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ÆGER LONGIROSTRIS, G. G. Münster, Petrefactenkunde (1839), pl. xxvi. p. 67. *Lithographic Slate*; *Eichstädt.* Münster Collection. [T.]

ALVIS OCTOPUS, G. G. Münster, Petrefactenkunde, heft. iii. (1840), pl. i. f. 1, p. 20. *Lithographic Slate*; *Eichstädt.* Münster Collection. [T.]

BOLINA ANGUSTA, G. G. Münster, Petrefactenkunde (1839), pl. ix. f. 14, p. 24. *Lithographic Slate*; *Solenhofen.* Münster Collection. [T.]

ERYON LATUS, G. G. Münster, Petrefactenkunde (1839), pl. v. f. 6, p. 9. *Lithographic Slate*; *Eichstädt.* Münster Collection. [T.]

ERYON MEYERI, G. G. Münster, Petrefactenkunde (1839), pl. iii. f. 1, p. 6. *Lithographic Slate*; *Solenhofen.* Münster Collection. [T.]

ERYON OVATUS, G. G. Münster, Petrefactenkunde (1839), pl. vii. f. 3, p. 12. *Lithographic Slate*; *Solenhofen.* Münster Collection (counterpart). [T.]

ERYON SCHUBERTI (Meyer), G. G. Münster, Petrefactenkunde (1839), pl. vii. f. 9, p. 13. *Lithographic Slate*; *Solenhofen.* Münster Collection (counterpart.)

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ELONICHTHYS BINNEYI, R. H. Traquair, GEOL. MAG. Dec. III. Vol. V. (1888), p. 251. *Lower Coal Measures*; *Stanton, Derbyshire.* Binney Collection. [T.]

GYRODUS GRACILIS, G. G. Münster, Petrefactenkunde, heft. iii. (1840), pl. viii. f. 2*a*, *b*, p. 128. *Lithographic Slate*; *Kelheim.* Münster Collection. [T.]

GYRODUS RUGOSUS [Münster MS.], L. Agassiz, Poiss. Foss. vol. ii. (1839), pl. lxix. f. 1–4, p. 227. *Lithographic Slate*; *Kelheim.* Münster Collection. [T.]

HOLOPTYCHIUS SAUROIDES, Williamson, E. W. Binney, Trans. Manchester Geol. Soc. vol. i. (1841), pl. v. f. 8, 10, p. 165. *Coal Measures*; *Pendleton.* Binney Collection.

MEGALICHTHYS HIBBERTI, Agassiz, E. W. Binney, Trans. Manchester Geol. Soc. vol. i. (1841), pl. v. f. 4, p. 162. *Coal Measures*; *Pendleton.* Binney Collection.

MESOLEPIS MICROPTERUS, R. H. Traquair, Trans. Roy. Soc. Edinb. vol. xxix. (1879), p. 356. *Lower Coal Measures* (*Dalemoor Rake Ironstone*); *Stanton-by-Dale, Derbyshire.* Binney Collection. [T.]

PALÆONISCUS EGERTONI [Agassiz, MS.] (Egerton), E. W. Binney, Trans. Manchester Geol. Soc. vol. i. (1841), pl. v. f. 12, p. 166. *Coal Measures*; *Pendleton.* Binney Collection.

NOTICES OF MEMOIRS.

I.—THE MOUNTAINS OF SCOTLAND. By MARCEL BERTRAND, Professor in the École des Mines, Paris. Translated by G. A. LEBOUR, M.A., Professor of Geology in the Durham College of Science, Newcastle. From the "Revue générale des Sciences pures et appliquées," No. 23, December 15, 1892.

THE chief impression carried away by all visitors to Scotland is that Scotland is a country of mountains. Notwithstanding the somewhat low altitude of the heights (the tallest peak, Ben Nevis, does not reach 1400 metres), one could at many points imagine oneself in the upland valleys or on the higher plateaux of the Alps.

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Latitude has something to do with this. As is the case with the flora, with which its aspects are associated, so there is in the scenery a northern character which in many ways reminds one of the Alpine character. The impression produced corresponds no less with a geological truth: Scotland is one of the countries in which are to be found the best marked traces of those great movements of the earth's crust by which mountains have been created. It is the witness of an ancient chain which ran on into Norway, of which the central core must have been located in the Grampian *massif*, and of which the slightly diverging subordinate ranges were directed to the north-east, towards Edinburgh, and nearly to the north along the western coast, facing the Hebrides. This chain is doubly interesting, first because of its size and of the complex accidents which the work of the last ten years has made known within it, and also because of its great antiquity: the *Caledonian Chain* is one of the oldest, if not the oldest, which it is given us to reconstruct. One finds oneself there face to face with movements dating from the dawn of primary times, that is, from a time when theoretically[1] the mean thickness of the crust affected by these movements must have been sensibly less than at the time of the formation of the Alps. Was there in this an appreciable cause of difference in the behaviour of the phenomena? What analogies as regards the whole and what modifications as to details will be brought out by a comparison with the Alps? No doubt the interpretation of these differences will always remain a little arbitrary; but it is enough to indicate the question to which they may belong to show the interest which they possess. If it be added that the investigations carried out in the North of Scotland seem to be of a nature to throw some light on the still so obscure subject of the gneissose rocks, it will be seen that they are of an order far surpassing that of a mere regional description. They are, I think, among those that deserve to be presented in some detail to the readers of the "Revue."

Thanks to a courteous invitation from Sir Archibald Geikie, Director-General of the Geological Survey of Great Britain, we were enabled, M. de Margerie and I, to study last summer the region of Assynt, under the guidance of Mr. Peach, who specially directs the work of the Scotch map.[2] Mr. Peach made us verify the principal sections published; we saw with him the irrefutable evidence on which they are based, and we admired the minute accuracy of the survey. Mr. Peach said to us at starting: "I do not think that there is now in the whole of Great Britain any region better or more completely known than that which we are about to visit." We may add, after what we have seen, that in

[1] The theory of the cooling of the globe is not free from objections; it is necessary to admit that the earth is at the same time homogeneous, solid, and plastic. These are hypotheses which are certainly not all exact and that are admissible as approximations only. I think, however, that the formulæ may be considered as indicating the drift of the phenomena.

[2] We had the good fortune of making this excursion in the company of Baron von Richthofen, the eminent Berlin professor, Professors Hughes and Sollas, and Messrs. Harker and Watts.

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no other part of Europe has it been possible for geological mapping to be carried further, or to do greater honour to its authors.

I. Before describing the results of the most recent work, it will be well to say a few words as to that which preceded it. This part of Northern Europe has long been famous for the discussions to which it has given rise. The discovery of fossils in the Durness limestone in 1854 induced Murchison to study the region, and it was he who drew attention to the peculiarities of its structure; a thick series of sediments, quartzites and limestones, rests to the west on the gneisses of the coast, and dips regularly, and at a somewhat low angle, beneath an enormous mass of micaceous schists and of other gneisses, without presenting anywhere indications of interruption or unconformity. Murchison concluded that the upper gneisses were "newer gneisses," and that these must be regarded as Silurian deposits, overlying the fossiliferous beds, and metamorphosed at a later period.

Professor Nicol, of Edinburgh, who accompanied Murchison on his first visit, was the first, on returning to the same investigation in the following years, to propose a different interpretation. He thought that the Silurian quartzites and limestones of the base were repeated several times by folds and by faults; that the conformity with the upper gneisses was only local and apparent, and that in reality there occurred between them a fault along which the gneisses of the east had been pushed over the Silurian. These remarkable views—destined to brilliant confirmation in the future—were supported by a comparison with the known phenomena of the Alps. They were unfortunately not accompanied by those decisive proofs which would at that time alone have caused such innovations to be accepted. Murchison's authority prevailed, and for more than twenty years the existence in Scotland of Silurian gneisses was admitted without discussion, and unreservedly.

The question, taken up from 1878, in the notes of Messrs. Hicks, Bonney, Hudleston, and Callaway, entered upon a new phase upon the publication of the memorable Memoir by Professor Lapworth, entitled "The Secret of the Highlands.[1] Professor Lapworth, in 1882 and 1883, set himself to make a detailed survey of the Durness and Erriboll districts; he was thus led to revive the views of Nicol, but supported this time by decisive proofs.

These proofs were singularly difficult to discover. Fossils were wanting, or nearly so; it was necessary to carry out a stratigraphical study based upon lithological characters only, and in order to do this it was requisite that a sufficient number of precise horizons should be singled out in a series very uniform in appearance, or at least in which there were only two very distinct members. I remember the astonishment, I might almost say the terror, which seized M. de Margerie and myself the first day, when Mr. Peach pointed out to us the characters which differentiated these successive horizons; their apparent insignificance, their almost intangible

[1] Geol. Mag. Decade II. Vol. X. 1883, pp. 120, 133, and 137; and Proc. Geol. Assoc. 1884, vol. viii. p. 438.

nature, immediately called forth two thoughts; the first, that one would have been incapable of distinguishing them oneself, and the second, that one must be very bold to establish such grandiose results upon bases of this kind. But these characters, recognized at first in a limited field, were found, ever the same, without modification, for a distance of 150 kilometres. Messrs. Peach and Horne have multiplied the number of these horizons, and their subdivisions, as well as the chief divisions of Professor Lapworth, are repeated in the entire region with the same precision. Each proof, taken separately, would seem of small value; but they are cumulative, and together form an indestructible whole.

Professor Lapworth has shown that the apparent enormous thickness of the system was formed by a small number of beds, "piled again and again," and indefinitely repeated, always with the same direction of dip. After this it becomes natural to ascribe the presence of this gneiss above the Silurian to this same phenomenon of "heaping." There the proofs are of another order, and are drawn from the nature of these gneisses and mica-schists; for it can be shown that they have been subjected to tremendous movements, that their particles have undergone a true *re-arrangement*, which, however, allows the recognition here and there of certain portions, less altered, of the ancient gneiss of the coast or of the Silurian divisions. It is to be looked upon as a heterogeneous mass, crushed and pounded by mechanical action and reproducing, in consequence of a sort of general cleavage, the appearance of primitive stratification.

In this way the existence of phenomena, hitherto acknowledged only in certain portions of the Alps, and in the Franco-Belgian coal-field, became recognized in the North of Scotland; the displacement and horizontal carriage of thick superficial masses over several kilometres. Similar examples are now abundant; one led to the other. But ten years ago this was not the case, and I have been told that Professor Lapworth, seized by a kind of fever in facing the consequences which he saw successively opening out before him, in his dreams thought himself caught up in the cogs of these tremendous movements, and crushed along the planes of thrust.

Few geological careers exhibit successes comparable to those of Professor Lapworth. In the South of Scotland, it was by means of Graptolites—lowly organisms of which the palæontological value might appear but slight—that he established horizons in a series that had defied all efforts, and the zones determined in the little corner of Dobb's Linn are now detected all over Europe, and even in America. For the North of Scotland it is with still more insignificant data, with tracks of worms, with lithological differences of colour and grain, that he has fixed his horizons—horizons which, here again, have been found to possess unexpected constancy and extension. By the aid of these tools, forged by himself, and which others would have disdained, he has given the key to the geology of two great provinces of Scotland; he has thus done for the stratigraphy of the Highlands what Sir Archibald Geikie has done for the history of the eruptions of that region, and both names will

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remain associated with one of the most brilliant phases of Scottish geology.

II. The share of those who have followed Professor Lapworth is sufficiently handsome to enable me, without depreciating their merits, to insist upon those of him who was the *precursor*. The earlier work had shown general affinity to Alpine structure. That of Messrs. Peach and Horne, whilst adding precision to the resemblance, at the same time has brought out differences of which the interest is considerable.

The Geological Survey, although rather prejudiced in favour of the old views, hastened to undertake the general study of the region where the phenomena pointed out by Professor Lapworth could be verified and followed out. Already, in 1884, Mr. Peach, placed in charge of these investigations, was able to invite the Director-General to come and ratify the decisive results which had been obtained, and Sir Archibald Geikie, abandoning the opinion he had hitherto held, lost no time in accepting the evidence of the facts, and in widely announcing the change of view by publishing in "Nature"[1] the conclusions to which the officers of the Survey had been led, step by step, and almost in spite of themselves. But it was only in 1888 that a detailed report was issued embracing the whole of the observations made by Messrs. Peach, Horne, Gunn, Clough, Hinxman, and Cadell.[2] Since then, the observations have been completed, but the Report of 1888 still represents the principal features of the results obtained.

These results are remarkable for more reasons than one. On hearing that the mountains of Scotland showed traces of horizontal displacements comparable to those of more recent mountain ranges, it was possible to suppose that the ancient chain would prove to be constructed on exactly the same plan as the Alps. Later, on finding in the sections of Messrs. Peach and Horne important differences between them and the classical types of Switzerland, it was still possible to believe that these differences might be more apparent than real, and that by varying the method of interpretation they might be reconciled. All such expectation must, I think, be given up; the differences are real and profound; their causes and their theoretical importance can be discussed, but their existence cannot be contested.

To begin with, there are three great planes—or better, three great independent surfaces of "push" or carriage (thrust planes). Each of these has produced horizontal displacements of several kilometres. For the third, the most easterly, these displacements attain 15 kilometres. The first two have brought into superposition above the normal series only beds similar to those which they overlie; the third, on the contrary, has brought a different series into superposition—precisely that of the ancient "recent gneisses" of Murchison. In places, the last thrust plane comes to override the first two; so that there is then a three-storied edifice, the two lower

[1] "Nature," 13th November, 1884.
[2] Quart. Journ. Geol. Soc. 1888, p. 378.

stories built up of the same materials as the basement, and the third formed of different materials. What is more extraordinary, but nevertheless appears to be certain (though I have seen but one example of it, and could not devote sufficient time to its examination), is that the third story does not everywhere rest upon the second, but cuts across the latter obliquely so as to lie upon the first, or even immediately upon the basement. This is an extraordinary complication, of which other instances have not been recognized. Series of thrust planes, arranged one behind the other in *échelon*, and each producing horizontal displacements of several kilometres, are well known, notably in Provence. But each of these thrusts corresponds with a distinct fold; each one has its own domain, and does not trespass upon its neighbour's. In Scotland one cannot resist the impression that one is face to face with a unique phenomenon, and that the division in three folds, each forming one of the three stories, is non-existent.

There is, however, a difference still more important in my eyes; it is the absence of *inverted beds*. In the Alps and in Provence what characterizes these phenomena of horizontal movements is the more or less intermittent presence of beds succeeding one another in inverse order, the oldest above, the most recent below. These inverted beds are at the same time generally *dragged out*; that is to say, the normal thickness of the divisions is there very greatly reduced. It is these beds that seem to give the clue to the phenomenon and that have allowed Professor Heim to formulate his theory, comparing it to the unfolding of a fold, the base of which, forced to stretch over a larger area, undergoes a kind of rolling out. It would be very easy, it is true, to answer that in Scotland the diminution in thickness has proceeded to zero; that the dragging out process has been carried on so as to become suppression, and the same mechanism can still be resorted to. But there is something further: above each thrust plane there are in Scotland also beds in an abnormal position, only this position is of quite another kind; these beds are oblique to the thrust plane and are indefinitely repeated in consequence of a series of small faults with a hade a little larger than the dip of the beds. In other words, each of our three stories has a *floor*; but this floor, instead of being formed of planks parallel to the base of the story, is as if it had been formed by cutting these planks into slices obliquely uptilted. All these slices are similar among themselves, always sloping to the east, *i.e.* towards the side whence the movement has come, and *the beds are never inverted.* The small separating faults are usually not very obvious, and consequently, finding similar beds recurring over large areas, without apparent horizons and always dipping in the same direction, one would be led to attribute to them impossible thicknesses. It is here that the utility of the subdivisions introduced in the series becomes manifest: one is confronting a set of *numbered* beds, the numbers of which, though not very distinct, are yet well known and are everywhere recognizable by minute examination. This examination has been carried out in an extraordinarily careful and conscientious

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manner. In the region visited by us, in the midst of this maze of beds always resembling one another and repeated in narrow compartments, we did not come across a single compartment of which Mr. Peach was not able to show us the detail represented on his plottings on a scale of $\frac{1}{10560}$; not one bed of which Mr. Peach could not beforehand tell us the number.

The arrangement that I have been attempting to describe is exactly what Professor Suess has called *schuppen-structur* (imbricated structure), and it is to be seen produced, in the Bernese Jura for instance, as a particular case of folds; it is, in fact, the structure which one would obtain by imagining a series of folds inclined in the same direction and by further supposing that in all these folds the half corresponding to the inverted beds had been suppressed. What makes the explanation difficult here is the fact that the movement is localized in the floors of the three stories, that is to say in bands of deposits of small thickness; one is at first led, under these conditions, to attribute it to the friction due to the masses which have been moved. It would be easy to suppose that these bands had been plicated by the shearing, as a cloth can be wrinkled by passing the hands over its surface, but it is less easy to conceive how these folds have been replaced by half-folds, or more exactly how the plication can have been replaced by a breaking up into fragments accompanied by a uniform uptilting of the successive fragments.

Without for the present seeking for the meaning and cause of each of these complications, a common character becomes distinguishable: folds are absent or exist only in a concealed form. At least, those that are met with are local accidents which can scarcely be appealed to in explaining the whole of the phenomena. Schematically one can always refer any movement to a fold of which a portion has disappeared; but whereas in the Alps such a disappearance is always momentary and allows the complete fold to reappear at a short distance, in Scotland the disappearance of the inverted portions is constant and almost without exception. In the one case, the explanation by means of folds is the result of direct observation; in the other it is based on *a priori* reasoning. The difference may be expressed in yet another way: in the Alps the suppression of beds is almost always due to sliding parallel to the stratification, and I have attempted in a previous article[1] to explain that this was a natural consequence of the parallelism between the beds and the forces of compression. In Scotland there is the same parallelism and yet the slides, with the exception of the three great thrusts, are almost always oblique to the stratification of the deposits. For so complete a change there must certainly be a general and deep-seated cause. This cause cannot reasonably be sought for in the nature of the forces in action; it must therefore depend upon the resistances brought into play. A remarkable series of experiments by Mr. Cadell, partly carried out with the collaboration of Mr. Peach, is perhaps such as to throw some light upon the question.[2]

[1] Revue générale des Sciences. t. iii. p. 1.
[2] Trans. Royal Soc. Edinburgh, vol. xxxv. part ii. p. 337.

It was necessary to use substances capable of plication within certain limits, but incapable of supporting a greater strain without fracture; alternations of plaster of Paris and damp sand fulfilled this condition; moreover, deformation was allowed free play by not weighting by the addition of any foreign body the mass of compressed beds. This was to deviate appreciably from the natural conditions where the weight of the beds, owing to the magnitude of the masses in motion, plays a considerable part, whilst in the experiment this action is negligeable. On the other hand, if by these means structures are produced comparable with those of the Highlands, one may conclude that the causes of the peculiarities presented are connected with a diminished *plasticity*, due either to the nature of the beds, or rather to their diminished thickness. It is, indeed, admitted now, following Professor Heim, that the plasticity to which the folds of mountain beds testify is due, on the one hand, to the slowness of the movements, and, on the other, to the enormous weight with which the folded parts were charged. For solid bodies, with sufficient pressure, the distribution of this pressure becomes, as with liquids, equal in every direction; and then, even if the cohesive forces be overcome, the particles, energetically maintained from all sides, are incapable of any but very slight relative motion; the body is deformed progressively, and without breaking.

As a matter of fact, Mr. Cadell's experiments reproduced with surprising fidelity some of the singular sections described in Scotland, notably the superposed thrust planes, and the beds piled up over themselves. The starting point is always the formation of a fold; but the beds thus brought in relief, being no longer sustained laterally, break, and their fragments transgress one over the other; the fold remains visible in the depth, although its form has disappeared from the surface owing to this kind of breaking up into pieces. As regards the piling up, the experiments produce it only in the upper portion of the system, almost always above a thrust plane, and as an extreme case of the collapse of an arch. The friction exerted by an upper thrust plane therefore plays no part here, and one is led to think that it may have been so likewise in the natural phenomenon, or at least that the friction above has only intervened so far as to cause the piled up fragments to lie in the direction of the movement, and to dispose them more horizontally. In other words, the slices of piled up beds would represent slices which have advanced further than that which is above them, and not so far as that which is beneath; and which, moreover, because of their small thickness, have undergone in a greater measure the retarding or accelerating action of the masses between which they lay. In this form, it will be at once perceived that the formula can be generalized, and gives in a very simple manner the general explanation of the phenomena; the thrust masses have been split up into a series of horizontal slices which have moved independently of one another;[1] the thick masses in one piece, the others subject

[1] This is exactly the formula which I proposed two years ago, when pointing out

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to the secondary influence of the neighbouring masses. Taken thus, this formula only differs from that by which I attempted before to summarize the Alpine movements in that, as regards the latter, the successive slices are precisely the beds themselves, the planes of division being the planes of stratification, and therefore almost indefinite in number. In Scotland, as in the Alps, the pressures at first would form an arch, then, as they became greater, the arch in the one case would be bent downwards, and in the other would break. Pressure continuing, sliding movements would take place, following the bedding in the reclining fold, and following the fractures in the broken one. The difference between the two regions would be due to a difference in plasticity, *i.e.* to a difference of weight or thickness in the masses compressed. It will be seen how well this explanation agrees with the theory,[1] according to which the thickness of the superficial rocks compressed in consequence of the cooling of the earth increases with time. The differences between the mountains of Scotland and the Alps would be *a question of time.* No doubt this is yet but a hypothetical consequence; it may be added, however, that it is further confirmed by a comparison with the Coal-measure range. This chain, so well studied by Prof. Gosselet in the Ardenne, presents, on the borders of the coal-field, an intermediate structure, the part played by faults being there more accentuated than in the Alps, and less so than in Scotland. In any case, I cannot share Mr. Cadell's opinion when he says that in examining the Alps by the light of the new facts recognized in the Highlands, and confirmed by laboratory experiments, the same structures will be found there. It is not the observers who are at fault, it is the mountains which are not the same.

III. Side by side with the study of the movements themselves must be noted that of the metamorphism due to them. Nowhere, perhaps, outside the works of Reusch, in Norway, have clearer or more instructive examples been described. In the conglomerates, pebbles are seen to lengthen in the direction of the movement, sericite is developed in the quartzites, transforming them into micaceous schists traversed by little felspathic veins ("veins of pegmatite"). All these effects go on increasing as one approaches the east, and beneath the last great thrust plane we were enabled to see a remarkable section where, by reason of the repetition of the beds, one can follow almost step by step the progress of transformation, to a point at which the beds altogether cease to be recognizable, and can no longer be distinguished from the mass of crystalline schists ("Moine Schists") which surmount them.

At other points, on the contrary, where the upper "story" (in consequence of a lesser degree of denudation) advances further to the west, the separation of the two systems is clear and distinct; there is then found at the base of the crystalline schists a veritable

that the Scottish sections exhibited the material realization of this theoretical idea. Comptes Rendus Acad. Sci. 29th December, 1890, Report of M. Daubrée on the "Vaillant" prize.

[1] Davison and Darwin, Phil. Trans. Roy. Soc. 1887, p. 281.

friction breccia several metres thick in which the broken fragments are visible to the naked eye, but which is divided into parallel layers and appears stratified. The bedding is here evidently mechanical; it is a cleavage of the broken mass. For Mr. Peach the phenomenon is the same when the fragments cease to be visible and when they melt into an entirely crystalline paste. Everything then, the texture of the rock, as well as the appearance of sedimentation or its crystalline structure, would be the result of mechanical action, and that over an area of some thousands of square kilometres. It would even not be necessary to regard this enormous mass of schists as corresponding to any particular stage; the materials of which it consists would be furnished pell-mell by the rocks and deposits anterior to the movements. Here the hypothesis becomes somewhat too colossal to enable one to associate oneself with it. It seems to me difficult not to see in these crystalline schists as a whole, one special formation of definite age, which if not yet determined is certainly anterior to the Cambrian, a true stage (*étage*) in the ordinary acceptation of the term, and more or less to be likened to the micaceous schists and slates (*phyllades*) which in France form the top of the crystallophyllian series. It may be admitted that the apparent thickness of this group is augmented by a series of thrust planes and by the mechanical piling up already described; it may also be admitted that the visible bedding is cleavage. But in the absence of new arguments one can scarcely go further; the still recognizable patches of ancient gneisses or of Silurian beds found within the mass, must be considered as more ancient or more recent parts brought in by folds or by faults; but they cannot bear out the conclusion that the whole mass was formed of the same materials. As to the sudden appearance of a thick sedimentary series entirely wanting a few kilometres to the west, it reminds one exactly of what takes place in the Alps in the zone between the *massifs* of Mont Blanc and Monte Rosa. It would even be possible to follow, member by member, a curious correspondence in position between the series of the two regions: the gneisses of the Mont Blanc chain would tally with those of the Scottish coast; on these gneisses rest unconformably on the one hand the Carboniferous deposits, on the other the Torridon Sandstones of which I shall speak presently; the quartzites and limestones of the Trias take the place of the Cambrian quartzites and limestones of Scotland, and, east of the last named, instead of seeing a reappearance of the older divisions over which they lie to the west, we encounter only an immense succession of schists unknown on the other side, the *Moine schists* in Scotland and the lustrous schists in the Alps. In these last, moreover, patches of Trias are found intercalated, as shreds of Cambrian are enclosed in the schists of Scotland.

This is, however, not the place to insist upon these *Moine Schists*, the question which of all others remains the most obscure, and on which next to nothing has been published. The last observations I wish to mention in concluding are those relating to the gneiss rocks of the coast. There the post-Cambrian movements are no longer felt; the prodigiously dislocated region which we have been

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studying is succeeded by another where reigns the appearance of the most complete calm, where the gneisses themselves are but feebly and gently undulated, where the sandstones which overlie them have almost everywhere remained horizontal. These sandstones (Torridon Sandstone), of which I have already said one word, recall the aspect of our Permian stage by their coarsely detrital nature and by their red and brown colour; in their entire thickness, which is great, there is no trace of metamorphism. They are, however, incontestably older than the Cambrian quartzites everywhere to be seen resting unconformably with a slight dip to the east on the slices of the obliquely cut horizontal sandstones. Erosion has deeply sculptured these sandstones into isolated peaks of quaint and abrupt shapes, like immense fortresses guarding the coast-line; strangers, as MacCulloch said—the first geologist who described them—by their nature and by their structure to all that surrounds them, they seem astonished to find themselves where they are. Their horizontality, their freshness, their resemblance to more recent deposits, these are so many indications that the basement on which they rest has itself remained much as it was in those ancient times. What can be studied at their base then are the gneisses such as they were before the Cambrian epoch.

This is a point of great importance; it is owing to circumstances of this kind that the study of the gneisses will always be easier and more fruitful in more northern countries than in ours. Where the Earth's crust has been dislocated up to more recent times, the gneisses are only exhibited to us with all the transformations due to metamorphism and to the successive injection of igneous rocks. It does not appear that this complexity has given rise to special types (though I do not think that a serious study, without preconceived views, has yet been carried out in this direction); but at any rate such a complexity renders it more difficult to assign to each agent the part it may have played. Where on the contrary, as in Scotland, deformation has stopped at its first stage, the problem is simplified, and the chances are greater that precise conclusions may be arrived at.

The gneisses of the Scottish coast are basic gneisses, in which the mica is replaced by amphibole or pyroxene; they are granitoid gneisses, *i.e.* the arrangement of the dark-coloured materials along parallel lines is but faintly indicated in them. They are traversed by numerous basic veins, from diabases to peridotites, and all anterior to the Torridon Sandstone. These gneisses, as I have said, constituted the platform against which the tremendous flood of the Silurian displacements stopped without shaking it; but in it are found traces of more ancient movements, later than the basic veins, and prior to the Torridon Sandstone. These movements it has been possible to study in detail, and they have yielded precious data.

They are narrowly localised along lines or bands of small breadth, compared by Mr. Peach to vertical thrust planes; along these lines, which sometimes follow the veins, the latter are transformed into hornblende schists, with a little mica and lenses of dioritic matter; the peridotites pass into talcose schists. In the gneisses there is mica formed in lines along the direction of the movement; the new planes of orientation are independent of the primitive foliation, which they cut across, and sometimes obliterate completely, and all the stages of the changes can be followed. It will be seen, since the original foliation here plays no part, that the result would have been the same in a granite; one is assisting, in fact, *at the formation of gneiss, and of amphibolic schists, by means of the foliation of granitoid rocks.* This theory has already been several times proposed, and has been cited in support of other observed facts; but I do not think that any of these facts can present themselves more clearly or offer an easier verification than those under consideration.

It may be objected that the effect is produced in narrow areas only; but it is so by a phenomenon which is only exceptionally localised, and which, from what is known of its other effects, is capable of extension over masses of almost indefinite size. One is thus in a position to conclude that the foliation of eruptive rocks is one of the possible and even probable modes of formation of gneiss. In the first place, as has been seen already, the same mechanical metamorphism may be applied to sedimentary rocks. Moreover, M. Michel Lévy has proved that the injection of eruptive into sedimentary rocks can equally produce true gneisses: perhaps only in this case is it less easy to conceive of the extension of the phenomenon to vast areas. Lastly, there may be gneisses formed originally and directly in the state of gneiss, corresponding in such a case to the first crust of the solidified globe. Dr. A. C. Lawson has described an interesting example of this in Canada, which would approximate this mode of formation to that of the eruptive rocks. Here are, then, three possible origins, outside all theory, and it must be owned that choice, in the present state of knowledge, is very seldom possible for each particular case. The observations of Messrs. Peach and Horne mark, however, a further step towards the solution, and what L. von Buch said of the Tyrol may be repeated here. All geologists who take up these questions should make a pilgrimage to the Scottish coast.

In concluding this summary, it remains to me to express the desire that these beautiful discoveries may be, as soon as possible, published in all their details. The Report of 1888 is but a *résumé* of the most important facts; one has a right to expect, and to demand, a complete monograph. All the elements of such a monograph will soon be gathered together; they must be made public. It is desirable that a micrographic study should accompany and give precision to all the observations relating to metamorphism, and especially that it may be possible to follow the perusal of the Memoir on detailed maps. An immense amount of labour has been undergone in carrying out this survey: the reduction to a scale one-sixth of that of the original maps will cause an almost inextricable complication of lines, and the loss of the benefit of part of the work. The region presents an interest sufficiently exceptional, and sufficiently general, to raise it above the rules of ordinary routine, and the question of cost is not an obstacle. England is rich enough to pay for its glory.

II.—On a Meteoric Stone found at Makariwa, near Invercargill, New Zealand. By G. H. F. Ulrich, F.G.S., Prof. of Mining and Mineralogy in the University of Dunedin, New Zealand. (Proc. Royal Society, London, 1893.)

THE specimen described in this Memoir was found in the year 1879, in a bed of clay, which was cut through in making a railway at Invercargill, near the southern end of the Middle Island of New Zealand. Originally this meteorite appears to have been about the size of a man's fist, and to have weighed 4 or 5 lbs., but it was broken up, and only a few small fragments have been preserved. The stone evidently consisted originally of an intimate admixture of metallic matter (nickel iron) and of stony material, but much of the metallic portion has undergone oxidation. Microscopic examination of thin sections shows that the stony portion, which is beautifully chondritic in structure, contains olivine, enstatite, a glass, and probably also magnetite, and through these stony materials the nickel iron and troilite are distributed. The specific gravity of portions of the stone was found to vary between 3·31 and 3·54, owing to the unequal distribution of the metallic particles. A partial chemical examination of this meteorite was made by the author and Mr. James Allen, but the complete analysis has been undertaken by Mr. L. Fletcher, F.R.S., of the British Museum. The analysis, which, when finished, will be communicated to the Royal Society, has gone so far as to show that the percentage mineral composition of the Makariwa meteorite may be expressed approximately by the following numbers; nickel iron 1, oxides of nickel and iron 10, troilite 6, enstatite 39, olivine 44.

REVIEWS.

I.—An Introduction to the Study of the Genera of Palæozoic Brachiopoda. Part I. By James Hall, assisted by John M. Clarke. Geological Survey of the State of New York. Palæontology. Vol. VIII. 1892. Large 8vo. pp. xvi. 367, with 43 Plates.

IN many valuable volumes Professor Hall has described the Palæozoic fossils of the State of New York. In the long series of years during which he has been labouring in his appointed field, progress has necessarily been made in other lands and by other workers. Consequently, work that was admirable twenty years ago runs some risk of being overlooked at the present day unless it is brought into line with recent discoveries. To summarise and consolidate the knowledge of the past enables us to appreciate better both the perfections and imperfections of our science, and affords the surest ground for further progress. This is the task that is now being attempted for Palæozoic Brachiopoda by Messrs. Hall and Clarke, and the first-fruits of their toil are seen in the volume before us.

The plan of this work is to describe very completely each genus