

cannot find any certain evidence of them. Mr. Frederick Chapman, A.L.S., favours me with the following list of Foraminifera, which he has determined on the weathered surface of the flint on one of the Somali implements:—

*Operculina*, sp. with rib-like septa, common.

*Heterostegina depressa*, D'Orbigny, a very good specimen.

*Cristellaria*, two species, frequent.

*Pulvinulina*, one specimen.

I agree with Mr. Chapman in regarding the rock as being probably of Miocene or Pliocene age. T. RUPERT JONES.

#### NOTE ON *DINOCYSTIS BARROISI*.

SIR,—Please allow me a few words in reply to the valuable paper of Mr. F. A. Bather on *Dinocystis Barroisi*.<sup>1</sup> In his paper “Sur l'étage devonien des psammites du Condroz en Condroz” (Bull. Acad. des Sci. de Belg., 1875, 2<sup>e</sup> sér., t. xxxix, pp. 658–9), Mr. M. Mourlon mentions, from Mr. Malaise's collection, an “astérie” found near Walcourt in an indeterminate “assise” of the “psammites du Condroz.” This fossil is no longer quoted in the list of the fossils of this series, given by the same author in his “Géologie de la Belgique,” but it is replaced (t. ii, p. 23) by *Agelacrinus*, very rare, in the “assises” of Montford and Évieux, the two upper assises of our Psammites du Condroz, and this is supposed by Mr. Bather to be the same as his *Dinocystis Barroisi*. Now the “astérie” of 1875 is the species found by Mr. L. Bayet, and described by me in my “Fragments paléontologiques” (Ann. Soc. géol. de Belg., 1881, t. viii, Mém., pp. 52–54, pl. iii, figs. 1 et 2), under the name of *Protaster Decheni*, and for important reasons I believe that the *Agelacrinus* of 1881 is the same species. Recently, I have learned from Mr. L. Bayet that his fossil was found in the “assise d'Évieux.” G. DEWALQUE.

LIÈGE, January 9, 1899.

#### THE SUBMERGED PLATFORM OF WESTERN EUROPE.

SIR.—In your January issue Dr. J. W. Spencer takes up the cudgels for Professor Hull on this subject, and treats your readers to a display of quarter-staff argument, by which he seems to hit me very hard, but is really cudgelling figments of his own too fervid imagination, fabrics which have far less substance than the windmills on which the renowned Don Quixote exercised his arms.

Dr. Spencer's communication may, indeed, be described as consisting in part of a discussion of points which I did not call in question and in part of denials of statements which were never made.

He says first that I denied the great subsidence of the continental margins, and a few lines lower (p. 18) that I denied their recent

<sup>1</sup> See GEOL. MAG., Dec. IV, Vol. V, December, 1898, p. 543.

elevation. By referring to my letters published in September and November last your readers can see for themselves whether these statements are true or false. Dr. Spencer must have read my remarks so hastily that he failed to understand the drift of my argument, and perhaps when he succeeds in realizing that I did not anywhere deny either the subsidence or elevation of the continental margins he will have the courtesy to acknowledge that he has done me an injustice.

He accuses me of "turning the correspondence into an academic disputation of the meaning of the word 'escarpment,'" and into a "flat denial of the evidence submitted by Professor Hull." I hope to show that I did neither of these things.

Ever since Mr. Whitaker carefully distinguished between the features presented by a steep slope fashioned entirely by subaerial agencies and those of a raised sea-margin or cliff,<sup>1</sup> English geologists have restricted the term escarpment to the former, and have thought that the primary origin of the slope was a point of some importance. Dr. Spencer tells us that by American geologists the descent from a tableland or plateau is termed an escarpment, and he implies that they do not trouble themselves to ask whether the course of the feature was determined by the sea or by subaerial agencies. Of course Dr. Spencer ought to know, and I can only express surprise at what he says is American usage.

Again, Dr. Spencer asserts that I flatly denied Professor Hull's evidence. Now most people reading such a statement would suppose that I had flatly contradicted Professor Hull on statements of fact, which is very far from being the truth. Hence I am led to ask whether Dr. Spencer has any clear conception of the difference between evidence and inference.

I could wish that Professor Hull had displayed his facts more fully by reproducing some of the actual charted records, but I did not question the accuracy of his observations, nor his inference that the submerged plateau was once a land-surface and that traces of river-valleys may be found upon it. It was not his evidence but his further and more theoretical inferences that I questioned.

What I denied, and still deny, is that Professor Hull adduced any evidence to prove that the steep declivity which forms the border of the plateau was an escarpment in the English sense of the term, in other words that it has been formed and fashioned *entirely* by subaerial agencies. Not only did Professor Hull assume that it was so formed, but he further assumed, without proof and without consideration of other possibilities, that the platform was formed first and the declivity afterwards. Further, he referred these events to definite periods of geological time. I asked him to give his reasons for these conclusions, but in his reply (October) he did not answer the two latter questions (promising, however, to discuss one of them later on), and confined himself to defending his use of the term escarpment. Is it surprising that my rejoinder was similarly

<sup>1</sup> GEOLOGICAL MAGAZINE, Vol. IV, pp. 447 and 483 (1867).

limited? and is Dr. Spencer justified in saying that I converted the correspondence into an academic disputation?

Because in my second letter I still denied that Professor Hull had given any good reason for regarding the declivity as a feature of subaerial origin, this denial is construed by Dr. Spencer into a flat denial of all Professor Hull's evidence. His desire apparently is that Professor Hull and himself should pose as martyrs for a new faith, and he hurls a defiance at any individual, council, or president of any learned society in the universe who should venture to doubt or deny his interpretation of the facts!

So long as Dr. Spencer maintains such an attitude it is impossible to argue with him. I am deeply interested in the history of this submerged portion of the European continent, and I look forward to Professor Hull's promised consideration of it, when he will doubtless answer my questions, for he is always a courteous opponent.

I have supposed that the position of the great declivity and its general course were determined by marine erosion (just as the present coastline has been), and, further, that the platform between the modern and the ancient coastlines was formed subsequently at the expense of the ancient continent. Both platform and declivity may have been afterwards modified by subaerial agencies, and this may have been the penultimate phase in their history, but this order of events is not that indicated by Professor Hull. A. J. JUKES-BROWNE.

WILLIAM COLCHESTER, J.P., F.G.S. This well-known geologist, so long connected as Chairman with Messrs. Lawes' Chemical Manure Company, of the Manganese Bronze and Brass Company, and Senior Partner in Messrs. Colchester and Ball's Chemical Manure Manufactory, who had attained to the advanced age of 85 years, died at his residence, Burwell Hall, Cambridgeshire, on the 16th November, 1898. We shall give an obituary of Mr. Colchester in our March Number.

HENRY ALLEYNE NICHOLSON, M.D., D.Sc., Ph.D., F.R.S., F.G.S., Regius Professor of Natural History, Aberdeen University, since 1882,—who had previously held the Chairs of Natural History at Toronto, Dublin, and St. Andrews; many years Swiney Lecturer in Geology in the British Museum (Natural History); the well-known author of a "Manual of Palæontology," and many other important geological publications,—died, after a short illness, at his residence, Newthorpe, Queen's Road, Aberdeen, on the night of the 19th January, at the age of 55 years. His ability as a writer and lecturer, but still more his geniality and kindness of heart, attracted to him a large and attached circle of friends, who will long mourn his loss. We hope to give a suitable notice of Professor Nicholson's life and work in the March number of the GEOLOGICAL MAGAZINE.—ED. GEOL. MAG.