

CORRESPONDENCE.

CRYSTALLINE SCHISTS OF THE LEPONTINE ALPS.

SIR,—Permit me to express my sincere regret to Dr. Stapff for having abbreviated not only his name but also—what is worse—his life. How the second misconception arose I cannot tell, but it is certainly not a recent one. Perhaps I ought also to apologize for not referring to his papers more frequently, but the truth is that I have only seen one of them, and that (for reasons on which it is needless to enter) I had but little opportunity of consulting. For this neglect some of my fellow-workers will probably visit me with censure. Be it so, I can only say that I do not always find myself quoted “over the water,” and in this matter take as my maxim: *hanc veniam petimusque damusque vicissim*.

Except for this, my only purpose in writing, is to excuse myself from discussing at present Dr. Stapff's friendly and interesting communication. I am still at work on the subject of that singular complex of rocks in and about the Urserenthal, and cannot publish anything more till I have tested certain hypotheses on the ground. This I fear cannot be done during the present summer, since I anticipate that my steps must be turned in another direction, and I am not one of those fortunate persons who can undertake a long journey at pleasure in order to investigate a geological problem.

So I ask permission only to observe:—

(1.) That I do not deny the possibility of Jurassic rocks or Carboniferous rocks entering into the complex of the Urserenthal. But I doubt the occurrence of organisms in the Altkirche marble. Without seeing the slides, it would be difficult to express an opinion on the nature of the objects figured by Dr. Stapff on page 18 of this volume; the upper one certainly has an organic aspect; the lower strikes me as more doubtful. But the nature of the objects is not the only thing to be considered.

(2.) That, if I am right in understanding Dr. Stapff to assign the Piora schists to the Carboniferous system, this identification appears to me only an hypothesis. If there be any valid evidence in favour of it, this is unknown to me, while I am aware of some serious difficulties in which we should be landed by accepting it.

(3.) That, from what I know of crystalline rocks and their ways, I venture to doubt the accuracy of the identification (p. 17) of “rolled quartz grains (*sand*) in some beds of the Guspis micaceous gneiss.” For years I hunted for traces of an original elastic structure in gneisses and certain associated crystalline schists, longing to find them, but in vain. Again and again I have seen them curiously simulated here and there, by the results of pressure, and so, having been often taken in for a while, I have become rather sceptical.

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CONCHOLOGICAL NOMENCLATURE.

SIR,—Mr. A. J. Jukes-Browne, in the January Number of the GEOLOGICAL MAGAZINE, takes objection to some points in Conchological Nomenclature adopted in the “Systematic List of the F. E.

Edwards Collection of British Oligocene and Eocene Mollusca," to which I beg to offer the following remarks.

Mr. Jukes-Browne calls attention to the proposed disuse of *Cytherea* and *Triton*; two generic names which the reviewer discussed when noticing my book in "Nature" of October 29th, 1891. In a subsequent issue of the same Journal (November 12th, 1891), Baron Osten Sacken advocated the retention of *Cytherea* because the earlier Dipteroid genus of the same name being a synonym, and therefore rendered obsolete, could, from his point of view, be retained for another group.

Evidently these writers have not consulted the literature dealing with the Molluscan genera under discussion, or they would have ascertained that Lamarck's *Cytherea* had been replaced by his earlier *Meretrix* by many competent authorities such as Dr. J. E. Gray in 1847 (Proc. Zool. Soc. p. 183), Deshayes in 1853 (Cat. Conchifera British Museum, p. 34), H. and A. Adams in 1857 (Genera, p. 423), and other specialists, including Dr. Paul Fischer, who, in the latest and most elaborate treatise (Manuel, 1887, p. 1079) on the Mollusca, fully adopts it.

Concerning the name of *Triton*, we find that it has been used for three separate organisms: by Linnæus for a Cirripede in 1767; for an Amphibian by Laurenti in 1768; and for a Mollusk by De Montfort in 1810.

Writers on the Reptilia have ceased to regard it as one of their genera, because the Linnæan name has priority, and they have substituted *Molge* for it, a genus founded by Merrem in 1820. On the same grounds Malacologists also refuse to acknowledge it (as exemplified by the works of H. and A. Adams, Philippi, Weinkauff, Stoliczka, Zittel, Dall, etc.). Link's *Tritonium* of 1807 being the name now generally known for this shell, but as this differs from Müller's *Tritonium* of 1776, I have utilized the next most appropriate synonym, and brought into prominence Schumacher's *Lampusia* of 1817.

I hope this explanation will serve to show Mr. Jukes-Browne and others interested in this subject that the rejection of *Cytherea* and *Triton* as generic names in Zoology, being brought about through the operation of the law of priority, is now almost universally acknowledged.

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READE'S THEORY OF MOUNTAIN BUILDING.

SIR,—I read Mr. Jukes-Browne's criticisms of some points in my "Origin of Mountain Ranges"¹ with interest, and until I came to the Postscript, which, like a lady's letter, contains the most important part of the communication, contemplated replying to them. This last paragraph however being destructive of the need of the preceding criticisms puts another complexion on the matter.

Mr. Jukes-Browne must be aware that I have replied to Mr. Davison's arguments against the "expansion theory of Mountain

¹ GEOL. MAG. Jan. 1892, p. 24.