

CORRESPONDENCE.

ON THE SUPPOSED PECTORAL LIMB IN *COCCOSTEUS DECIPIENS*.

SIR,—Permit me a few words in reply to Prof. v. Koenen's most courteous remonstrance concerning the supposed pectoral limb in *Coccosteus*.

Although I did indeed suggest that Prof. v. Koenen may have mistaken the outer margin of the interlateral plate in his *C. Bickensis* for a pectoral spine, I did so without dogmatism; and when I have the opportunity of examining the German specimens, I shall do so with a mind perfectly open to conviction.

But I stand firm as regards the position which I have taken up as regards the absence of any such "Ruderorgan" in *Coccosteus decipiens*, the type of the genus; and I do not think that the argument upon which Prof. v. Koenen bases his expectations of its ultimate discovery in this species, carries any weight whatever. When we take into account the position in the head of the sclerotic ring, its delicacy, and the manner in which the Scotch specimens are crushed, it is by no means astonishing that this structure should be so rarely observable in *Coccosteus decipiens*. Far otherwise would be the case with a pectoral limb, were such a thing present,—for it is simply incredible that a *long stout prominent external* appendage, like the "Ruderorgan" in Prof. v. Koenen's restored figure, should have escaped preservation in the hundreds and hundreds of specimens of Scotch *Coccosteus*, which are to be found in the museums of this country, many of which are absolutely entire from the tip of the snout to the point of the tail.

I cannot therefore share Prof. v. Koenen's expectations as to the future discovery of a pectoral limb in *Coccosteus decipiens*, and consequently must still maintain that if such a limb is really present in *C. Bickensis*, v. Koenen, that species must be removed to a new genus.

7th April, 1890.

R. H. TRAQUAIR.

MR. MELLARD READE ON THE PHYSIOGRAPHY OF THE LOWER TRIAS.

SIR,—So kindly is the tone of Mr. Mellard Reade's reply to my criticisms on his explanation of the Physiography of the Lower Trias that it is not without regret that I am compelled to observe that in my opinion he has failed to meet them. His reply, in short, as it seems to me, errs in excess and in defect. In excess, for these reasons:

(1) I do not "misconceive the facts in speaking of the Bunter generally as a 'conglomerate.'" Mr. Mellard Reade has misunderstood my words by isolating my last paper from all that I have previously written. I have touched upon the anomaly of the Lancashire Bunter (of what I know something) twice at least (GEOL. MAG. Dec. II. Vol. X. p. 204: Address to Sect. C, British Association, Birmingham, 1886). I did not again mention it, because I had nothing to add to my previous remarks. In reading the proof the

idea of inserting a protecting clause did indeed occur to my mind, but I abstained from so doing, because I supposed that I should be credited with the possession of what is common knowledge. Readers get wearied if, in writing geological papers, we imitate the style of legal documents. I dwelt upon the thickness of the Staffordshire pebble beds (which I understated rather than overstated), because the strength of a chain is the strength of its weakest link, and I cannot explain, for reasons already given, these conglomerates, as they occur over a considerable area of the Midlands, by Mr. Mellard Reade's hypothesis. The comparative absence of pebbles in the northern region is undoubtedly an anomaly for which we have not yet found the explanation (I could offer one, but, as it would be an hypothesis, I abstain on the present occasion, lest I should trespass too much on the Editor's tolerance). But on the hypothesis of a southern derivation, the much greater thickness which the Bunter group as a whole attains in the district about the Mersey compared with that in Staffordshire (more than double) is also an anomaly. To this I believe we might add—though here, as my personal observations are not very numerous, I must speak with caution—the greater abundance of felspar fragments in the sandstones of the Lancashire-Cheshire Bunter. So in this matter, as it appears to me, our difficulties are mutually destructive, like Kilkenny cats, and they may leave us much as they found us.

But I cannot understand how the nature of the sand in the Bunter helps Mr. Mellard Reade. "An inspection of the geological map of Scotland shows such a diversity of rock structure, and there exist such lithological differences in the various areas that would have drained into these two hypothetical rivers, as to seem irreconcilable with the required travel of sand southwards." This inspection, as it seems to me, shows that the area chiefly drained would be the great crystalline region—then doubtless more Alpine in character than now, the fragments of which are called the Scotch Highlands. Mr. Mellard Reade forgets that the detrital beds of this region (which were doubtless also undergoing denudation at this epoch) present no small resemblance to the Bunter Beds of England. Parallels to this argument may be found in the sandstones of the Carboniferous system in England, and in not a few cases in other lands.

(2) Mr. Mellard Reade falls into a second, though perhaps more natural, misconception in regard to my views as to the efficacy of tidal currents. My doubts as to their potency referred to their action *under the physical conditions of the English Trias*; that is, in an elongated gulf (adopting for a moment his hypothesis), to which, moreover, in all probability, the entrance was narrow and shallow. To discuss the whole question would extend this letter too much, but I must remark that citations concerning the action of the tide off the British Isles, where the physical conditions are very dissimilar, do not appear to me germane to the subject.

Next, as to the defect. Mr. Mellard Reade refrains from noticing my comparison of the Bunter of England to the Nagelfluë and

Molasse of Switzerland, because “he has no personal observations to record” on them. But is not this in effect admitting that he is making wide generalizations on a rather limited experience, or, in other words, falling into an error too common among British geologists? But still more serious a defect is his silence as to my main argument, which briefly stated is this: “I think I have a fairly good knowledge of British rocks; I can identify the majority of the Bunter pebbles (not of one rock species only) with rocks which occur *in situ* in the Highlands and as pebbles in later Palæozoic beds in Scotland down at least as far as Arran, but I have as yet failed to find them, either *in situ* or in older conglomerates in the southern half of England, or to discover a spot in which we may assume them to be hidden from our sight.”

T. G. BONNEY.

OBITUARY.

FRIEDRICH AUGUST VON QUENSTEDT.

BORN 9TH JULY, 1809; DIED 21ST DECEMBER, 1889.

By the death of Prof. Quenstedt, Science has to mourn the loss of the Nestor of German geologists. He was born at Eisleben in Saxony, and after the death of his father, a member of the Gendarmerie of that town, he was adopted by his maternal uncle, a schoolmaster at Meisdorf; here he learnt Latin and music, and by the latter accomplishment managed to earn sufficient money to go to a University. He went to Berlin in 1830, and having overcome his uncle's wish that he should devote himself to theology, Quenstedt threw himself into the study of natural science and philosophy; he worked especially at crystallography and mineralogy under Wiess and Mitscherlich. After the conclusion of his University course, Quenstedt was appointed an Assistant in the Berlin Museum; his two principal papers published at this time were “Ueber Afterskrystalle des Serpentin” and “Die Entwicklung und Berechtigung des Datholiths.” In 1837 he was appointed Extra Professor at Tübingen, and in 1842 he was promoted to the full Chair of Geology, Mineralogy, and Palæontology. Here he laboured for more than fifty years, investigating the palæontology and geology of Würtemberg, building up the collection of the University, and popularizing the study of geology in the neighbouring district. That the last object was not the least in Quenstedt's ambition is illustrated by the fact that the first work he published in his new home was a small popular volume, “Schwaben, wie es war und ist.” Immediately after his appointment at Tübingen, Quenstedt began the work on the Suabian Jurassics, with which his name will always be associated. His “Flözgebirge Württembergs” (1843) was the first fruit of his labours in this field. In order to compare this series with that of other areas, Quenstedt made a number of walking tours in France, North Italy, Savoy, etc. A serious illness of the lungs in 1859, due to over-exposure, compelled him to abandon these annual excursions; he had however already acquired the knowledge he sought, and his “Der Jura” had appeared in the previous year.