

that the caverns had been completely filled with these materials, and in the case of the Cae-Gwyn cave they appeared to have been conveyed mainly through the entrance recently discovered under the drift. The stratification at this entrance was so marked, and could be traced so continuously inwards over the bone-earth, that there could be no doubt that this was the main entrance. There was not the slightest evidence that any portion of the material had been conveyed in through a swallow-hole, and the conditions witnessed throughout were such as to preclude any such idea.

The author quoted a Report by Dr. Geikie, who considered that the wall of the cavern had given way, but before the deposition of the glacial deposits, which were subsequently laid down against the limestone bank so as to conceal this entrance to the cavern.

In conclusion, he referred to the presence of Reindeer remains in these caves, in conjunction with those of the so-called older Pleistocene mammalia, proving that these had reached the area long before the period of submergence, and evidently at an early stage in the Glacial period. It was important to remember that Reindeer remains had been found in the oldest river-gravels in which implements had been discovered. Man, as proved by the implements discovered, was also present at the same time with the Reindeer, and it was therefore natural to suppose that he migrated into this area in company with that animal from some northern source, though this did not preclude the idea that he might also have reached this country from some eastern or southern source, perhaps even at an earlier period.

Mr. De Rance, in an Appendix, confirmed Dr. Hicks's observations as to the identity of the deposits outside the cavern with those in its interior, and noted the occurrence of limestone blocks in the lower deposits, not merely at the spot where the supposed broken wall was situated, but also throughout the whole tunnel. He stated that the sand-bed forming the uppermost cave-deposit resembled the sand associated with gravels in a pit 400 yards east of the cave at a slightly higher level. The drift exposed in this gravel-pit he believed to be of the same age as that of the Mostyn and Bagillt pits to the north, which were undoubtedly overlain by Upper Boulder-clay. The westerly termination of the bone-earth outside the cave had not been determined, which he regretted; but traces of bone had been found at a point five feet from the overhanging ridge of the cave.

CORRESPONDENCE.

PALÆONTOLOGICAL NOMENCLATURE.

SIR,—The questions raised by the gentleman signing himself Rob. W. Haddow in the *GEOL. MAG.* for November, 1887, and discussed by Mr. S. S. Buckman in the March number, are well worthy of further consideration in your pages.

I confess that I largely agree with Mr. Haddow in his protest against the entire suppression of the old genus *Ammonites*, and I would reply to Mr. Buckman, (1) that the genera of one family

should differ from one another in characters of equivalent value, and (2) that it is *not necessarily wrong* "to include in the same genus species descended for a long time through entirely different lines of ancestors." There is in fact very little wrong or right in the matter, it is one of convenience and of sensible proportional treatment.

We may admit that the whole family Ammonitidæ requires revision and reconstruction, and possibly that it is desirable to create a certain number of new genera out of the old genus *Ammonites*, but I join Mr. Haddow in protesting against the infinite subdivision which some palæontologists are trying to force upon us. The old principles of classification may not be defensible, but is it so very certain that some of the principles now adopted in their stead, such as the form of the mouth, are any better? Is there not some analogy between the case of the genus *Ammonites* and that of the genus *Helix*, in which an infinity of peculiar variations occur in the shells without any important differences occurring in the structure of the animals?

If mere sections and subgeneric groups are raised to the rank of genera, the old genera become tribes and subtribes, and Mr. Buckman even wants us to accept names for generic and subgeneric groups, ranking between genera and subtribes. Surely, Sir, such an arrangement as he gives us in his Monograph on Inferior Oolite Ammonites is the height of cumbrousness, and shows the absurdity to which the system is capable of being carried. Stated in full this arrangement is as follows:—

Family—Ammonitidæ.

Subfamily—Ammonites (note the termination).

Tribe—Egoceratidæ.

Subtribe—Harpoceratinæ.

Generic group—Hammatoceratidæ.

Generic subgroup—Hildoceratinæ.

Genus—Ludwigia.

Species—Murchisonæ.

Really I think a trinomial or even a quadrinomial system is better than this, which is practically a septinomial one. The small section of a group which is here elevated into a genus hardly merits a name at all, it is a mere section of *Harpoceras* which may be regarded as a subgenus of *Ammonites*. I therefore take up Mr. Buckman's challenge, and would speak of the species trinomially thus—

Ammonites (Harpoceras) Murchisonæ,

var. *obtusa.*

By this method it would still be possible for the stratigraphical geologist to speak of it as *Ammonites Murchisonæ*, while the palæontologist who makes a special study of the genus would doubtless usually call it *Harpoceras*; but no other Ammonite could receive the same specific name, whereas, if *Harpoceras* be admitted as a generic name, new species referable to that genus might receive the same names as those now applied to other well-known species of Ammonites; thus we might have *Harpoceras cordatus*, *H. cristatus*, etc.

As regards the rectification of erroneous identifications, we are of

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.

A. J. JUKES-BROWNE.

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GLAUCOPHANE IN ANGLESEY.

SIR,—The interesting paper by Prof. Blake, "On the Occurrence of a Glauconite-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.

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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