would be admissible. I certainly supposed that all students of Lower Palæozoic geology knew that the fossil which has been determined as *Trinucleus seticornis* by Salter, Angelin, and many others occurs abundantly in Middle Bala (Caradoc) beds. If Mr. Reed will turn to my Sedgwick Essay "On the Classification of the Cambrian and Silurian Rocks" (published in 1883), where the Upper Bala beds are generally separated from those of Middle Bala age, he will find *Trinucleus seticornis* recorded as a Middle Bala fossil in North Wales (p. 39), the Lake District (p. 57), and Scandinavia (p. 76), and nowhere recorded in the Upper Bala list. Mr. Roberts and I used the term *T. seticornis* beds locally because we thought (wrongly it appears) that under the circumstances it could mislead no one.

In the above-mentioned discussion I disputed the statement that *T. seticornis* was a characteristic Upper Bala fossil, as the form which is usually taken as *T. seticornis* occurs, as remarked above, in Middle Bala beds. I have not seen Hisinger's original specimen, and have been unable to obtain access to a copy of the later (1840) edition of "Lethæa Suecica" in which the form is figured. If Mr. Reed has examined Hisinger's specimen, and can prove that the reference by Angelin, Salter, Linnarsson, Törnquist, Tullberg, and others of the common Middle Bala form to Hisinger's species is erroneous, I will cry "Peccavi." I am quite prepared to believe that there is a characteristic *Trinucleus* in the Upper Bala beds, but doubt whether it is Hisinger's species. I should not be surprised if the variety *Bucklandi*, amongst others, be eventually proved characteristic of these beds.

There are many statements in the body of Mr. Reed's paper on the Keisley Limestone with which I regret that I am unable to agree, but I do not wish to trouble your readers with questions of detail, especially as I am given to understand that the fauna of the Keisley Limestone is about to be examined by a very competent palæontologist.

JOHN E. MARR.

CAMBRIDGE, *July* 15, 1897.

ON THE ORIGIN OF LYNCHETS.

SIR, — Mr. E. A. Walford's article, which attempts to give a natural origin of lynchets, based on geological conditions, is not very convincing, and seems indeed to refute itself. Had he seen the many perfect examples which the Chalk Downs afford, I think he would never have suggested a natural explanation for lynchets. Joining the high road from Amesbury to Salisbury and close to the Workhouse are some very typical lynchets, all in the Upper Chalk, and yet confined to only a very small portion of it; yet, if geologic conditions are the cause, they should occur in many other localities of the Chalk area. The opponents of the artificial origin must prove why they are confined to small areas when the geologic conditions, which they consider as the cause, occur over wide areas.

Taking the Amesbury examples as typical ones, they will be seen

all to start from one straight line drawn about the middle of the slope, and to end, diminishing gradually in width, on the steep side of the slope. Their ground-plan also is *always* the same, and that is the shape of the blade of a scythe, and they end against one gradual curve, viz., the steep side of the slope, and are as nearly one size as the shape of the slope will allow.

When they occur on the slopes of escarpments and run along combes, these terraces will be seen to be parallel with the sides of the combe; and yet when they occur on the spur or slope separating two combes they will be found to run almost at right angles to those running up the combe, and right across the slopes, always commencing at one straight line and having the curved termination before alluded to. This, I think, shows conclusively that *natural* causes could not result in two sets of opposing terraces occurring under precisely similar geologic conditions. These artificial terraces, too, have in many cases quite altered the natural shape of the valley or combe, turning a $\wedge$-shaped valley into a $\cap$ one, as at Heddington, near Devizes. Again, the walls of these terraces may often be found, when cut into, to be faced with stones; flints in some areas, and sarsens in others, and these walls have, from this reason, been resorted to for the sake of the facing material.

This facing seems to me to have been a gradual process, and is another proof of the artificial origin of the terraces, which go as far back as the Common Field System, as Mr. Seebohm has so well shown in his book on the "English Village Community." The holdings in these fields were divided into strips all of the same length, forty rods long and four rods wide when they were acre strips, and two rods wide when they were half-acre strips. Now a boundary, of course, was necessary to separate strip from strip, and this boundary on the flat land consisted of a strip of turf, or a "balk," or a "mere," as it was termed.

Now in the case of the hill slopes it would not be so easy to keep a well-defined boundary, owing to the wash of the soil when cultivation began. No doubt to begin with, the strips were marked by cutting into the side of the hill, and we see that they all started from one straight line. To keep the boundary, stones seem to have been used, and fresh marked each year no doubt, as the line became soiled up. In process of time these lines upon lines of stones would result in a bank faced with stones, as we now find, and in flat ledges or terraces on the hillside.

These terraces, moreover, are often found associated with pit-dwellings, earthworks, and old trackways; and I have generally found near them many pot-boilers or flints, which when heated were used for cooking purposes. Hence, a very much stronger case can be made out for the artificial than for the natural origin of those terraces, as shown by Scrope, Seebohm, and others.

F. J. Bennett.

Salisbury, *July* 8, 1897.