

perpetually radiating away. When this is brought into the reckoning, there is no such decrease in available energy as Mr. Greenwood's argument supposes. The reason for expecting that the winter snowfall will be increased with increased eccentricity is that the heat-receipt during winter will be then diminished. Some vapour which the sun's heat might have maintained as vapour during the winter, will then radiate off its heat without compensation; will be chilled and fall as snow. But the summer's receipt of heat is increased, increased to the exact extent of the winter decrease, and so to the extent required for the dissipation of the supposed additional snow. The heat thus spent in dissipating Mr. Greenwood's extra foot of snow would before the increase of eccentricity have been spent in preventing that snow from being formed. There is no increase of work to be done.

Mr. Greenwood's argument would become correct if the snow were supposed to be generated in some different region, and thence brought to the region considered. Obviously a room will be chilled if a block of ice be introduced.

I find it difficult to reconcile the language of Mr. Greenwood's second paragraph with the article which it criticizes. He says that I argue from "increased radiation being greater in proportion to the increase of temperature." He probably means, "greater *than* in proportion." He says that I ignore the fact that if radiation is increased in greater proportion by a rise in temperature, it is decreased in like proportion by a fall. This fact is only roughly true, just as when a conical vessel contains water, it is true that whether the water level be raised or lowered an inch, the quantities to be poured in or poured out are nearly the same. But only roughly, not quite. The equality is not perfect. This is pointed out at some length in the article considered, and the argument questioned by Mr. Greenwood was built on this absence of equality. The words "ignore" and "fact" seem incorrectly applied.

The question is at present scarcely worth discussion. Mathematical calculation of the effect is envired with apparently insuperable difficulties. But the rough attempts at calculation which I have made lead me to suspect that its amount is insignificant, and not even inadequate to alter mean temperature by a degree.

ST. JOHN'S COLLEGE, CAMBRIDGE, Aug. 10.

E. HILL.

#### SCLEROTIC BONES OF COAL-MEASURE REPTILES.

SIR,—Would you kindly permit me to inform your readers that I have obtained from the Northumberland Coal-measures a perfect ossicular sclerotic ring of a Carboniferous reptile? It consists of eight ossicles of a quadrate form which slightly overlap each other and produce a perfect ring, the central opening of which is  $\frac{1}{8}$ ths of an inch in diameter; and the extreme diameter of the ring of ossicles is  $\frac{3}{4}$ ths of an inch.

I have also obtained a series of six sclerotic ossicles lying in regular order. The ring, if complete, indicates the existence of about 24 ossicles, and the central opening about  $\frac{3}{8}$ ths of an inch in diameter.

I have neither seen nor heard of any other sclerotic plates having been obtained from British Carboniferous strata, and shall be glad to know if any collector of British Coal-measure fossils has obtained specimens from any British colliery or coal strata.

26, ARCHBOLD TERRACE,  
NEWCASTLE-ON-TYNE, July 17, 1880.

T. P. BARKAS, F.G.S.

#### FOSSILS ON TRANSVERSE CLEAVAGE PLANES.

SIR,—Will you kindly accord me a little of your space to give publicity to certain observations which I have made upon the above subject?

The possibility of fossils occurring upon cleavage planes, when those planes do not happen to be coincident with the bedding, first occurred to me as a question in connexion with investigations made by me somewhat more than a year ago in the Culm-measure Limestones of Westleigh, in Devonshire. A (?) fossil seemed to occur on a (?) cleavage plane. This I showed to several competent judges, to whose opinions I should usually, and with good reason, readily yield. But in this instance opinions were conflicting. First, by some I was told that it was a fossil, and that *therefore* the plane on which it occurred was a bedding plane, not cleavage. Next, by others I was told that the plane was certainly a cleavage plane, and (*ergo*) that the supposed fossil was no fossil.

This set me considering whether there could be no *via media* in the matter. And I found, when I began to make inquiries, that better geologists than myself had observed similar phenomena, and confessed themselves to be perplexed by them.

I do not lay any great stress upon the Westleigh specimen. I confess myself to be very doubtful now about its organic origin, though at one time I held a different view. It, however, led me to make inquiries elsewhere, and through the kindness of Mr. H. B. Woodward, Mr. Kinahan, and Mr. W. Hughes of the Victoria Slate Quarries, Carrick-on-Suir, Ireland, I was furnished with specimens which were perfectly convincing as to the fact, account for it how we may.

The first specimens sent to me by Mr. Hughes from these Lower Silurian rocks showed Graptolites and Fucoids upon what he affirmed to be cleavage planes, but, not feeling quite satisfied about the matter, I wrote to him again, asking for details of the structure of the rocks, and pointing out, by means of drawings, that the cleavage which in one place was inclined at a high angle to the bedding might elsewhere, through the folding of the beds, become coincident with the bedding.

To this he replied by sending me a specimen in which both bedding and cleavage were shown. The former was shown by colour streaks, and upon the latter, inclined to the bedding about 80°, was a Fucoid impression. He says: "*In no part (of the quarry) are the bedding and the cleavage coincident.* We find the fossils occurring on the cleavage in different parts of the quarries, whatever position the latter may hold with regard to the bedding." He added also