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When hobby horses are pigs and when opinions converge...

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Pickford (1995) has united several pages of criticism on my work. This criticism does not form part of a discussion on some aspect of the study of fossil Suoidea, stratigraphy or paleobiogeography, but it is in the form of a list of 11 comments. In future papers I will continue to discuss differences of interpretation of the fossil record when they arise from the study of a particular subject, which seems to me the most appropriate way. However, I feel that I should respond to, at least, some of the comments by Pickford.

I am accused of misquoting and criticizing the wrong quotation. This refers to my «most unfortunate citation» of Pickford's opinion, where I wrote that he considered *Kenyasus* and *Nguruwe* as primitive Listriodontinae (Van der Made, 1992, p. 88). For Pickford the kubanochoeres constitute a separate subfamily, I include them in the Listriodontinae. I am sorry for this lapsus, but my arguments why *Kenyasus* does not belong to Listriodontinae, are equally valid for excluding it from the kubanochoeres, whether separate subfamily or a tribe of the Listriodontinae. If I have misquoted someone, it was not with the intention to criticize the wrong quotation. However, I feel I am the subject of frequent misquotations in these comments by Pickford. I am criticized for holding an opinion that Pickford himself held before, for having the same doubts or for committing the same errors. In some cases, it seems that I am criticized for an opinion which I never held, and in criticizing me Pickford adopts an idea which cannot be found in his earlier papers, but which was published by me.

I am accused of ex cathedra statements (p. 256). This probably refers to some abstracts of conferences, which are cited in many of Pickford's comments (Van der Made, 1992). Such abstracts reflect work in progress and generally precede a paper were the subject is discussed at length, as is also the case here (Van der Made, 1995, in press).

«Made's (1992) chronological ordering of the East African sites render his proposed identifications on phylogenies of Sanitheriidae dubious» (Pickford, 1995, p. 259). I have adhered strictly to arrangement of the localities in Faunal Sets by Pickford (1981a, 1986 a & b). Any criticism on this point must be considered as criticism by Pickford (1995) on Pickford (1981a, 1986 a & b). Pickford (p. 259) proceeded: «In fact, the Kenyan fossils come from levels dated from 15.5 to 16.5 Ma, making them more or less equivalent in age to the upper part of Zone MN 04 (MN 04b).« That is exactly the conclusion of Van der Made (1992, p.97) who used suoid evolution to correlate Maboko to European MN 4. It should be noted that Krijgsman et al. (1994) for the first time presented magnetostratigraphic dating of the MN 4/5 boundary at 14.1-14.05 Ma, whereas before the boundary was placed at 17 Ma. Pickford's reasoning resulting from the datations of the African localities simply was not possible before 1994. As far as I know, I am the first person who correlated Maboko to MN 4, but Pickford presents the correlation as if it were his, as if it were nothing new and as if it were in contradiction to my work. A more detailed discussion on correlations between Africa, Europe and the Indian Subcontinent is given by Van der Made (1995).

Pickford (point 10) wrote that he suffers from «doubts that the words 'large form' engender in the mind» because of my use of the name «*Parachleuastochoerus* large form» in a range chart and that the

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use of the right name *P. huenermanni* «would open up interesting biostratigraphic possibilities». These possibilities were fully recognized by Van der Made (1990a, figure 2), but ignored by Pickford (1981b) who studied the same material. The use of «large form» and «small form» in the figures (Van der Made, 1990 a & b) stems from the problem that the name *P. huenermanni* (Heissig, 1989) was not yet published when the papers were submitted. Subsequently minor changes were made in the text, such as the inclusion of the name *P. huenermanni*, but, for practical reasons, not in the figures. Pickford should be aware these facts, since I asked to comment on of the manuscripts in April 1988, when we met in a workshop.

Pickford (1995, point 1) criticized me (Van der Made, 1990b) for listing some taxa, including *Schizochoerus*, *Albanohyus* and *Barberahyus*, under the heading "Subfamily indet." and for not placing them in the Doliochoerinae, as was current practice. In continuation Pickford used my paper in support for raising Schizochoerini to subfamily rank, thus agreeing with me that the current classification was not correct. *Albanohyus* (= *Barberahyus*) turns out to be a suid and therefore not to be related to the "Doliochoerinae" (Van der Made, in press).

Pickford (1995, p. 256) suggested that I use almost exclusively dental characters and that he uses all available parts of the fossil record and that therefore his conclusions are more reliable. For instance Van der Made & Hussain (1992) were criticized for not assigning sanitheres to a family and not discussing the skull morphology relevant to this theme. It simply was not the aim of that paper to discuss any taxonomy higher than generic level. But well, I will discuss here some skull characters.

A character stressed by Pickford (1984 & 1995) is the supposed curved tooth row of the sanitheres. It is the right tooth row of an individual from Karungu that is curved when seen from buccal (Pickford, 1984, fig. 8). Other specimens like the maxillas from Bugti and Leoben were only figured by Pickford in occlusal view (Pickford, 1984, figs. 14 & 16), but do not show any curvature when seen from buccal. In fact the left tooth row of the individual from Karungu is not curved. It can easily be seen that there is a big gap between the right P^3 and P^4, but no such gap is seen in any other sanithere, nor in the left side of the same individual from Karungu. I have studied the specimen on which Pickford bases the character of the curved tooth row and I believe that the specimen is deformed and that the premolars are not glued in the right position.

A character which certainly would be of interest is the position of the cranio-mandibular joint. Pickford (1995, p. 258) states that it is located high above the occlusal plane of the cheek teeth. This observation must be based on the fragmentary skull from Karungu. In my opinion, an important part between the glenoids and the tooth rows is lacking and this is reflected in the fact that Pickford did not figure the skull as a whole, but figured the different fragments on different pages (Pickford, 1984, figs. 8 & 9). As a result of the fragmentary nature of the material, nothing can be said on the position of the cranio-mandibular joint.

Pickford (1995) used skull characters for placing *Kubanochoerus* in the Kubanochoerinae and not in the Listriodontinae. These characters include the shape and postition of the posterior choanae and the size of diastemas. A large sample of recent *Sus scrofa vittatus* shows that the length of the diastemas is subject to ontogenetic, sexual and individual variation (Van der Made, 1992). The same samples of *S. scrofa vittatus*, *S. verrucosus* and *S. barbatus* studied by Van der Made (1992) show also that there are great differences in diastema size of these species. These samples also illustrate that the shape of the posterior choanae is subject to ontogenetic age and their position varies greatly between one species and another of the same genus.

Like Pickford, I do take cranial morphology into account and like Pickford (p. 237) I do study recent suoids. But unlike Pickford, I conclude from the study of variation of cranial characters in recent samples, that cranial characters in limited fossil samples should be interpreted with caution. However, in the first place, one has to recognize the limitations of fragmentary or deformed fossils.

Though every palaeontologist has a superficial knowledge of the International Code of Zoological Nomenclature, many become only much later familiar with some of the peculiarities of the ICZN. Pickford, citing the ICZN (point 5), points out that I have wrongly ascribed the authorship of the Sanitheriidae to Pickford, 1984, who only raised the taxon from subfamily to family rank. Though this is no excuse, it was a common error to cite the wrong author for a suoid subfamily: the author of the Listriodontinae is not Simpson, 1945 but probably Gervais, 1859 (Van der Made, 1995), likewise the author of the Tetraconodonitinae is not Simpson, 1945, but Lydekker, 1876 and Zittel, 1893 is not the author of the Suinae, as Gray, 1821 described the Suidae. I have been long overdue in recognizing this nomenclatorial problem and so was Pickford (1988, 1994, etc.).

Another peculiarity of the ICZN is the principle of priority. Pickford, 1988 cannot be the author of the Hippohyini, since the name was already in use (Thenius, 1972).

Many of the comments by Pickford appear to be criticism for the sake of criticism. In the cases when Pickford «criticises» my stratigraphical interpretations, he appears to have adopted my results: he correlates Set III localities to MN 4 (as was done before by Van der Made, 1992), he recognizes the stratigraphical potential of *Parachleuastochoerus* (as was done before by Van der Made, 1990a) and he agrees with me (Van der Made, 1990b) that *Schizochoerus* should not be placed in the "Doliochoerinae". In these cases, we do not have divergent, but convergent opinions and there is no reason for "criticism" at al. In fact we agree on the vast majority of interpretations of suoid fossils.

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