

## CORRESPONDENCE.

"STEM-OSSICLES" OF CRINOIDEA, IN THE LEPTÆNA-KALK  
(UPPER ORDOVICIAN) DALECARLIA, SWEDEN.

SIR,—A reviewer (F.A.B.) of my notice "Om förekomsten af en *Caryocrinus* i Sverige" has said in the GEOLOGICAL MAGAZINE, December, 1890, p. 570: "The rock is filled with stem-ossicles which Dr. Holm, after the curious custom of collectors, thinks it necessary to ascribe to Crinoidea." What I regard as stems of at least two species of Crinoids, the reviewer says are stems of Cystidea without his having seen a single one of them. But the real fact is that these stems in consequence of their structure must have belonged to Crinoids. One of the supposed species has left fragments of stems 45 mm. in length by 16 mm. in breadth. These show the starting-points where strong cirrhi originated, and as far as known cirrhi have not been found in the Cystidea. Thick and expanded rootlets of Crinoids are also not rare. To this must be added, that all known Cystidea from this same locality, with the exception of *Caryocrinus* *cf. ornatus*, Say, are devoid of a stem. According to Angelin the genera *Sphæronis*, *Encystis* and *Caryocystis* are sessile, having the basis of the perisome itself directly affixed to foreign bodies without the mediation of a stem, and several of the specimens observed by me of *Sphæronis oblonga*, Ang., and *Encystis* *sp.*, either show the surface by which they were attached, or are even yet affixed to Bryozoans or other marine Silurian fossils. Of *Caryocystis* I have seen only a single specimen, and this does not show the basis of the perisome clearly. A species of *Encystis* is numerically by far the most predominant form. This is the cause why I have, with full intention, referred the stems to Crinoids.

STOCKHOLM,  
15th January, 1891.

GERHARD HOLM.

## THE DENUDATION AND ELEVATION OF THE WEALD.

SIR,—In the December Number of the GEOLOGICAL MAGAZINE, Dr. Irving states that in my sketch in the September Number I reproduced partly some arguments which he put before the Geological Society in June (May 21st, 1890?), and published in the September Number of the GEOLOGICAL MAGAZINE. I applied to Dr. Irving for particulars, and he informed me "that the argument from the raised beaches and the reference to the crag at Lenham were the points referred to."

Now, Sir, as to the argument from the raised beaches, mine is quite a different argument from that used by Dr. Irving. I use it in the course of a criticism on Sir A. Ramsay's diagram (Geol. and Geog. of Gt. Brit. 1878, p. 343, fig. 73) as a local and recent example of elevation turning a horizontal beach-line into a curve. Dr. Irving omits the fall to the west, and uses it to prove western elevation of the Wealden area.

As to the Lenham beds, I have the right to refer to them without obtaining leave.

I do not lay claim to any great originality for my little sketch, but, in fact, I did not derive my ideas from Dr. Irving's paper.

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HORACE W. MONCKTON.

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REPLY TO MR. A. SOMERVAIL.

SIR,—I owe an apology to Mr. Somervail for plucking a leaf from his coronet of laurels. It is the simple truth that the paper which he cites had not in any way impressed itself on my mind, and thus (as the index for the last volume was not then published) escaped recollection. While making this atonement, I will take the opportunity of explaining to him why I use that plainness of speech to which he evidently objects. If he is right in his principal hypothesis about the rocks of the Lizard, I am so hopelessly wrong that I must begin my petrological studies *de novo*. The one or the other of us, so to say, is ignorant of the very grammar of the language. Now, as it happens, I have given, for nearly twenty years, more attention to petrology than to any other branch of geology; twice or thrice every year I have visited districts which were known to be instructive, making often long journeys in order to study some critical question. I have examined many of the most interesting localities on the Continent of Europe, a few also in Canada. I have formed a very large collection of rock specimens and microscopic slides, to the study of which I have devoted such leisure as I can command. Now in Mr. Somervail's writings no evidence appears of either wide experience or knowledge of the microscope, both of which are necessary for theorizing on difficult problems in petrology; indeed, of the latter, not so long since, he admitted his ignorance. Of course I know that many of these problems are yet unsolved; I make no claim to infallibility; I am well aware that notwithstanding all my pains I have not escaped the fate of workers in a progressive science, and have to modify or even abandon conclusions which at one time seemed most accordant with facts, but some of Mr. Somervail's hypotheses appear to me irreconcilable with facts and inductions which, not only I, but also petrologists of greater repute, accept almost as axioms. To me he appears to occupy the position in which I should have placed myself had I signalized my entrance in the "fifth form" at school by publishing "adversaria" on a trilogy of Æschylus.

T. G. BONNEY.

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DYNAMO-METAMORPHISM.

SIR,—M. Spring's valuable experiments have had a very stimulating effect on many minds; so much so that his experiments are sometimes quoted in proof of positions very much in advance of those taken by M. Spring himself. Thus Mr. Harker in his letter on the subject of dynamo-metamorphism in your last issue, after remarking that "the practical verification" of "the *direct* correlation of mechanical and chemical energy" "rests on such experiments