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Paper review

Why complicate anatomical terminology?

Bjerring, C. 1997. The question of the Eotriassic tetrapod genus *Wetlugasaurus* in Greenland and thoughts on the fossa coniformis entopterygoidea. — *Meddelelser om Grønland, Geoscience* 34, 1–25.

The worldwide distribution of a number of tetrapods during the Late Permian and Triassic time is widely regarded as obvious. It is consistent with a concept of the supercontinent Pangaea extending from high southern up to high northern latitudes with no geographic barriers interrupting its continuity. However, climatic differences between the margins and the center of the supercontinent must have limited the expansion of particular tetrapod species, and particularly that of clumsy fresh-water dependent amphibians such as the Triassic temnospondyls. While all but two of the eleven temnospondyl families known in the Triassic have been recorded from both the Northern and the Southern Hemispheres, the number of genera recorded in both Laurasia and Gondwana is just five. Each of them may prove to be a taxonomic artifact and should be scrupulously tested. That is just what has been done by the author with respect to an Eotriassic alleged *Wetlugasaurus* species, *W. groenlandicus* from Greenland.

The revision of the original Säve-Söderbergh (1935) material (the holotype and cotype) of *W. groenlandicus* has demonstrated that this species belonged to a new genus *Selenocara* different from *Wetlugasaurus*. The problem of congenerity of the remaining Laurasian wetlugasaur species with those from Gondwana (*W. milloti* from Madagascar and *W. magnus* from South Africa) and that of the relationships between *Selenocara groenlandica* and wetlugasaurs have remained out of the scope of the paper. The capitosaurid affiliation of Selenocara has just been briefly mentioned in the conclusions (p. 12: '*Selenocara groenlandica* has a general capitosaurid appearance').

In fact, the taxonomic problem discussed in the paper has been reduced to just a question of the generic identity of the material under craniological sudy. The paper is thus concerned more with anatomy than phylogeny. While the approach to the fossil material shows that the author is an anatomist, the way in which he treats the material demonstrates how sophisticated his anatomical training is. We are dealing with a representative of a famous Swedish school of anatomy, deeply-rooted in the 19th century European tradition of this branch of natural science. The references include a sequence of famous names beginning with Arendt (1822) paper 'De capitis ossei Esocis Lucii structurae singularis', and Cuvier's (1825, 1837) papers, and ending with modern (1990s) authors. The author's fascination with Latin is obvious, and it does not make his written style easier to understand.

The author is strongly concerned with the problem of homology, and consequently extremely sensitive to terminology matters. The strict dependence of the latter on the former sometimes results in a necessity of changes of anatomical names used for certain structures with changing understanding of their identity. However, the range of changes and the number of terminological novelties introduced by the author seems quite excessive, thus conflicting with the stability and informativeness recommended for anatomical nomenclature. As illustrated and explained in Fig. 2, the dermal skull roof of *Selenocara* (and, I believe, of other labyrinthodonts) includes a pair of 'frontopluteal' bones, instead of parietals, and a pair of 'iniacopluteal' bones posterior to them, instead of the postparietals, the names suggesting some fusion in ontogeny. The supratemporal is called parietal, which suggests an evolutionary shift of the bones that usually bear the parietal foramen (here for. neuroepiphysiale) in a lateral direction. Some bones have been just renamed (squamosal is a 'melonic

bone', tabular – became a 'trachelic bone'). The only reason for these and other revolutionary changes should be falsification of the homologies accepted hitherto. It is a pity that the reader is left without any comments and just sent to one of the authors earlier papers (1995, I must say, I am not convinced by this paper about the necessity of these changes).

The discussion of the embryological development and homology of the parasphenoid brings an interesting corollary that this bone forms as a result of fusion of a various number of small dermal elements ('crepidules') instead of being a normal dermal bone entity growing at the margins and getting thicker by apposition of bone layers. Still, the replacement of the widely used name 'parasphenoid' by a 'crepidine bone' seems unjustified. Better founded may be a substitution of the term epipterygoid with mesopalatoquadrate bone in the temnospondyles. In this case, we are not dealing with a variable set of serially homologous dermal elements, as crepidules are, but rather with some well understood endochondral bones and not just the epipterygoid only.

The main problem discussed in the second section of the paper relates to the kind of articulation between the braincase and palatoquadrate ossifications (nine points of contact in the Rhipidistia, according to the author) and the dermal palatal bones in primitive tetrapods (*Lyrocephaliscus, Benthosuchus, Greererpeton*). What is usually called a basicranial articulation is in fact a variable composition of bony joints. The very complicated matter discussed in this section requires quite sophisticated methods of thinking and a very detailed terminology referring to the nervous, circulatory and muscular systems of the head, similar to or taken directly from human anatomy and medical sciences.

The problem discussed has much bearing on the evolutionary development of akinesis in temnospondyles and some other functional aspects of labyrinthodontan craniology. Many interesting issues arise when reading this paper: (1) Whether or not the divergence in construction of what is usually called a basicranial articulation parallelled the main divergence of amphibian phylogeny? (2) Whether or not the development of an immovable sanidio-entopterygoid articulation (between the fossa coniformis and the affacial process) was functionally associated with the loss of kinesis in some temnospondyles? (3) If the sanidio-entopterygoid articulation helped to avoid dislocation during muscular contractions, was it better (in acting like that) or just different from a basipterygoid articulation? (4) Was the development of hearing mechanism suggested in the paper really dependent on the presence of the sanidio-entopterygoid joint? The author failed to formulate simple answers to these and other questions. He seems not to realize how difficult his hermetic language is for collegues working on most common fossil material. I feel that a certain gap separates the anatomical studies of the author from those of the majority of vertebrate paleontologists who constitute the natural audience for these sorts of ideas. It seems advisable that this gap be filled by a more accessible version of the paper.

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