neighbourhood of Manchester."[1] That this similarity may be made more apparent I here place the two series in juxta-position.

DRIFT DEPOSITS OF THE NORTH-WESTERN AND EASTERN COUNTIES.

N. W. Counties.	*Eastern Counties.*
3. Upper Boulder-clay, or Till.	3. Upper Drift (Boulder-clay).
2. Middle sand and gravel.	2. Middle Drift (sand and gravel).
1. Lower Boulder-clay, or Till.	1. Lower Drift (Boulder-clay).

I see that Mr. Maw, in the March number of the GEOLOGICAL MAGAZINE, endeavours to show that the above is not the true order in time of the Lower and Upper Boulder-clays in the Eastern Counties; but he allows that the evidence is not conclusive, as the true relations of the coast Boulder-clay (1) and the high-level Boulder-clay (3) have not been laid open to inspection; but from his own account I should conclude that the evidence is in favour of Mr. Wood's classification, as he says, "there are very many instances of the coast Boulder-clay being capped with gravel, and of the Boulder-clay of the high ground being super-imposed on a subjacent gravel bed; it must be admitted that these gravel beds correspond in height, and in many cases present the appearance of continuity, but proof of their identity seems to be wanting." I should say from the above, that if the *proof* is wanting, the *evidence* is very strong.

Any evidence which shows the sequence in the Drift deposits on the opposite sides of England to be similar is of such value, and is so great a stride towards simplifying our knowledge of the quaternary beds, that I, for one, sincerely hope Mr. Wood's classification will ultimately be established beyond the possibility of a doubt; and as regards the succession in Lancashire and Cheshire given above, more extended investigations made since my paper was written, have confirmed me in the belief that it is a real and widely-extended sequence of deposits of the Glacial period.

I am, etc.,

EDWARD HULL.

THE ORIGIN OF ESCARPMENTS.

To the Editor of the GEOLOGICAL MAGAZINE.

SIR,—May I beg the insertion of a few observations upon a letter by Mr. Mackintosh which appears in your MAGAZINE for March.

It will be remembered that Mr. Mackintosh, in the interesting articles which first invited discussion in your pages, repeatedly declared his belief in the marine origin of escarpments, and as frequently referred to "terraces," etc., thereon which were thought to support his views. Bearing this in mind, I was a little surprised to find the following admission in his letter of last month. "These longitudinal valleys and basins, which are *not open plains*, and which often occur in what *must once have been land-locked situations*, appear the more mysterious the more frequently they are contemplated." I think Mr. Mackintosh must admit that marine action would be wholly unable to erode continuous lines of cliffs in "land-locked

[1] Mem. Lit. and Phil. Soc. of Manchester, vol. ii. third series, 1863–4.

situations," or indeed along the borders of any submerged areas "not open plains."

I find no mention of ordinary marine action in this part of Mr. Mackintosh's letter, other than the supposition that "transverse gorges" may have been finished by "marine currents." Such transverse valleys it is admitted "may have been partly excavated by streams flowing down *once* continuous slopes." Rain and frost are rejected by Mr. Mackintosh as sufficient agents for the denudation of the valley systems referred to; but, he adds, "It must have been a *wholesale denudation*, and not a denudation by instalments. Large bodies of water in the shape of marine currents, or "waves of translation," caused by sudden elevations, ought not, I think, to be rejected as a cause, until their inadequacy has been clearly shown."

I think, Sir, your readers will have perused the foregoing assertion with no little surprise, remembering that Mr. Mackintosh originally commenced this discussion as an avowed advocate of Sir Charles Lyell's theory, that escarpments are old sea-cliffs, formed by *ordinary* coast action. Now, it should not be forgotten that both Sir Charles Lyell, in advocating marine action, and the "subaërialists," in advocating atmospheric-action to account for the origin of escarpments, alike appeal to *causes now in action*, the effects of which are known. This is the chief lesson which the works of Sir Charles Lyell have taught geologists, and subaërialists in urging "rain and rivers" as denuding agents are consistently following out his philosophy. But when ideas are introduced wholly unknown to modern science, such as "large bodies of water," "waves of translation," etc., the discussion is at once carried back to geological controversy, as it existed before the publication of the "Principles of Geology." I think the sentence quoted above should read thus, and I am sure Sir Charles Lyell would fully agree with my rendering—"Large bodies of water in the shape of marine currents, or 'waves of translation,' (however produced), ought not to be admitted as a cause, until their adequacy, (or, at least, their possibility,) has been clearly shown."

Upon Mr. Mackintosh's further suggestion that, in the event of such diluvial action being found untenable, "equally great bodies of moving ice" may prove satisfactory, I need say nothing. It apparently belongs to the same cataclysmic class of agencies as the former. The claims of *Ice*, in a moderate and reasonable form, have already been advocated in your pages by the Rev. O. Fisher (Nov. 1866); and if Mr. Fisher is prepared to show that Glacial action in comparatively modern (geological) times, has been universal over the globe, as we *know* escarpments to be, the idea might be worthy of consideration, as indicating a probably important agent in modifying, if not in originating, escarpments. But it is unwise to accept agencies of limited applicability to explain universally occurring phenomena, when we have agencies *everywhere* active, and which are believed, *given sufficient time*, to be equal to the work performed.

In a footnote at p. 137, Mr. Mackintosh refers to a paper by me on

East Yorkshire, which appeared in the GEOLOGICAL MAGAZINE for October, 1866. A comparison was there made between escarpments and modern sea cliffs, and I remarked "on the Yorkshire coast, we pass in the *same line of cliffs*, from Lias in the north, through all the Oolitic series in succession, to Chalk in the south. Such is never the case with an inland escarpment. This presents the same set of beds throughout the entire length." This I believe to be true of all the Secondary escarpments of England, as it is known to be true of all sea-cliffs whatever. But Mr. Mackintosh observes, "this theory is not applicable to many parts of the south-west of England and other districts, where the sea, in making cliffs, shows a tendency to follow the strike, and where many inland cliffs run obliquely to the strike. Numerous instances might be brought forward, did space permit." Now it is not enough to show that any line of cliffs has "a *tendency* to follow the strike," though this would be an interesting fact. Cliffs might even for a short distance *coincide* with the strike, as in some part of their course they probably would do; but can Mr. Mackintosh give a single instance of a *long line* of modern sea-cliffs following the strike, and being at all comparable to what we all know as escarpments? Does not a simple inspection of any geological map, show that *different* formations are intersected by the sea *along the same line of cliffs;* and that "the sea, where we now see it at work, pays no regard to dip and strike?"

Mr. Mackintosh's second objection, that, in the south-west of England, "many inland cliffs run obliquely to the strike," is more difficult to meet. We all understand what is meant by the term "a sea-cliff," but we are by no means agreed upon the definition of "an escarpment." The subaërialists, I presume, limit the term to such lines of hills as, more or less constantly, run along the strike, and whose steepest sides, the "scarp," faces the dip: in this sense I believe the term is generally used by geologists. The Chalk, Greensand, and various Oolitic "escarpments" of east and central England are of this character. If, therefore, any hills were proved to run obliquely to the strike, such would probably not be called escarpments. It is quite possible that of such hills some may be true inland sea-cliffs, in which case marine deposits will probably be found at their bases. It would be interesting to have examples of some of the numerous instances to which Mr. Mackintosh refers.

At present I think the point at issue may be simply stated thus—Can the advocates of the marine theory produce any undoubted traces of marine action, along the many hundred miles of inland cliffs we all call "escarpments," or any undoubted marine deposits, *not* Glacial drift, at their bases? Or, on the other hand, can they produce a single instance where the sea is now forming a long continuous line of cliffs having any analogy to such escarpments?

W. TOPLEY.

GEOLOGICAL SURVEY OFFICE, JERMYN ST.,
March 9th, 1867.