CORRESPONDENCE

ON PERMIAN AND TRIASSIC TETRAPODS

SIR,—In the March–April number of the *Geological Magazine*, 1942, there is a most interesting paper " On Permian and Triassic Tetrapods " by Professor D. M. S. Watson, in which he reviews our present knowledge. With what we know of the Lower Permian Tetrapods of America, those of the Middle and Upper Permian and Lower Triassic of Russia, and those of the Upper Permian and Triassic of South Africa we can now claim that the land vertebrates of these periods of the world's history are better known than those of any period till we come to the Oligocene. Efremov is greatly expanding our knowledge of the Russian forms ; and most promising work is being done in China by C. C. Young, where some of the fossil forms found are remarkably like those of South Africa.

On only one or two points do I wish to comment. I am delighted to see that Watson now definitely transfers " *Seymouria* " to the Amphibia. In 1918 Watson published his important paper " On *Seymouria*, the most Primitive known Reptile ". Broili and Williston and Case had also before this expressed the opinion that it is a reptile ; and since 1918 Gregory, Romer, White, and Steen have been unanimous in regarding it as a reptile.

In 1922 I expressed a contrary opinion, stating that : " The study of Watson's paper leads me rather to the conclusion that *Conodectes* is a highly evolved and specialized Embolomerous Amphibian with a very few reptile-like characters which have been acquired by a parallel development." As I was not dealing with *Conodectes* I did not give my reasons for this decision.

In 1925 Sushkin, from his study of the ear region in the Russian Seymouriamorphs, also came to the conclusion that *Conodectes* must be placed among the Amphibia, and that it is not a reptile ; and in his recent edition of Zittel, Smith Woodward also places it in the Amphibia.

The discovery of a lateral line seems now to remove all doubt, and Watson's conclusion is now the same as mine in 1922, that " We have in fact in the Seymouriamorpha a group, derived from embolomerous Labyrinthodonts, which developed many reptilian features in its skeleton, whilst retaining an amphibian life history ". " It [*Seymouria*] was by definition an amphibian."

But another point remains. Why call the beast *Seymouria* ? Cope in 1896 described a rather imperfect skull, now in the American Museum, as a new type of Cotylosaur under the name *Conodectes favosus*. The skull was badly figured, and not very well described. Still the skull exists, and everyone who has examined it seems to be of opinion that it is the same animal as was described by Broili in 1904 under the name *Seymouria baylorensis*. The two types were from the same formation and probably from near the same locality. No one so far as I am aware has ever been able to say in what points they differ. Williston wrote in 1916 that *Conodectes* is probably identical with *Seymouria*, and Case is inclined

to agree with Williston. When the American Museum made the restored skeleton of "*Seymouria*" I understand that the skull was mainly modelled on the type skull of *Conodectes*. If the two are identical the name *Conodectes* must be used, as it has eight years' priority. Those who use the name *Seymouria* must show how Broili's type differs from *Conodectes*. To regard *Conodectes* as a *nomen nudum*, as Watson suggests, seems quite impossible.

Another point I should like to comment on is with regard to the Crocodile, *Notochampsa*. In 1904 I described two species *N. istedana* and *N. longipes*. The former is from the Cave Sandstone. It has a fairly well preserved skull with most of the dorsal body armour and a few other parts: the latter which is from the Red Beds has no skull, but shows the pelvis satisfactorily, parts of both limbs and much of the dorsal and ventral armour. The pelvis of *N. longipes* is typically crocodilian, and though the skull of *N. istedana* is much specialized, it is also crocodilian. In Europe our oldest crocodiles are those of the Lias, and these are very primitive in type. I therefore argued that our South African *Notochampsa*, which is really not a very primitive crocodile, is more likely to be Lower Jurassic than Triassic.

About twenty years later both specimens were re-examined by v. Huene and Haughton. v. Huene regarded both as Pseudosuchians; but Haughton, while considering that *Notochampsa istedana* is a Pseudosuchian, agreed with me in holding that *N. longiceps* is a true crocodile; and believing that it is not allied to *N. istedana* he placed it in a new genus *Erythrochampsa*. Here the matter has rested till Barnum Brown in 1933 discovered a nearly complete skeleton of a closely allied form in Arizona, and he called it *Archaeosuchus richardsoni*. As the generic name was preoccupied by one of our South African reptiles Brown changed it to *Protosuchus*.

This Arizona crocodile seems to me to prove beyond dispute that our two South African fossils are very closely allied, and both crocodiles; and if not belonging to the same genus, at least belonging to the same family. When in America in 1937 I made a careful examination of *Protosuchus*. It is stated to have come from Upper Triassic beds; but when I inquired further about the age of the beds I was told by what I consider very good authority that there is a doubt about the age, and that the beds may be Lower Jursassic. I am not at liberty to state who informed me of this.

It is thus satisfactory to find that my 1904 determinations are fully confirmed, and that I may yet be right in believing that the Stormberg Beds are Lower Jurassic rather than Triassic.

Watson refers to *Youngina*, and says he is inclined to doubt my interpretation of the postero-lateral corner of the skull roof. "I think," he says, "it is probable that the squamosal is separated from the parietal by a very narrow supratemporal, interpreted by Broom as a tabular, and that a real tabular exists as a thin film on the occipital surface."

Fortunately *Youngina* is not very rare. We have one specimen in which the post-temporal region is almost perfectly preserved. It is essentially, though not quite, as I figured it in 1914. The parietal has a well-developed

posterior and outer process which forms the upper part of much of the occiput. This is probably the bony plate which Watson has mistaken for the tabular. Between it and the squamosal there is a single narrow bone which I regard as the tabular, but which Parrington, Romer, and others consider to be the supratemporal. There are only three bones in this region, and two of these are the squamosal and the parietal.

It matters little whether the intermediate bone is called supratemporal or tabular. As it is mainly an occipital bone, and as no fibres of the temporal muscle are attached to it in lizards it seems more likely to be the tabular than the supratemporal.

R. BROOM.

PRETORIA,
1*st September*, 1942.

REVIEWS

LANDSCAPE : AS DEVELOPED BY THE PROCESSES OF NORMAL EROSION. By C. A. COTTON. pp. xviii + 301, with 45 plates and 214 text-figures. Cambridge University Press, 1941. Price 21*s*.

Twenty years ago Professor Cotton brought out Part 1 of *The Geomorphology of New Zealand*. This Part was intended to serve as a " textbook of geomorphology for New Zealand students and general readers ". In a very favourable review in the *Geological Magazine* for 1923, p. 184, Professor Marr said : " It is, however, much more than that. It can be strongly recommended for use by students of any country, and is the most complete textbook of the kind in the English language which has come to my notice."

The volume now under review is much more limited in scope but much fuller and more detailed. It may, indeed, be looked upon as an expansion of a part of the previous work, brought up to date. It deals only with the landforms produced by what is often called normal erosion, i.e. erosion by moisture, rain, and rivers in a climate like ours. It is an unfortunate expression for, as the author allows, this kind of erosion has no more claim to be considered normal than other kinds in other climates.

In another respect the book is limited. Before any erosion can take place there must be an elevated mass to erode. The author considers the effect of warping and faulting upon erosion but not the influence of folded mountain chains and he seldom refers to volcanic action.

A book on landscape so entirely limited to one type of erosion and one type of earth-movement must necessarily be restricted in use. A visitor to the Alps will find little to help him in understanding the scenery that he sees ; a traveller in sub-polar latitudes or in the dry regions of the globe will find nothing. Even for a reader in England the book is incomplete ; there is no reference to the striking features impressed upon the country by the ice of the Glacial Period.