

CORRESPONDENCE.

CALAIÏS NEWBOLDI.

SIR,—Since the publication of my paper “On a Fossil Octopus (*Calaiïs Newboldi*, J. de C. Sby. MS.), from the Cretaceous of the Lebanon,” in the Quart. Journ. Geol. Soc., vol. lii, p. 229, 1896, I find that the name *Calaiïs* has been twice used—first by F. L. de Laporte in 1836 (Silbermann, *Revue Entomologique*, vol. iv, p. 9) for a genus of COLEOPTERA; secondly by J. A. Boisduval, also in 1836 (“J. A. Boisduval, Species Général des LEPIDOPTÈRES,” vol. i, p. 584; quoted as a synonym of *Idmais*, Boisd.). Under these circumstances it becomes needful to propose a new generic name for Sowerby’s *Calaiïs*; I would therefore suggest that the name of *Palæoctopus* replace that of *Calaiïs*, which is preoccupied by a genus of COLEOPTERA.

HENRY WOODWARD.

LIFE-ZONES IN CARBONIFEROUS ROCKS: A CORRECTION.

SIR,—I observe that in your last number (p. 519), the British Association Report on Life-zones in the British Carboniferous Rocks is stated to have been drawn up by me, whereas it was the work of the Secretary, Mr. E. J. Garwood.

JOHN E. MARR.

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LINNÆUS ON THE APPENDAGES OF TRILOBITES.

SIR,—In the May number of the *American Geologist*, Dr. C. E. Beecher has published an article entitled “On a Supposed Discovery of the Antennæ of Trilobites by Linnæus in 1759.” As this article refers to my short communication to the GEOLOGICAL MAGAZINE, p. 142, March, 1896, I shall be glad if you will afford some space for a few remarks in reply to the interpretation of Linné’s figure proposed by Beecher.

To begin with, I regret that, in the introductory words, I have used so inaccurate an expression as to suggest that Linné’s paper had been overlooked ever since its first appearance, while I have only taken into consideration the period which I have spoken of as a time of important researches into the ventral structure of Trilobites.

Beecher sums up his reasoning concerning the organs, regarded by Linné as antennæ, in the following terms: “It necessarily follows that the cephalon of the specimen figured by Linnæus is without free cheeks, and with this interpretation of the figure, the supposed antennæ can only be homologized with the thickened border between the points where the facial suture cuts the anterior margin.” This conclusion is drawn under the supposition that the disputed organs are not antennæ, but no conclusive evidences are adduced that they are not. Before entering upon an examination of Beecher’s arguments, I think it suitable to give a short review of the modes in which the head of *Parabolina spinulosa*, Whalb., is

usually found preserved. In the zone which is characterized by this fossil, it occurs in the greatest profusion; but owing to the friability of the matrix, it is often difficult to protect the specimens from destruction when kept in collections. Entire heads are not rare, but they more commonly display only the central shield between the facial sutures; other specimens present the same part, but with the portion situated before the glabella and the ocular ridges broken off. That the free cheeks are wanting in the specimen figured by Linné, I have so much the less reason to doubt, as I have in my own collection heads of *Parabolina* quite agreeing with the Linnean figure, but for the parts there visible before the glabella. The free cheeks are indubitably absent for the greater part, but a small portion of them behind the eyes seems to be coherent with the fixed cheeks, since the outer margin of the whole presents an uninterrupted curvature, just like that seen in Linné's delineation. I cannot explain the cause of this shape; it may, perhaps, be due to the form of the inflected portion of the free cheeks. However, a further examination of Linné's drawing does not confirm Beecher's suggestion as to the nature of the parts which Linné called antennæ. If these were to be interpreted as the thickened border between the facial sutures, they ought not to have been pointed towards the end, nor to have been so long as they are. These circumstances might, however, be considered as depending upon carelessness of the draughtsman; and I should scarcely have mentioned them, if they had not been combined with another detail, which cannot be accounted for as due to such carelessness. Before the frontal lobe of the glabella there is a smaller rounded lobe projecting between the pieces in question, which is not only a little larger, but also more distinct in the original figure than in Beecher's reproduction. The form of this lobe seems to me to preclude the idea that we have before us the thickened frontal margin. But if we imagine the foremost part of the central shield before the glabella to be broken off, as is often the case, there is another part which has just that position, quite the same shape, and the same size as that lobe, viz. the anterior part of the hypostome, or, more strictly speaking, of its central portion. This organ is very often met with amongst the specimens imbedded in the slates. From beneath this lobe the antennæ appear springing forth, and their bending can easily be imagined continued beneath the hypostome, to their points of attachment at the sides of the same.

Though I think this interpretation to be more in harmony with the Linnean figure, I admit that the question is not so clear as could be desired. But I believe I am fully justified in having directed the attention of scientists to this early mention of antennæ in Trilobites, each palæontologist being, of course, entitled to attach just as much importance to it as his conviction demands.

By several expressions in Beecher's paper, I feel called upon to repeat from my earlier communication that "this reference to an old"—and isolated—"observation, can by no means abate anything

of the value of the brilliant discoveries of our days” made by the eminent palæontologists cited by Beecher, or by that distinguished investigator himself.

SV. LEONK. TÖRNQUIST.

LUND, September, 1896.

ANGLESEY AGGLOMERATES.

SIR,—In his short paper in your current (November)¹ number, Sir A. Geikie is very ready to give up his opinion as to the agglomeratic character of certain fragmental rocks in Anglesey, but I hope my own opinion was based on too solid a foundation to be so easily overthrown. After reading this retraction, I turned to Sir A. Geikie’s and my own original description of these agglomerates, quoted below, and it appeared to me at once that if the phenomena in the Isle of Man were the same as in Anglesey, the rocks in the former locality could not be “crush-conglomerates.” I therefore turned again to the description of these “crush-conglomerates” as given by Mr. Watts, and this is what he says:—“The fragments exhibit a great uniformity in composition, and nothing has hitherto been found in them but grits and slates,” which “could all be matched either in the transition series or else in the main grits and slates” (between which the crush has taken place). “Although Mr. Lamplugh was alive to the importance of looking out for the existence of fragments of igneous rocks and other strangers, and collected a number of specimens to be tested with this point in view, not a single fragment of any other rock has up to the present been detected.”²

We cannot doubt that Sir A. Geikie is equally alive to the importance of this feature, and, indeed, his new descriptions of the rocks in Anglesey indicate as much, but I think in his enthusiasm he must have forgotten his older, fuller, and, I think, more accurate account of them. This is what he first said about the rocks at Llangefni: “The agglomerates . . . contain abundant blocks of reddish quartzite, pieces of various felsites and of finely amygdaloidal andesites.”³ My own statement is practically identical: “They contain huge masses of quartzite and igneous rocks.”⁴ These are certainly not descriptions of the rocks of the neighbourhood between which the crushing could have taken place. Sir A. Geikie *now* writes: “The strata affected appear to have been originally shales or mudstones (with possibly some fine felsitic tuffs), alternating with bands of hard siliceous grit.”⁵ These two descriptions are very different. Can Sir A. Geikie reconcile them?

Of the rocks near Cemmaes he originally wrote (of the vent on Mynydd Wylfa): “It is filled with a coarse agglomerate, among the large blocks in which fragments of quartzite, limestone, felsite, grit, and shale may be noticed” (five varieties of rock); and the vent on the west side of Cemmaes harbour “appears to have been drilled

¹ GEOL. MAG., Dec. IV, Vol. III, p. 481.

² Q.J.G.S., vol. li, p. 591.

³ *Ibid.*, vol. xlvii, p. 130.

⁴ *Ibid.*, vol. xlv, p. 487.

⁵ GEOL. MAG., Dec. IV, Vol. III, p. 481.