F. (nearly that of London), and assume that the present rise of crust temperature is 1° F. for every 50 feet of descent, which is rather too rapid. If so, 212° F. is reached at 8100 feet, and 250° F. at 10,000 feet. Though the latter temperature is far from high, yet we should expect that under such a pressure chemical changes would occur with much more facility than at the surface. But many Palæozoic or even later rock masses can now be examined which at a former period of their history have been buried beneath at least 10,000 feet of sediment; yet the alteration of their constituents has been small: only the more unstable minerals have been somewhat modified, the more refractory are unaffected. But for a limited period after the *consistentior status*, the increase of crust temperature in descending would be far more rapid; when one-twenty-fifth of the whole period from that epoch to the present had elapsed, and this is no inconsiderable fraction, the rate of increase would be 1° for every 10 feet of descent. Suppose, for the sake of comparison, the surface temperature as before, the boiling-point of water would be reached at 1620 feet, and at 10,000 feet, instead of a temperature of 250° F., we should have one of 1050° F. But at the latter temperature many rock masses would not be perfectly solid.[2] According to Sorby, the steam cavities in the Ponza trachyte must have formed, and thus the rock have been still plastic at so low a temperature as 680° F. At this period, then, the end of the fourth year of the geological century, whatever be its units, structural changes in igneous and chemical changes in sedimentary rocks must have occurred more readily than in any much later period in the world's history. A temperature of 2000° F., sufficient to melt silver—more than sufficient to melt many lavas—would have been reached at a depth of about 4 miles. It would now be necessary to descend for at least 20 miles in order to arrive at this zone. It, during the ninety-six years of the century, has been changing its position in the earth's crust, more slowly as time went on, from the one level to the other.

There is another consideration, too complicated for full discussion, too uncertain, perhaps, in its numerical results to be more than mentioned at present, which, however, seems to me important. It is this, that in very early times, as shown by Prof. Darwin and Mr. Davison, the zone in the earth's crust, at which lateral thrust ceases and tension begins, must have been situated much nearer to the surface than at present. If now, at the end of the century, it is at the depth of 5 miles, it was, at the end of the fourth year, at a depth of only 1 mile. Then, a mass of rock, 10,000 feet below the surface, would be nearly a mile deep in the zone of tension. Possibly this may explain the mineral banding of much of our older granitoid rock, already mentioned, and the coincidence of foliation with what appears to be stratification in the later Archæan schists, as well as the certainly common coincidence of microfoliation with bedding in the oldest indubitable sediments.

Pressure, no doubt, has always been a most important factor in the metamorphism of rocks; but there is, I think, at present some danger in over-estimating this, and representing a partial statement of truth as the whole truth. Geology, like many human beings, suffered from convulsions in its infancy; now, in its later years, I apprehend an attack of pressure on the brain.

The first deposits on the solidified crust of the earth would obviously be igneous. As water condensed, denudation would begin, and stratified deposits, mechanical and chemical, become possible, in addition to detrital volcanic material. But at that time the crust itself, and even stratified deposits, would often be kept for a considerable period at a temperature similar to that afterwards produced by the invasion of an intrusive mass. Thus not only rocks of igneous origin (including volcanic ashes) would predominate in the lowest foundation-stones, but also secondary changes would occur more readily, and even the sediments or precipitates should be greatly metamorphosed. Strains set up by a falling temperature would produce, in masses still plastic, banded structures, which, under the peculiar circumstances,

[1] If we take the specific gravity of water as unity, and that of mean rock as 2·7, the pressure would be = 3911·1 feet of rock.

[2] The lowest temperature, which, so far as I know, has been observed in lava (basic) while still plastic, is 1228° F.

might occur in rocks now coarsely crystalline. As time went on, true sediments would predominate over extravasated materials, and these would be less and less affected by chemical changes, and would more and more retain their original character. Thus we should expect that as we retraced the earth's course through "the corridor of time," we should arrive at rocks which, though crystalline in structure, were evidently in great part sedimentary in origin, and should beyond them find rocks of more coarsely-crystalline texture and more dubious character, which, however, probably were in part of a like origin; and should at last reach coarsely-crystalline rocks, in which, while occasional sediments would be possible, the majority were originally igneous, though modified at a very early period of their history. This corresponds with what we find in Nature, when we apply, cautiously and tentatively, the principles of interpretation which guide us in stratigraphical geology.

I have stated as briefly as possible what I believe to be facts. I have endeavoured to treat these in accordance with the principles of inductive reasoning. I have deliberately abstained from invoking the aid of "deluges of water, floods of fire, boiling oceans, caustic rains, or acid-laden atmospheres," not because I hold it impossible that these can have occurred, but because I think this epoch in the earth's history so remote and so unlike those which followed, that it is wiser to pass it by for the present. But unless we deny that any rocks formed anterior to or coæval with the first beginning of life on the globe can be preserved to the present time, or, at least, be capable of identification (an assumption which seems to me gratuitous and unphilosophical) then I do not see how we can avoid the conclusion to which we are led by a study of the foundation-stones of the earth's crust—namely, that these were formed under conditions and modified by environments which, during later geological epochs, must have been of very exceptional occurrence. If, then, this conclusion accords with the results at which students of chemistry and students of physics have independently arrived, I do not think that we are justified in refusing to accept them, because they lack the attractive brilliancy of this or that hypothesis, or do not accord with the words in which a principle, sound in its essence, has been formulated. It is true in science, as in a yet more sacred thing, that "the letter killeth, the spirit giveth life."

SYSTEMATIC RELATIONS OF PLATYPSYLLUS AS DETERMINED BY THE LARVA.

PROF. C. V. RILEY, in a paper read at a recent meeting of the National Academy of Science (U.S.A.), drew attention to the unique character of *Platypsyllus castoris*, a parasite of the beaver; and gave an epitome of the literature on the subject, showing how the insect had puzzled systematists, and had been placed by high authority among the Coleoptera and the Mallophaga, and made the type even of a new order. He showed the value, as at once settling the question of its true position, of a knowledge of the adolescent states. He had had since November 1886 some 14 specimens of the larva, obtained from a beaver near West Point, Nebraska, and had recently been led to study his material at the instance of Dr. Geo. H. Horn, of Philadelphia, who at a recent meeting of the Entomological Society of Washington announced the discovery of the larva by one of his correspondents the present spring, and will publish a full description of it. Prof. Riley indicated the undoubted Coleopterological characteristics of the insect in the imago state, laying stress on the large scutellum and five-jointed tarsi, which at once remove it from the Mallophaga, none of which possess these characters. He also showed that the larva fully corroborates its Coleopterological position, and that its general structure, and particularly the trophi, anal cerci, and pseudopod, confirm its Clavicorn affinities. He showed that the atrophied mandibles in the imago really existed as described by Le Conte, and that even in the larva they were feeble and of doubtful service in mastication. He mentioned, as confirmatory of these conclusions, the finding by one of his agents, Mr. A. Koebele, of Leptinillus (the Coleopterological nature of which no one has doubted, and the nearest ally to Platypsyllus), associated with Platypsyllus upon beaver-skins from Alaska; also the parasitism of Leptinus upon mice. He paid a high compliment to the judgment and deep knowledge of the late Dr. Le Conte, whose work on the imago deserves the highest praise, and whose conclusions were thus vindicated. "Platypsyllus therefore," he concluded, "is a good Coleopteron, and in all the characters in which it so strongly approaches the Mallophaga it offers merely an illustration of modification due to food, habit, and environment. In this particular it is, however, of very great interest as one of the most striking illustrations we have of variation in similar lines through the influence of purely external or dynamical conditions, and where genetic connection and heredity play no part whatever. It is at the same time interesting because of its synthetic characteristics, being evidently an ancient type, from which we get a very good idea of the connection in the past of some of the present well-defined orders of insects."

SCIENTIFIC SERIALS.

Atti della R. Accademia dei Lincei, July and August 1888.—In both of these numbers G. Vicentini and D. Omodei continue their important inquiries on the thermic expansion of certain binary alloys in the liquid state. So far they have arrived at the following general conclusions: (1) the variation of volume accompanying liquid metallic mixtures is extremely slight; (2) no relation can be established between the variations of volume that accompany the formation of alloys in the solid and liquid states; (3) the variation of density at the moment of solidification is in general less than would be the case were the constituent metals to preserve in the alloys the value that they possess in the isolated state; (4) the binary alloys of lead and tin, of tin and bismuth, and of tin and cadmium, possess in the state of perfect fusion an expansion equal to that resulting from the sum of the expansions of the associated metals; (5) the alloy of Bi_2Pb possesses a coefficient of expansion far greater than the sum of the expansions of the constituent metals. These experiments, which conclude for the present with a preliminary study of the antimony and zinc alloys, have been carried out at the physical laboratory of the University of Cagliari, Sardinia.

Rivista Scientifico-Industriale, October.—Experiments made with Crookes's radiometer, by Prof. Pietro Lancetta. The experiments here described have been undertaken chiefly for the purpose of making a synthesis of certain phenomena which are more easily produced by this apparatus than by any other means. It is also shown that the radiometer may in some cases be more advantageously employed than the ordinary thermometer, especially in testing certain laws regarding latent and luminous heat, Crookes's instrument being sensible both to the dark and luminous wave of the solar rays. The results of the experiments show generally that in a homogeneous medium the radiation of the thermo-luminous wave is propagated in a straight line; that the luminous wave is propagated *in vacuo*; that the intensity of the thermo-luminous wave is in inverse ratio to the square of the distance; that the evaporation of fluids as well as the rarefaction of gaseous bodies is accompanied by a lowering of the temperature, while the condensation of gas develops heat.

Journal of the Russian Chemical and Physical Society, vol. xx. fasc. 6.—On the speed and the products of decomposition of the chlorate and chlorite of lithium, by A. Potilitzin, being the second part of an inquiry into the properties of galloid compounds. The decomposition of the two above-mentioned salts, as well as of the bromate of barium, is best explained according to the law of unstable equilibrium indicated by the author in his former works, and which he sums up as follows: in each chemical reaction the equilibrium of the system depends upon the values of their atomic weights, their masses, and their stock of potential energy.—On the relation between the rotatory power and the refraction of organic compounds, by J. Kanonnikoff, first part.—On the action of organic iodides on natrium-nitro-ethane, by N. Sokoloff.—Obituary notice of Prof. Wroblewski, by S. Lamansky.—The total eclipse of the sun of August 19, 1887, by N. Egoroff; and on the results of meteorological observations during the same eclipse, by N. Hesehus.

SOCIETIES AND ACADEMIES.

LONDON.

Royal Society, May 31.—"On the Effect of Occluded Gases on the Thermo-electric Properties of Bodies, and on their Resistances; also on the Thermo-electric and other Properties of Graphite and Carbon." By James Monckman, D.Sc. Communicated by Prof. J. J. Thomson, F.R.S.