

EXPLANATION OF PLATE II.

- FIG. 1a.—*Heterodictya gigantea*. A broken frond, of the natural size. The specimen is split longitudinally along the line of the central laminar axis, and thus shows the bases of the cells.
- FIG. 1b.—Portion of the same, near its smaller end, enlarged to show the penniform arrangement of the cells.
- FIG. 1c.—Transverse section of the frond, of the natural size.
- FIG. 1d.—A few of the cells of the same viewed in profile, showing the tabulæ. Enlarged.
- FIG. 1e.—A small portion of the surface greatly enlarged, showing the shape of the bases of the cells. On the left hand side of the figure, a portion of the longitudinally striated laminar axis is preserved.
- FIG. 2.—*Ptilodictya cocciniformis*, a broken specimen growing on *Heliophyllum Halli*. Of the natural size.
- FIG. 2a.—Portion of the same enlarged, showing the meshes of the network, and their striated borders.
- FIG. 2b.—A portion of the same still further enlarged, showing the form and arrangement of the cells and the interstitial tubuli.
- FIG. 3.—*Fenestella Davidsoni*; a small portion of the non-poriferous side. Of the natural size.
- FIG. 3a.—Portion of the same enlarged.
- FIG. 3b.—Portion of the poriferous side of another specimen of the same, enlarged.
- FIG. 3c.—Small portion of a branch of another example of the same, greatly enlarged.
- FIG. 4.—Fragment of *Retepora Trentonensis*, of the natural size.
- FIG. 4a.—Portion of the same enlarged, showing the arrangement of the cells.
- FIG. 4b.—Portion of another example of the same, enlarged; showing the striated non-poriferous surface.
- FIG. 5.—A small crust of *Ceramopora Huronesis*, growing on *Heliophyllum Halli*, enlarged.
- FIG. 5a.—Portion of the same, greatly enlarged, showing the form of the cells and cell-mouths.

V.—MODERN 'VULCANICITY.'¹

By J. CLIFTON WARD,

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THE theory proposed of late years by Mr. Mallet to account for volcanic and earthquake phenomena, while having a charm about it from its very simplicity, is one which must nevertheless meet with very decided criticism from geologists.

Mr. Mallet sees "linked together, as parts of one grand play of forces," the elevation of mountain-chains, the production of volcanos, and the origination of earthquakes. His theory, however, necessitates the following suppositions:—

1. "That the geological doctrine of absolute uniformity cannot be true as to Vulcanicity. . . . Its development was greatest at its earliest stages." (p. 75.)¹

2. That the movements of elevation and depression at the present time are "slow and small," but these, "at a much remoter epoch, acted upon a much grander and more effective scale." (p. 62.)¹

On page 47¹ the "stratigraphic geologist" is described as one who discerns "a change in the order or character" of the "fused masses which have come up from beneath. He sees immense outpourings of granitoid or porphyritic rocks that have welled up and overflowed the oldest strata. . . . Later he sees huge tables of basaltic rock

¹ Introductory Sketch to Palmieri's Vesuvius.

poured forth over all." Such products, which the author says are "commonly called plutonic," he distinguishes from those of the volcano by being "*not explosive*."

Does it not seem that Mr. Mallet is here making a difference between action from below in the early stages of geologic *history* and that action in modern and recent geologic times which does not exist in fact?¹ Volcanic products, both sub-marine and sub-aerial, of the most unmistakable character, occur in rocks of all ages down to the base of the Lower Silurian. Basaltic lavas, in which the component minerals seem to have crystallized in the very same order and under the same conditions as in modern flows, have now been traced back to periods of the world's history before, apparently, vertebrate life came into existence, and when the very ancient order of Graptolites flourished. There is nothing to mark the old sub-aerial volcanic products of Cumbria, and the sub-marine volcanic products of Snowdonia, from those of recent sub-aerial or sub-marine volcanos, *except* the metamorphism which the older rocks have been inevitably subject to, but which has seldom succeeded in obliterating their original character as a whole. There would seem to be as little doubt that 'Vulcanicity' presented phenomena of an '*explosive*' character, characteristic of the volcano, in the English Lake-district during the middle of the Lower Silurian, as that such phenomena now occur on the shores of the Bay of Naples.

But, if this be so, since our geologic history is, properly speaking, bounded by the lowermost of known sedimentary formations, it surely is not safe to say that there is any *essential* difference between modern 'Vulcanicity' and that which prevailed at the earliest stages of the Earth's history.

Let us now examine a little into the truth of Mr. Mallet's supposition that "the great masses of the mountain-chains were elevated" during the "earliest stages" of Vulcanicity. He evidently regards it as unlikely that *great* movements of elevation and depression are now taking place, such as result in the formation of mountain chains or in the depression of such beneath the waters of the ocean, although he does not deny that such chains "may be possibly increasing in stature year by year, or at times; but in any case at a rate almost infinitesimally small in its totality over the whole earth to that with which their ridges were originally upreared." (p. 63 *op. cit.*)

Are there, however, any legitimate reasons for supposing that the movements of elevation and depression were in the earlier course of geologic *history* more rapid and sudden than at present? Is there any evidence, for instance, that in times so far back as the Silurian, great elevations or depressions of land took place at all *rapidly*? Can we with any show of truth assign the origin of the leading chains of mountains to the earliest geologic ages? To answer these questions aright, we must consider denudation as well as upheaval and depression. Prof. Ramsay has shown that in Wales the many thousand feet of strata formed during the Lower Silurian period were upraised, contorted, cleaved, and extensively denuded before the

¹ See also Mr. Scrope's criticism in *GEOL. MAG.* January, 1874, page 31.

Upper Silurian beds were deposited upon them. Can any one conceive of such a denudation as is here implied being effected during a more or less speedy movement? A sweep of waters during some rapid action could not have effected the truncation of many thousands of feet of contorted strata, as Mr. Mallet will probably allow. But given such an action as is now going on around our coasts, and given long periods during which denudation could take effect upon land being slowly upraised, and then as slowly depressed, such an amount of work done between the deposition of the Lower and Upper Silurian strata can be realized. Or to take another example. In Cumberland, conglomerates assigned to the Upper Old Red, or perhaps with more truth to the base of the Carboniferous, lie unconformably upon Upper Silurian beds, upon the Cumbrian Volcanic Series, and upon the Skiddaw Slate. At the close of the Upper Silurian period, the Skiddaw Slate of the Lake-district was probably buried beneath at least 12,000 ft. of volcanic rocks and some 14,000 ft.¹ of Upper Silurian strata; yet between the period of deposition of the uppermost of the Kendal Silurians and the formation of the Conglomerate of Mell Fell, there must have been a removal by denudation of this 26,000 ft. of rock, to say nothing of any thickness of Skiddaw Slate which may have been swept away also. I believe that this denudation took place during the first upheaval of the present Lake-district group of mountains, and it is hard to conceive of any process by which it could have been effected other than the slow but sure gnawing and planing action of the sea upon the slowly rising tract, and the action of atmospheric powers upon those parts fairly above the sea-level. Such a denudation, carried on by such means, gives a forcible idea of the length of the Old Red Sandstone period, and there exists somewhere a thickness or an extent of strata formed during that period, strictly correlative with the amount of denudation produced. If we are to believe that the denudation of a great thickness of rock could be effected during a rapid rate of elevation, we must also believe that a great thickness or extent of strata could be as rapidly deposited. But we know from fossil evidence that sedimentary deposition has been in most, if not in all cases, exceedingly slow; therefore the denudation must have been proportionately so.

With regard to the existing mountain-chains, evidence is not far to seek, showing that *in the main* their formation dates from recent geological periods. If all such giant chains as the Alps or the Himalayas could be proved to be of early Palæozoic origin, and such diminutive mountain groups as those of Wales and Cumberland to be of recent origin, then indeed one might be inclined to argue that forces which raised the former had well-nigh spent their power, and were now only equal to producing slow elevations of 2000 or 3000 feet. But when oftentimes the very reverse of this is found to be the case, when the mountain groups of Cambria and of Cumbria are representatives of *some* of the earliest tracts of land, when the rocks forming the bulk of the Alps and the Himalayas were being formed

¹ Thickness of Upper Silurian in the Kendal district, according to Mr. Aveline.

beneath oceanic waters long ages after the mountains of Wales and Cumberland first began to take form, and when therefore the principal mountain-chains are but infants in age as compared with the Snowdon of Wales and the Scafell of Cumberland, it is surely illogical to assume that the *great* movements of elevation and depression were confined to the earliest stages of Vulcanicity.

There is another statement made by Mr. Mallet which must strike every working geologist. Geology, it is said, must make poor progress, "until all who profess to be geologists shall have learnt that, to make sound progress, they must first become mathematicians, physicists, and chemists." Now no one will deny that geology derives very material assistance from every other branch of science, the students of science forming together one great mutual help society; but to affirm that "sound progress" can only be made by the geologist when he becomes mathematician, physicist, and chemist, is to withhold any hope of progression from the many, and confine it wholly to those few comprehensive minds which arise but seldom on the intellectual horizon. One of the great charms of natural science is the way in which it develops the powers of observation, and of reasoning logically on such observation, and it gives a noble independence of thought which trusts to nature, and cares not for human authority *merely as such*. Surely many, if not most of our leading geologists, who have made our science to progress so rapidly, were neither mathematicians, physicists, nor chemists, much less all three together; but they have been and are careful observers, loving students of nature, ever willing and anxious to receive help from the mathematician, the physicist, and the chemist, but *not* willing to allow these scientists to pervert ascertained facts in order to accommodate them to their own special modes of thinking. For example, we may suppose the case of a mathematician desirous of advancing the science of geology by some new theory worked out by such abstruse mathematical reasoning that the simple field-geologist could not follow the line of argument; and if the latter has reason to suppose that the facts upon which the argument is based be correct, he feels bound to acquiesce in the result. Should, however, the geologist find that the mathematician resolutely refused to believe evidence on some special point founded on the careful observation of nature—such as the production of striæ on rock-surfaces by ancient glacial action—the geologist might well hesitate to accept the mathematical conclusions upon some *other* subject at the basis of which accurate *observational* powers should have been employed.

VI.—SUPPLEMENT TO THE PAPER ON WEST INDIAN TERTIARY FOSSILS.¹

By R. J. LECHMERE GUPPY, F.L.S., F.G.S., etc.

THE descriptions of two of the species enumerated in my paper on the Tertiary Fossils of the West Indies having been accidentally omitted, the defect is now supplied.

¹ See the GEOL. MAG. Decade II. Vol. I. (October Number), 1874, p. 433.