

is overlain by the Llandeilo Shales. The author concludes that the andesites, andesitic ash, rhyolites, and diabase-porphyrte are of Lower Llandeilo age; and that the diabases are post-Llandeilo and pre-Llandovery.

8. "On the Relations of some of the Older Fragmental Rocks in North-west Caernarvonshire." By Prof. T. G. Bonney, D.Sc., LL.D., F.R.S., F.G.S., and Miss Catherine A. Raisin, B.Sc.

In a recent paper on "The Felsites and Conglomerates between Bethesda and Llanllyfni, North Wales," it is argued that, in the well-known sections on either side of Llyn Padarn, a great unconformity separates the rocks into two totally distinct groups.

The authors of the present communication discuss at the outset the great physical difficulties involved in this hypothesis.

They further affirm, in the course of a description of the sections, which are most clear and afford the best evidence:—

- (1) That the strike in both the supposed rock-groups is similar.
- (2) That the same is true of the dips.
- (3) That very marked identity of lithological characters may be found in rocks on either side of the alleged unconformity, specimens occasionally being practically indistinguishable.
- (4) That in no case which has been examined can any valid evidence be found in favour of the alleged unconformity, and that in the one which is supposed to be the most satisfactory proof of it the facts are wholly opposed to this notion.

CORRESPONDENCE.

ON THE ALLEGED CONVERSION OF CHLORITE INTO BIOTITE.

SIR,—The discussion between Dr. Callaway and myself, on the above subject, has reached a stage beyond which I do not think it can be profitably carried.

My great difficulty in this controversy has been to know where to have my adversary. Hunting Dr. Callaway's theories, from paper to paper over the leaves of the *GEOLOGICAL MAGAZINE*, takes my memory back to certain sporting episodes in my life connected with the Indian Gazelle. These graceful but restless little creatures are constantly on the move, and object, in a provoking way, to stand still to be shot at. I hope I may be pardoned for saying that Dr. Callaway's theories display a similar unsteadiness under fire.

I understood the author, in his first paper,¹ to be stating a case of dynamo-metamorphism, and treated the matter from that point of view. Dr. Callaway replied plaintively that "for the past four or five years, I have been insisting that, in the Malvern crystallines, biotite has been produced out of chlorite by 'contact-action';"² and on the following page he added, "I think it probable that there is not a scrap of biotite in the crystallines of the Malverns which has been produced except by 'contact-action.'"

On reading these statements, which seemed precise and definite, I tried another shot. Alas! the bullet flew wide of the mark. The

¹ *GEOL. MAG.* December, 1893, p. 535.

² *Ibid.* May, 1894, p. 217.

author writes¹: "I never said that chlorite is changed into biotite by contact-action *only*. . . . Chlorite is changed into biotite by contact action *plus* dynamic action."

I will only say in conclusion, that I do not see that the illustrations which Dr. Callaway has given in his July paper strengthen his case. If I understand him, he seems to think (to put the argument shortly) that because chlorite abounds where signs of shearing exist that the hydrous chlorite has been produced by shearing; and secondly, because he has observed cores of mica in the chlorite, that the mica has been produced out of the chlorite by contact-action. "The uniform appearance of mica," he writes, "where the shearing is great and where the granite veins are numerous, while it is nowhere seen where shearing and veins are absent, appear to demonstrate that these are the true causes of the generation of the mica."

I think all the probabilities of the case are opposed to this view. As I said in my last paper, all petrologists are ready to admit that dark mica is a very common product of the contact-action of granite intrusion in diorite; and I think that the subsequent more or less complete conversion of this contact mica into chlorite by aqueous agents that have found ready access to the rock along the lines of crushing and shearing is what one would naturally expect. That there should be cores of mica left in the secondary hydrous chlorite is in accordance with the petrologist's experience in his studies of the conversion of olivine into serpentine and of augite into hornblende. Dr. Callaway's theory involves the supposition that the production of hydrous chlorite should precede the gneiss of the mica; and that portions of the hydrous chlorite should escape unscathed from the burning fiery furnace of contact metamorphism that converted their fellows into anhydrous mica. These, and other difficulties enlarged on in my last paper, have not been met.

C. A. McMAHON.

[This correspondence is now concluded.—EDIT. GEOL. MAG.]

CATALOGUE OF THE MESOZOIC PLANTS IN THE DEPARTMENT OF GEOLOGY, BRITISH MUSEUM (NATURAL HISTORY). THE WEALDEN FLORA, PART I. 1894.—A CORRECTION.

SIR,—In adopting the generic name *Nathorstia* for a new type of Wealden fern ("Wealden Flora," p. 145), I was not aware that the late Prof. Heer had previously made use of the same genus.

My thanks are due to Prof. Nathorst of Stockholm for calling my attention to Heer's genus *Nathorstia*, which was instituted in 1880 for the reception of certain fragments of Marattiaceous ferns from the Cretaceous strata of Pattorfik, Greenland (Flor. foss. Arct. vol. vi. 1882. Nachträge zur foss. flor. Grönlands, p. 5, pls. i. and ii.). I propose, therefore, to substitute the generic name *Leckenbya* for the fern described in the Catalogue as *Nathorstia valdensis*.

CAMBRIDGE, July, 1894.

A. C. SEWARD.

¹ GEOL. MAG. July, 1894, p. 320.