Coccocrinus in the calyx-structure, further differs from *Hapalocrinus* in its heavy, biserial arms. *Cordylocrinus* is very closely allied to *Hapalocrinus*, but differs in the compound nature of the IBr, each of which bears two pinnules. *Marsupiocrinus* has but one IBr, biserial arms, and a large five-lobed axial canal in the stem. *Platycrinus* and *Eucladocrinus* have but one IBr, biserial arms, and stem of elliptic section.

In its arm-structure, then, *Hapalocrinus* is more primitive than any of the Platycrinidæ, except perhaps *Coccocrinus*, of which the arms are unknown; in its tegmental development it seems to be about on the same level as *Coccocrinus*, though perhaps a little further removed from the Larviform type. This stage of evolution consists with its geological range, which is from Middle Silurian to Lower Devonian.

REFERENCE OF THE MELBOURNE CRINOID TO HAPALOCRINUS.

In its arm-structure, and, obscure though it be, in the composition of the tegmen, the Melbourne crinoid appears more closely allied to *Hapalocrinus* than to any other genus with which I am acquainted. It has, however, three IBr, whereas no other Platycrinid has more than two. Therefore, if it be placed with *Hapalocrinus*, we must regard it as in this respect more primitive than the other species. It is also at least as primitive as any other *Hapalocrinus* in the structure of the IBr and pinnules. It would be unwise to found a new genus for the reception of this unique and rather obscure specimen, so I refer it provisionally to *Hapalocrinus*.

As there can be no question that it represents a distinct species, it requires a trivial name. I received the specimen and studied it during the week of Her Majesty's Diamond Jubilee; this fact, and the fact that it is the first Silurian crinoid crown found in the colony of Victoria, inevitably suggest the name—

Hapalocrinus Victoriae.

DIAGNOSIS.—IBr 3; arms fork once; IBr half as long again as wide, in zigzag, bearing alternately disposed delicate pinnules.

The type-specimen will be placed in the museum at Melbourne University by Mr. T. S. Hall, to whom, in conclusion, I express hearty thanks for giving me the opportunity of studying this highly interesting fossil.

II.—NOTES ON THE AGE AND STRUCTURE OF THE GNEISSOSE-GRANITE OF THE HIMALAYAS WITH REFERENCE TO MR. MIDDLEMISS'S MEMOIR ON THE GEOLOGY OF HAZARA.

By LIEUT.-GENERAL C. A. McMAHON, V.P.G.S.

PART II.

Origin of the Foliation.

IN the first part of this paper I confined myself to the consideration of the age of the Himalayan granite; in this I propose to discuss Mr. Middlemiss's views regarding the cause of its foliation.

Mr. Middlemiss appears to be so satisfied in his own mind that the foliation has been produced subsequent to the consolidation

of the rock by pressure-metamorphism, that he considers it unnecessary to discuss the facts and arguments that I have from time to time brought forward to show that pressure *before* consolidation was the efficient cause of the foliation of the Himalayan rock. I shall, as far as possible, refrain from repeating in this paper what I have previously written on this subject, and shall content myself with referring the reader to my address, as President of the Geologists Association (Proc. Geol. Assoc., xiv, p. 287), for a condensed presentation of my case. On the present occasion I shall confine my remarks to a statement of supplemental facts and arguments.

Mr. Middlemiss's hypothesis, given in his own words, is as follows:—"Just as the mineral composition of the great crystalline axis of the Himalaya has been found to be the same right through from end to end of the chain, so is it also found to be the case that the dynamic metamorphism of those rocks is also a constant factor throughout the range.

"I do not think there can be two opinions as to the origin of this state of things. It can only imply a steadily acting lateral pressure of the earth's crust tending to bank it up against the central crystalline core by a movement and a resistance in two opposite directions. To the south we have the newer zones of disturbance showing no dynamic metamorphism; but to the north the accumulated pressures consequent on the upheaval of several zones one after the other, with the gradually diminishing in size of the larger earth-folds as the centre of the solid crystalline core is approached, have all tended to bring about that intense cleaving, shearing, and deformation of the minuter particles of the rock which I embrace generally under the head of dynamic metamorphism" (p. 279).

The author tells us (p. 61) that the gneissose-granite "is present in the form of intrusive bands among the schistose rocks"; that these "bands are generally parallel to the foliation of the rocks among which they lie"; and that the foliation of the granite is "perfectly parallel to that of the schists" (p. 66). Some of the granite is foliated, but some is not (p. 64). The non-foliated variety represents the "original condition in which the rock first solidified after intrusion among the sedimentary rocks of the region" (p. 64); and the foliated variety has been derived from it "by the mechanical action of pressure metamorphism" (p. 64); "acting on the rock since the time it became a solid stationary mass" (p. 65).

The evidence on which the author appears to rely for proof of his position may be summarized as follows:—

- (1) The crushed condition of the Himalayan rocks generally (p. 279).
- (2) The parallel foliation and fissility of the rocks (p. 279).
- (3) The fact that the foliation of the gneissose-granite is usually parallel to that of the schists, although there are exceptions to this rule (p. 66).
- (4) That the gneissose-granite exhibits augen structure (p. 65).
- (5) That the minerals have been rolled and dragged out (p. 65), and exhibit "flaser" structure (p. 72).

(6) That the pebbles in the Infra-Trias conglomerate have been flattened and deformed (p. 55).

(7) That coarse grains of quartz in the Infra-Trias sandstone have been powdered and drawn out to eyes (p. 56).

I may say at once that I fully admit, and for the matter of that always have admitted, the truth of the facts alleged in the above seven points. What I have always denied, and still deny, is, that the foliation of the granite was produced by crushing and shearing *after* it became a solid mass. I have all along contended that a foliated structure was impressed on the granite *before* its final consolidation.

No one, I presume, doubts that the birth of the Himalayas was accompanied by great earth movements, and that during the elevation of these lofty mountains great compression, contortion, faulting, and shearing of the strata took place on a grand scale. I certainly have never denied it; and I have expressly admitted that the gneissose-granite itself bears evidence of having suffered many things from the dynamic forces of Nature. For instance, I stated in my Presidential Address already referred to, that "the granite was forced in thick sheets between beds of strata, and into the faults and fissures opened up in the course of the compression, uplifting, and contortion of the rocks which set in within the Himalayan area at the close of the Eocene period" (loc. cit., p. 288). I pointed out that in one region of special strain "the gneissose-granite passes at its edges into a rock that might, without microscopical examination, be taken for a mica-schist" (loc. cit., p. 288). In another paper (*GEOLOGICAL MAGAZINE*, 1887, p. 214) I said that the outer band of gneissose-granite at Dalhousie "not unfrequently looks, viewed macroscopically, as if it had been flattened under a steam-roller"; and on the same page I detailed the evidence of strain, fracture, and traction, supplied by the microscope.

The point at issue, therefore, is not whether the Himalayan rocks generally, or the granite in particular, give evidence of having suffered compression, contortion, or fracture, but whether the foliation of the granite was produced prior to, or posterior to, the consolidation of the rock. The production of evidence to prove points not at issue is illogical. When no one denies that the rocks of the Himalayas have suffered contortion and crushing, what is gained by proving what is already admitted? Moreover, as the question raised concerns an igneous intrusive rock, evidence to show the crushing of sedimentary rocks is really irrelevant, unless it can also be shown that the crushing relied on took place *after* the entrance of the intruder.

The first and main point at issue in the case of the granite of the Himalayas, as before said, is whether the foliation was superinduced *before* or *after* the complete cooling and consolidation of the rock. Evidence to be of logical value must be directed to that issue.

From first to last I have contended that the foliation of the Himalayan granite was produced before the consolidation of the rock, and in my Presidential Address before referred to I tried to explain briefly that lateral pressure applied to an intruded igneous

rock in which crystallization had commenced prior to its being moved into place, would, if the pressure were kept up during the subsequent stages of its cooling, whilst it was "gradually progressing towards rigidity," inevitably result in the production of foliation, and would account for the "undulose extinction, fractured minerals, and crumpled micas, and such like evidence of strain and mechanical action," to be found in the Himalayan gneissose-granite (Pres. Address, loc. cit., pp. 288-9). Given tangential pressure and strain exerted on an intruded granite during its gradual cooling, these results must follow.

The above explanation, at all events, offers a definite theory to explain the foliation and other phenomena to be observed in the Himalayan granite, and it is no answer to this theory to point to evidence of strain and mechanical action in the rock; for the existence of strain and mechanical action during the critical period in the history of the granite, is an essential part of the theory itself.

I have looked in vain through Mr. Middlemiss's Memoir for evidence, other than the above, to rebut my theory. My second Presidential Address, dealing with the structural characters of the granite, probably did not reach the author until after his Memoir was set up in type, but my address was, in the main, a condensed statement of a theory long held by me, and to be found in my previous papers. The one published in this Magazine in May, 1887, alluded to above, for instance, contains an abstract statement of the theory in question.

It may be as well to remark here that so far as the *foliation* of the Himalayan granite is concerned, the foliation set up was a change of structure only, and did not involve any metasomatic change in the chemical composition of the minerals. The foliation was, in its main features, a parallelism in the orientation of the minerals which already existed in the still plastic magma, and was superinduced by pressure applied before the final consolidation of the rock. The mineral principally concerned in producing the foliation was mica, an original mineral abundantly present in the granite before foliation set in.

Given an abundance of mica in a still plastic granite, I do not see how great pressure could be applied without causing the platy leaves of mica to arrange themselves with their flat basal faces at right angles to the direction of the pressure. We have here, then, a *vera causa* to account for the foliation of an igneous rock. Given a sufficient amount of mica, or other mineral of similar habit, and sufficient pressure applied between the formation of the mica and the consolidation of the rock, and we have a sufficient explanation of the foliation it may exhibit.

It may be as well to point out that the tendency of platy minerals to arrange themselves under pressure with their flat faces at right angles to the direction of the pressure, was probably aided by other contributory causes. Even after the granite was moved into place, and the crystallization of some of its minerals, begun in plutonic

regions and continued after its intrusion into the sedimentary rocks, had progressed considerably, the still uncrystallized residuum, or mother liquor, must have been, to some limited extent, capable of flow or motion. Tangential pressure caused by earth movements, continuously acting on the granite, would probably force some of it into new fissures and newly-created planes of weakness, along the margins of the intrusive mass; and this marginal extension of the intruded granite, combined with a continuance of the tangential pressure, would create a slight flow in the unconsolidated mother liquor, sufficient to allow the mica and other platy minerals to yield to the pressure and orient themselves with their flat faces at right angles to it.

Further, it seems probable that the pressure exercised on the granite by the molten mass below it fluctuated from time to time. Deep-seated movements, such as laccolitic intrusions in neighbouring areas, or the extrusion of acid lavas at the surface, would, for a time, relieve this pressure and cause a small movement in or back flow of the mother liquor. In other words, fluctuations in the resistance offered by the molten mass under the granite would allow the partially consolidated granite to be compressed by the tangential pressure continuously exercised upon it.

A very small amount of interstitial movement would suffice to cause the flat leaves of mica to be arranged at right angles to the pressure, while a back flow to any extent, by depriving the crystallized minerals of the elastic support of the mother liquor, would lead to their crushing and deformation.

Those who have watched the operations conducted in a sugar refinery will readily understand that the mother liquor may be drained out of a mass of crystals without the crystals being carried away with it. I do not wish to suggest that the mother liquor was ever pumped out of the granite to any great extent: I only desire to make it clear that small interstitial movements might result from the causes above explained; and that even small movements would suffice to give the tangential pressure that was compressing the partially consolidated granite a free hand, so to speak, and permit it to impress a foliated structure upon the granite.

Interstitial motion created in the two ways suggested above—namely, by the marginal extension of the granite, and by a temporary diminution of the resistance offered by the molten mass below it—might be very local in its action, and hence some parts of a granite might become foliated while other parts remained unfoliated.

One more point remains to be mentioned. The earth movements that produce faults often persist for some time, and though a rupture in the folded strata gives temporary relief to the strain, the stress is sooner or later repeated, with the result that either the throw of the fault gradually increases, or the walls of the fault are moved up and down at intervals. Movements of the walls of a fault into which an igneous rock had been intruded would, as the solidification of the intruder approached completion, inevitably produce marginal foliation and pronounced crush-structures.

In the above remarks my chief object has been to bring before the reader, as clearly as I could, what I believe to be the main factor in the production of the foliation observable in the Himalayan granite. Other causes contributed to produce the effects observed. Traction when the rock was being moved into place,¹ and the corrosive action of the acid residuum on the phenocrysts, might be named; but it is needless to dilate on them here, as I have already written much in other papers on these themes. I am glad to see that Mr. Middlemiss recognizes the validity of the last-mentioned factor, for he remarks (p. 63) with reference to the porphyritic crystals: "The larger ones have often only a very vague outline in the rock, as if their boundaries had become indistinct from the reaction of the still molten magma upon them whilst the rock was in a half-consolidated state."

It is a great relief to be able to turn at this point in the discussion to an interesting and important piece of work in the Scotch Highlands by the officers of H.M.'s Geological Survey. I allude to the explanation of Sheet 5, *Memoirs of the Geological Survey, Scotland*, by Messrs. Horne, Peach, and Teall, 1896. In describing the granite of the south-east portion of the county of Kirkcudbright, Mr. Horne tells us at p. 23 that "the granite occurs mainly in the massive form throughout this extensive area, but there are certain limited tracts where it has a marked foliated character. The latter variety is a marginal phenomenon. It does not, however, extend continuously round the border of the igneous mass, being restricted to well-defined areas. . . . As the observer passes inwards towards the centre, he finds the foliated variety of the border tract gradually to merge into the massive granite."

The granite contains basic inclusions and acid veins, which are also foliated, the foliation of the granite, basal inclusions, and acid veins having a common direction. "The evidence obtained in the field," Mr. Horne writes (p. 24), "clearly points to the conclusion that the foliation must have been superinduced on the massive granite with the basic inclusions, after the formation of the acid veins."

I infer, from what follows, that the acid veins must have been formed after the granite was moved into place, but before it cooled down. The foliation of the basic inclusions was evidently due to the cause that produced the foliation of the granite itself; and as the latter is attributable, as will be seen later, to pressure before cooling, I infer that the basic inclusions were made plastic by the contact action of the granite before the pressure was applied.

"After the development of the foliation," Mr. Horne continues (p. 25), "the foliated granite was injected by a well-marked series of dykes. . . . These dykes run in a north-westerly direction, and have fine-grained or chilled edges. No single instance has been observed among them of any trace of foliation."

¹ Sir A. Geikie mentions interesting cases of "extreme mechanical deformation by the movement of a still molten rock" in his paper on the Tertiary Basalt Plateaux of N.W. Europe (*Q.J.G.S.*, vol. lii, p. 333).

In order to realize the full significance of the presence of unfoliated dykes in the foliated granite, it is necessary to bear in mind that the dykes appear to have been derived from the same source of supply as the granite itself. Mr. Teall tells us at p. 45 :—" It follows from the above description, that there is very little difference in composition between the dykes of this type and the granitic rocks of the Criffel and Dalbeattie mass. . . . It is probable, therefore, that the structural differences between the porphyrites and the hornblende granites or quartz diorites are due to differences in the conditions under which the magmas consolidated. If we suppose that an intrusion of the granitic magma took place in dyke form after the greater part of the hornblende, biotite, and plagioclase had separated out, we shall have a satisfactory explanation of the difference in structure between the porphyrites and the granitic rock. In applying this theory, however, it must be remembered that dykes of the type now under consideration occur not only in the sedimentary rocks, but also in the granitic masses. Moreover, dykes are found in the foliated area unaffected by the foliation. Such dykes cannot be regarded as in any way representing apophyses of the granite. They belong to a later phase of igneous activity. The source of supply was probably the same as that from which the main igneous mass was derived; but if so, it must have been tapped at a time when crystal building had proceeded to a considerable extent."

It would appear, therefore, that although the foliation of the granite was affected after the formation of the acid veins, it was completed before the porphyrite dykes were intruded into the granite; and as the source of supply of the dykes and of the granite was probably the same, no great lapse, measured by geological time, is likely to have intervened between the foliation of the granite and the intrusion of the unfoliated porphyrite.

Mr. Teall, at p. 43, remarks :—"The interstitial movement which has resulted in the production of foliation, and which is proved by the field evidence to have been produced by earth-stresses operating after the rock had reached its present position, and after the formation of the acid veins, has not affected all the constituents alike. The quartz and alkali-felspar which, as we have already seen, were the last constituents to solidify, are those which have yielded most to the deforming stresses. They show signs of crushing, and frequently pass into thin micro-crystalline or even crypto-crystalline streaks, which wind round the comparatively uncrushed crystals of oligoclase. The distribution of the ferro-magnesian constituents has been modified by the movement. Instead of being irregularly distributed through the rock they tend to form flaser in association with the micro-crystalline aggregate of quartz and alkali-felspar. The oligoclase has been little, if at all, affected by the movement. These facts clearly prove that the minerals which yield most readily to the deforming forces are those which consolidate last. *It is probable that the pressure acted before the rock-mass had actually cooled.*" The italics are mine.

The above description of the petrographic characters of the Scotch rock might be applied word for word to the Himalayan granite. It is gratifying to be able to point to the independent evidence of an expert who shows that it is "highly probable" that the pressure which produced the foliation of a Scotch granite "acted before the rock-mass had actually cooled." It further appears from Mr. Teall's remarks that the granite was in a partially consolidated state when the pressure was applied. "The last constituents to solidify," he writes, "are those which have yielded most to the deforming stresses"; and the pressure applied to the uncooled and partially solidified rock crushed the quartz and felspar crystals to such an extent that they passed "into micro-crystalline and crypto-crystalline streaks." We get "flaser" structure, and, in the words of Mr. Horne (p. 25), "The quartz and alkali-felspar are the constituents which yielded most readily to dynamic agencies. The yielding of these constituents dragged out the biotite and hornblende." The correspondence of the Scotch and Himalayan rocks in the origin of their foliation, and in their crush and flaser structures, would seem to be complete; and the testimony of Messrs. Horne and Teall to the effect that these structures, in the case of the Scotch granite described in the above Memoir, were probably produced whilst it was still uncooled and incompletely solidified, is all the more important as these observers have been by no means unfavourable to the cause of dynamo-metamorphism.

If, then, we can get "flaser" structure, and the crushing and dragging out of minerals into streaks by pressure applied to an igneous rock prior to its complete cooling and solidification, it seems to me simply nonsense to appeal to flaser structure, crushing, and the dragging out of minerals, as conclusive evidence that the rock exhibiting these structures has been deformed *after* its consolidation. Those who wish to establish that conclusion for any particular igneous rock must produce good and convincing evidence to prove that the dynamic movements took effect *after*, and not *before*, its consolidation.

The condition of mind of those who think that it is only necessary to produce evidence of crushing to close controversy on the point at issue, reminds me of those who, in days gone by, exclaimed with enthusiasm as they stood gazing at a gabbro: "We know for certain that this is a metamorphosed sedimentary rock, *because* it is foliated!" Lest any should think this statement exaggerated I will give extracts from a recently published memoir (60th Report, U.S. Geol. Surv. 1894-5), "On the Principles of North American Pre-Cambrian Geology," by Mr. C. R. Van Hise. "A few years ago," he writes, "it was assumed as a matter of course that distinct lamination in the gneissose granites, gneisses, and schists, and the fact that these rocks were believed to grade into the clastics, were taken as conclusive evidence of the original sedimentary origin of the whole" (p. 748); and the author refers by name to five authors who, among others, actually acted on the above-mentioned principle. Van Hise also points out (p. 768)

cases in which "laminated gabbro" was, in deference to this principle, written down as a sedimentary rock!¹ I trust we have now nearly heard the last of arguments which may fairly be paraphrased into such a remark as—"This is a striking case of dynamo-metamorphism. It is quite evident that the rock was deformed by tremendous earth movements *after* its solidification, *because* it shows flaser structure and crushing, and some of its minerals have been drawn out into strings."

Turning again to Mr. Middlemiss's Memoir, there are some facts in it which, to my mind, raise difficulties in the way of the acceptance of the theory advocated by the author, and suggest the existence of evidence against it. Mr. Middlemiss tells us at page 75 of basic dykes which generally "form narrow bands or dykes roughly parallel to the foliation of the intruded rock"; but "in certain places the dykes cut across both the schistose series and the gneissose-granite intrusions, and also send off small veins into them." With the exception of the Indus Valley dykes, nothing is said about the foliation of the basic dykes that cut across the foliation of the schists. Are they foliated or unfoliated, and if the former, is their foliation parallel or at an angle to that of the schists? In fig. 4 (p. 76) one of the Indus Valley dykes is represented, and parallel fine white lines are to be observed along one edge of the dyke which appear intended to represent foliation. If these lines are intentional, and not an accident in printing, it is to be observed that they are at a high angle to the foliation of the schists and only occur along one margin of the dyke.

The author tells us (p. 75) that the dykes which cut through the gneissose-granite of the Indus are foliated along their edges. No information is given as to the direction of this marginal foliation in relation to the foliation of the granite; but it seems clear that the conditions under which the foliation was impressed upon the two rocks must have been different. If the foliation were imparted to the granite after its solidification, and after the intrusion of the dolerite, it is difficult to understand why the dolerite was not foliated from side to side.

In the detailed account of the Indus Valley section given in the Memoir (p. 253) a mass of dolerite is shown in fig. 51, as an intrusion in the schists parallel to their bedding. The section suggests the idea that the dolerite was folded up with the strata, and that the latter form a somewhat crushed anticlinal. The two dolerite bands on the right of the illustration might even represent the dying out of the dolerite seen on the left of the woodcut. The schists and the granite seen in this section appear from the author's general remarks (see ante, p. 346) to be foliated in a direction parallel to the folds of the bedding; but the dolerite, which was apparently folded up with the schists and the granite, and must have participated in the stress, crush, and shearing, which accompanied the folding of the rocks, is absolutely without foliation. If

¹ "This gabbro," he writes, "was formerly regarded as a later sedimentary series!" (Loc. cit., p. 768.)

the tangential pressure which crumpled up the strata produced foliation in the schists and granite, how did the dolerite escape uninjured? This is the more remarkable as dolerite takes on foliation very readily. The author has not given any explanation of the difficulty.

Mr. Middlemiss appears to rely very much on shearing and molecular movement to account for the results which he attributes to dynamo-metamorphism. "If we believe," the author writes, "(as we have a right to do from considerations as to the deep-seated nature of granite) that very great thicknesses of the slate or schistose rocks once overspread that crystalline core, we can understand that lateral stresses, if sufficiently powerful and long-continued, acting on such a material would be unable to violently fold and plicate them on a large scale, but would spend themselves in minute deformation of the rocks in shearing and in other ways characteristic of dynamic metamorphism" (p. 280).

If shearing and molecular movement took place in Mr. Middlemiss's crystalline and metamorphic zone to the extent supposed by the author, great heat must have been generated, and strong chemical action must have resulted. It is curious, therefore, to notice that these much "mashed" rocks, to use a term employed by geologists over the Atlantic, do not appear to have yielded the author much in the way of dynamo-metamorphic minerals. One reads of mica and of "incipient garnets," "which occur as minute specks or raised blisters" (p. 55), but I cannot call to mind anything else. This poverty of chemical and mineralogical result after the expenditure of so much dynamic energy recalls an ancient proverb to the effect that "a great mountain was in labour and brought forth a small mouse."¹

I see no reason to believe that the great masses of gneissose-granite now to be seen in the Himalayas were "mashed" by pressure after cooling and solidification. In following outcrops along the line of strike, as for instance along the Shankan outcrop in the Simla regions (Records G.S.I., x, p. 217), the granite passes repeatedly from massive to foliated, and from foliated to massive, in an apparently capricious manner. This feature I think quite incompatible with the supposition that the foliation is the result of shearing subsequent to the solidification of the rock, either in the direction of the strike or in the direction of the dip.

I would again invite attention to the heliogravure of an inclusion in the Dalhousie gneissose-granite published at p. 174, in vol. xvii, Records G.S.I. The plate represents a cross-section of a splinter of schist two feet long, shaped like a tent-peg. The outlines of the inclusion are perfectly sharp, and it is evident that the granite in the locality where the inclusion was found cannot have been sheared after the inclusion of the fragment. Had shearing taken place the inclusion would have been flattened and drawn out into ribbons.

To sum up, I think that though the facts brought before us in Mr. Middlemiss's Memoir are interesting, they shed no new light

¹ "*Parturiunt montes, nascetur ridiculus mus.*"—HORACE.

on the origin of the foliation of the granite of the Himalayas, and do not in the least traverse the theory I have offered to explain that phenomenon. The evidence supplied in the Memoir under review is on points which are not only not in dispute, but actually form an essential part of my theory. Nothing more need be said by me unless a serious attempt be made to meet or disprove that theory.

III. — THE GEOLOGICALLY RECENT ORIGIN OF THE SURFACE CONTOUR OF SCANDINAVIA AND FINLAND, AND ITS LESSONS.

By Sir HENRY H. HOWORTH, K.C.I.E., M.P., F.R.S., F.G.S.

SOME time ago I discussed in this Magazine the recent geology of Greenland; and I ventured to argue, and, I think, to prove, that the general rise of that great island from the sea, which is conceded by every inquirer, has been unbroken, and that during the so-called Glacial period, instead of being a lofty plateau as it now is, it was a comparatively low-lying land. I ventured further to argue, and also, I think, to prove, that with the rise from the sea of this great island there has been a corresponding fall in its temperature, which is a very natural and simple *a priori* meteorological conclusion, and to adduce a number of biological facts to show that during the so-called Glacial period Greenland, instead of having been mantled with an ice-sheet as it is now, had a comparatively mild climate; and that its scanty fauna and flora, instead of having been the result of sporadic migrations thither since the so-called Glacial age, is, in fact, the expiring and shrinking remnant of a once much more important life. The facts and arguments I adduced have nowhere been traversed or met, and it seems to me they are unanswerable.

I now propose a more hazardous work, namely, to apply the same arguments I used in regard to Greenland to Scandinavia and its borders, more hazardous because Scandinavia is the very sanctum of the Glacial geologist, and in attacking him there we attack him in his citadel. I may premise my remarks by saying that I have paid two visits to Sweden, one to the island of Gothland, and one to Finland.

That Northern and Central Scandinavia have recently risen from the sea, has been known since the beginning of the last century, and has been the subject-matter of many memoirs and many researches, in which Lyell, among others, distinguished himself. I do not know anyone that disputes or denies the conclusion. The fact is thus stated by that picturesque and careful writer, Réclus:—Elevated beaches, which can be traced by the eye like the steps of an amphitheatre, are arranged in stages at various heights on the slopes of the mountains. Heaps of modern shells are found at heights of 500 to 650 feet above the level of the sea; and the great branches of pink coral formed by the *Lophohelia prolifera*, which lives in the sea at a depth varying from 1,000 to 2,000 feet, are now raised up to the base of the cliff.¹ The pine-woods, too, which clothe the

¹ Carl Vogt, *Nordfahrt*.