

course indebted to Messrs. Wright and Buckman for their researches, and if necessary the names of species taken to characterise given zones must be altered in accordance with their determinations. In no department has our nomenclature yet reached perfection, and as Mr. Buckman says, we must effect changes of name as our knowledge increases, but at the same time we must agree upon general systematic principles.

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#### GLAUCOPHANE IN ANGLESEY.

SIR,—The interesting paper by Prof. Blake, "On the Occurrence of a Glaucophane-bearing Rock in Anglesey," which appears in your March issue, suggests a question of nomenclature which is likely to give us some trouble. I am very glad to have Prof. Blake's support in assigning an igneous origin to some of the Anglesey schists; but now that they are schists I should hesitate to call them "igneous." In Prof. Bonney's description (quoted by Prof. Blake) of a specimen from the Anglesey column, the constituent minerals are "probably a species of chlorite," "epidote," "quartz (?)," and "mica"; and they form "a foliated dense felted mass." According to my view, in which I understand Prof. Blake to acquiesce, this rock was once a diorite (hornblende and plagioclase). If so, the change from the eruptive rock to the schist is surely entitled to be called a metamorphosis. If we apply the term "igneous" to a crystalline schist when we can assign to it an eruptive origin, must we call it "aqueous" when we know it was once a sediment? And under what head must we class it when its genesis is unknown to us? I grant that in tracing a diorite or a granite into a schist, we cannot fix a hard boundary-line between the two; but a similar difficulty meets us in the study of metamorphosed sediments, and it is not found to be very serious. However, I write rather to raise a question than to settle it. If we are not to call crystalline schists by the term "metamorphic," how shall we designate them? They would be as sweet to me by any other name.

WELLINGTON, SALOP.

CH. CALLAWAY.

#### THE ATMOSPHERE OF THE COAL-PERIOD.

SIR,—In the review of the 2nd Vol. of my treatise on Geology which appeared in the last number of your MAGAZINE, your reviewer remarks (p. 161), "The author considers that, during the Coal-period, the atmosphere was more dense, and more charged with moisture and carbonic acid, and he is led to conclude that the coal-growth was in all probability one of extreme rapidity, and consisted of woods and plants containing a much larger proportion of carbon than any existing forest vegetation." With regard to the excess of carbonic acid gas, Mr. Carruthers has expressed an adverse opinion, and experiments made on living plants have shown that they are liable to be poisoned, like animals, by an excess of the gas." A footnote to this passage refers to GEOL. MAG. 1869, p. 300, and 1871, p. 497. The first is a

paper "On the Forests of the Coal Period," in which he remarks that the plants "grew in extensive level plains. . . . The moist atmosphere (*not at all likely to have been charged with more carbonic acid gas than that of our own day*)<sup>1</sup> would encourage the growth of cellular parasites, etc." The second reference is to a paper by Dr. H. Woodward, "On Old Land Surfaces," in which, after quoting some remarks by Dr. Sterry Hunt to the effect that the atmosphere of the Coal period contained, as originally suggested by Brongniart, a "comparatively large amount of carbonic acid," he adds in a footnote, "Later experiments have, however, proved that plants, like animals, are at once poisoned by an excess of carbonic acid." Now the first reference appears to me only the expression of an opinion, and in the second, although experiments are mentioned, the reference is not given. I know of no such experiments, and if your reviewer or any of your correspondents can refer me to any, I shall feel very much obliged. The only experiments bearing on the subject, and which show that plants can live, flourish, and grow rapidly in an atmosphere with an excess of carbonic acid, I have quoted (p. 120), and I know of no others. Excuse the length of this letter, but I am anxious for information on this point, and should be glad of confirmation or otherwise on this subject, which is one of much theoretical interest.

DARENT-HULME, SHOREHAM, SEVENOAKS,  
10th April, 1888.

JOSEPH PRESTWICH.

#### SPURIOUS FLINT IMPLEMENTS.

SIR,—Will you kindly allow me space in the *GEOL. MAG.* to inform its readers who may be collectors of Flint Implements, that there are at the present time being manufactured in London worked flints, which are stated to be genuine, but which are nothing of the sort, and at the same time to say that some of these manufactured flints have been sold to gentlemen for a high price, who are considered authorities on the subject, and I trust that should any of my readers meet with such as appear doubtful they will use their best endeavours to expose and stop such a fraud.

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#### ALPINE RIVERS AND BUNTER PEBBLES.

SIR,—Prof. Bonney's paper on the "Rounding of Alpine Pebbles" is a valuable contribution to a chapter of physical geology; but there are one or two considerations to which I do not think he has given sufficient recognition. (1.) Weathering of *débris* on the mountain-sides, which often gives a certain initial rotundity to fragments of rocks. (2.) The scouring action of sand in a mountain river. So far as I can recall my own Alpine observations, I am inclined to think that where the coarser detritus is most completely rounded, so that the pebbly form is generally produced, it has been in cases where a large proportion of sandy detritus was present also. On the other hand, I have generally found that at the mouths of

<sup>1</sup> The Italics are mine.