

MR. TOPLEY ON ESCARPMENTS.

To the Editor of the GEOLOGICAL MAGAZINE.

DEAR SIR,—I should not have troubled you with another letter, were it not that Mr. Topley, in your last, unintentionally misrepresents my views so as to make them appear inconsistent. In reply, I shall endeavour to be as brief as possible.

Revival of Old Theories.—Mr. Topley regards with disrespect the act of a geologist going back to old times for an explanation of phenomena, as if conformity to prevailing fashion in a science were more philosophical than a simple desire for truth. In the history of geology, old fashions have often been revived. The glacial theory of the Parallel Roads of Glenroy was framed by Agassiz in 1840, during his Highland tour with Dr. and Mrs. Buckland. It was displaced by the marine theory, which lasted until 1863, when the glacial theory, as explained by Jamieson, received the sanction of the Geological Society. Mr. Topley himself, in his rain theory, has gone back to the days of Hutton and Playfair. The theory of “waves of translation” has found favour with eminent geologists within the last few years, and is still held by Sir Roderick I. Murchison. I do not think Sir Charles Lyell would object to a wave of translation, such as might be caused by an earthquake capable of upheaving a sea-beach to a height of 40 or 50 feet, or that he would assent to the extreme form in which Mr. Topley has stated his protest against “large bodies of water.”

Lyell on Marine Currents.—I never regarded waves as more important denuding agents than currents, and Sir Charles Lyell, so far from disclaiming the latter, lays the main stress on them. He says (if I rightly remember his words) “the chief influence of the ocean is exerted at moderate depths below the surface, on all those areas which are slowly rising, or are attempting, as it were, to rise above the sea.” Currents may have formed the extensive escarpments and terraces revealed by soundings in the Atlantic Ocean, and currents may have commenced those long lines of subaërial escarpment which are rarely paralleled on modern sea-coasts; but most of the escarpments with which I am acquainted show traces of having been at least modified by coast-action. Assuming their littoral origin, England would not be a likely area to present fac-similes of them at the present sea level. Such can only be expected on coasts where the sea is “deep to;” where it is not prevented, by the task of silting up shallows, from following the strike; and where, beneath the line of cliff and the influence of waves, there must be a sloping submarine talus of angular materials, similar to that forming the lower part of many inland escarpments.

So-called Strike Escarpments.—The mode of action assigned by subaërialists to rain and frost involves an entire dependence on structure. Rain and frost can only originate and carry on the work of denudation in conformity to the strike; but on minute inspection it will be seen that many parts of so-called strike escarpments show a dip along the face of the cliff which proves that the denudation

in these parts must have proceeded obliquely to the strike. In those escarpments which consist of a succession of headlands, bays, and combes, a very considerable part actually runs unconformably to the strike; in other words, the bedding is oblique to the planes of marine denudation either above or below the escarpment (instances—the great Cotswold escarpment, the Eglwyseg line of cliffs near Llangollen, etc.). With regard to Mr. Topley's statement that the line of cliff now in course of being formed by the sea in N.E. Yorkshire is unparalleled by any inland escarpment, I think reasons might be assigned why a perfect parallelism should not be found in Britain. It is not true, however, that all the escarpments of this country exhibit a continuation of the same beds. The Lias escarpment N.E. of Taunton may be regarded as a continuation of the Greensand escarpment to the south. The escarpment extending from Uphill, near Weston-super-mare, towards the E. and N.E., embraces, in horizontal succession, a repetition of limestone, Trias, Lias, and, if I remember right, Permian conglomerate.

Short Lines of Obliquely-stratified Cliffs.—These may be met with almost everywhere in the Lake district, and in many parts of Wales, Somerset, etc. (Instances—some of the cliffs of Cader Idris; several cliffs on the S.E. side of the railway, between Penmaen Pool and Barmouth Ferry Station; cliffs in the upper valley of the Wye, at high levels above the river: the cliff behind Clevedon; many cliffs at high levels on the Mendip Hills, especially between Shute-shelf and Longbottom Passes, and on one side of the latter; parts of the celebrated Cheddar Cliffs, etc.).

Strike following Sea-coasts.—Among the instances in which the sea shows a tendency to follow the strike, may be mentioned the cliffs to the south of Clevedon, in Somersetshire. On the sides of Brean Down, near Weston-super-mare, the sea pays about equal regard to dip and strike. On many parts of the west coast of Wales the sea shows a preference for the strike. A whole article, detailing instances in other places, might be written. It is still true, that the sea pays comparatively little "regard to dip and strike," and equally true that dip and strike solely determine the direction of the denudation effected by rain and frost.

Synclinal Hills.—Though to Mr. Topley it may be sufficiently clear, I cannot understand how a hill, consisting of a perfect synclinal basin with the strata dipping inward on *all* sides (in the same paragraph, in speaking of the same hills, Mr. Topley uses the term *most* sides) can show a dip in any part of the face of the surrounding escarpment, as they do in Eston Nab and Upleatham Hill.¹ I venture to believe that the strata of many of the so-called synclinal hills dip towards each other only from two points of the compass, in which cases they do not form basins, but synclinal axes. It is certain that some of these hills have this structure, and that the denudation by which they have been left, has followed the strike on two sides only. In their case the atmospheric theory furnishes no more than half an explanation. They can be fully accounted for by

¹ Topley, *Geol. Mag.* Vol. III. (Oct. 1866), p. 438.

the sea, which shows a versatility not possessed by subaërial agents, and which can breach through and overcome almost any exception to its main mode of action. Many detached hills near escarpments appear to be *decapitated headlands*, and can be at once explained by the well-known tendency in the sea to enlarge bays laterally, until connecting passages are formed. D. MACKINTOSH.

TRIGNMOUTH.

P.S.—As the work of excavation for villas proceeds, the proofs of the marine denudation of the hills and valleys of the Torbay district assume a more and more demonstrative character. On this subject you will soon hear from me again.

GRAPTOLITES.

To the Editor of the GEOLOGICAL MAGAZINE.

SIR,—I am sorry to have again to beg for a portion of your space, but I am unwilling to let pass, without brief comment, certain statements advanced by Mr. W. Carruthers in his letter on Graptolites in your last number (page 187).

I do not find it necessary to enter here into any further discussion, as to the nature, or connexions, of what I consider to be the ovarian capsules of the Graptolites. I am now in the possession of a large number of specimens, proving, as I think, conclusively, that there is, in some species, an actual organic connexion, and I trust shortly to publish the results of my investigations on this point.

As to the error, whereby Mr. Carruthers inserted the name of *D. Whitfieldii* for that of *D. marcidus*, I should be inclined to think that this change does not much improve his position, as *D. marcidus* does not seem to agree with *D. tricornis* in anything except the common character of possessing three processes at the base.

Mr. Carruthers appears not to be fully acquainted with the true nature of a "radicle," as defined by Hall, or, I think, he would not assert that *D. Whitfieldii* is provided with more than one. The two lateral spines, to which he alludes, are found in *D. pristis*, and in various other species, and are simply processes from the first two cellules on each side, and not "radicles" in any sense of the term. My statement, that *D. tricornis* possesses three "mucronate" radicles, was simply made in deference to Mr. Carruthers's figure of this Graptolite, where the nature of the lateral spines cannot be made out; and, also, on the supposition that he would not have chosen a specific name expressive of a character common to several species.

Mr. Carruthers still seems to think that the cellules in *D. pristis*, of Hisinger, are mucronate. My assertion to the contrary, if wrong, is at any rate supported by all the descriptions of this species to which I am able to refer. In neither the figures nor descriptions of Salter, Hall, M'Coy, Harkness, or Geinitz, is there any mention of anything of the nature of spines to the cellules of *D. pristis*. As Mr. Carruthers has simply repeated his statement, and has not seen fit to bring forward any proofs of its accuracy, he must permit me in