

Dear authors,

Please find enclosed my revision of the study entitled: *The phylogenetics of Teleosauroidea (Crocodylomorpha, Thalattosuchia) and implications for their ecology and evolution* performed by Johnson, Young, and Brusatte.

First of all, I would like to set out that I have no previous experience in this particular group of cocodrylomorph, but I can recognize a good study when it fall into my hands. This is one of those rare occasions that I enjoyed reviewing a paper.

Despite its extension - up to 230 pages - the manuscript is very well written, with compressive and clears structure, being easy to read, which is appreciated.

It is beyond doubt that authors possess a thorough knowledge about the topic of study, providing detailed explanations about the historical background and anatomical descriptions of each studied specimen. Furthermore, all this information is sustained by an extensive review of the specialized literature, considering both classic and most recent bibliographic references. Therefore, I have to congratulate the authors for their work.

The methodology followed for the authors is appropriate. This is one of those uncommon occasions where phylogenetic methodology is well explained, and with enough details (memory setting, n° of replications, n° of saved tree per replication, etc..) that allow anyone to replicate the experiment.

I have no issue with the Systematic section; in fact I think that it represents a beautiful example of what a full-systematic-review paper should be, and I truly believe that the study has the potential to become a key-stone for future papers on teleosauroid cocodrylomorphs.

Saying that, all my comments are focused in the Discussion section, especially on paleoecologic and biogeographic implications.

Line 4362- Ecomorphological diversity

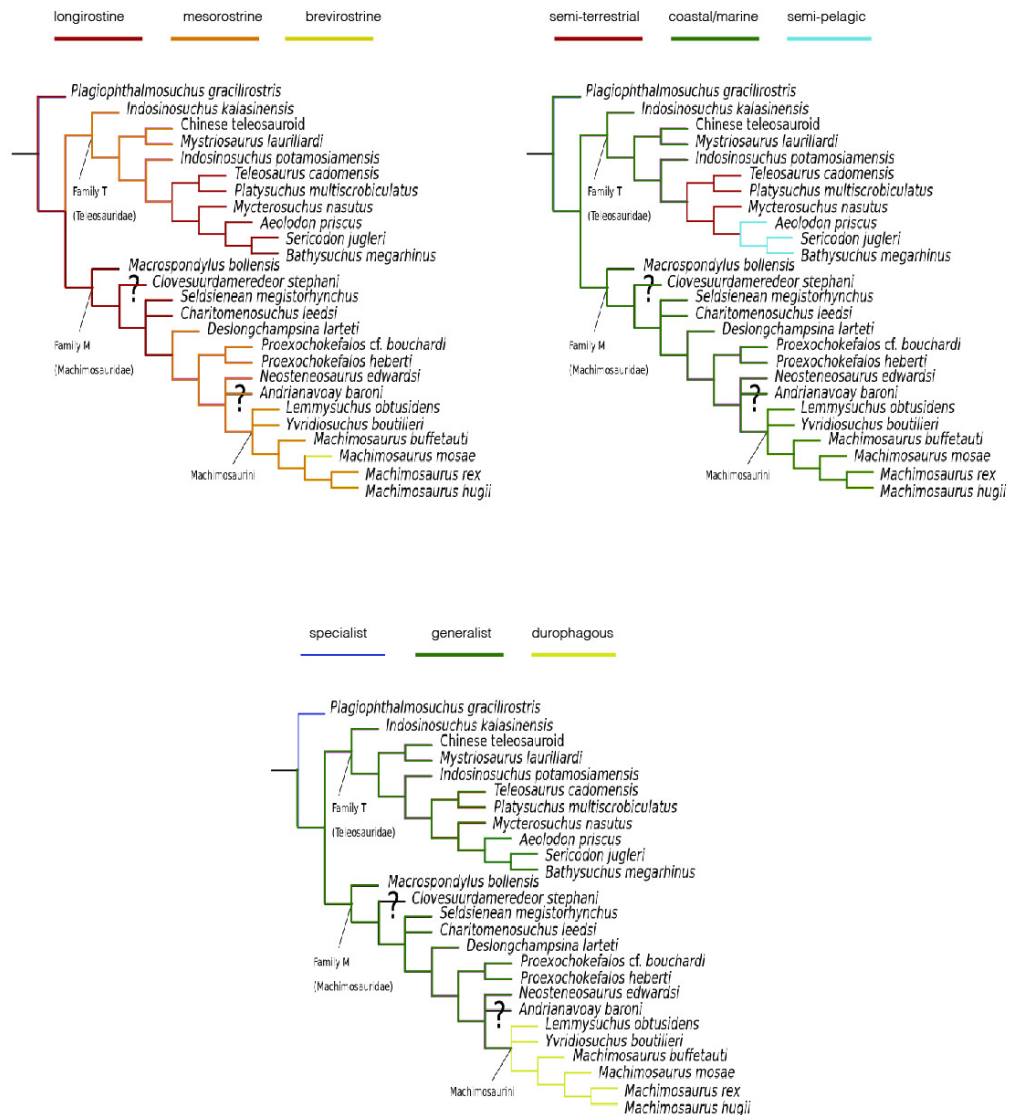
According to the text, teleosauroids can be classified in different ecomorphotyps, but I think that criteria are a bit mixed.

Only one criterion is common and applied to all taxa, which is that concerning the **general skull shape** (longirostrine, mesorostrine, brevirostrine). But then, depending on specie they are classified according its **main dietary adaptations** (piscivorous/specialist, generalist, macrophagous/durophagous); while other are classified according to **adaptations to particular environment** (semi-terrestrial or semi-pelagic).

Consequently, that makes the results a bit confusing for a non-expert reader (like me). For example, in both text and Fig. 63 *Platysuchus*, *Teleosaurus* and *Mycterosuchus* are identified as belonging to the longirostrine semi-terrestrial guild, but were they generalist? Durophagous?

The same question arise for *Aeolodon* + *Sericodon* + *Bathysuchus*, which are identified as longirostrine pelagic, but is unclear if they were generalist consumers, or specialized piscivorous or macrophagous/durophagous.

Figure 63 does not help to solve these doubts and neither reflects the ecomorphological diversity of the group. Thus, I suggest changing this figure (please, see my proposal below).

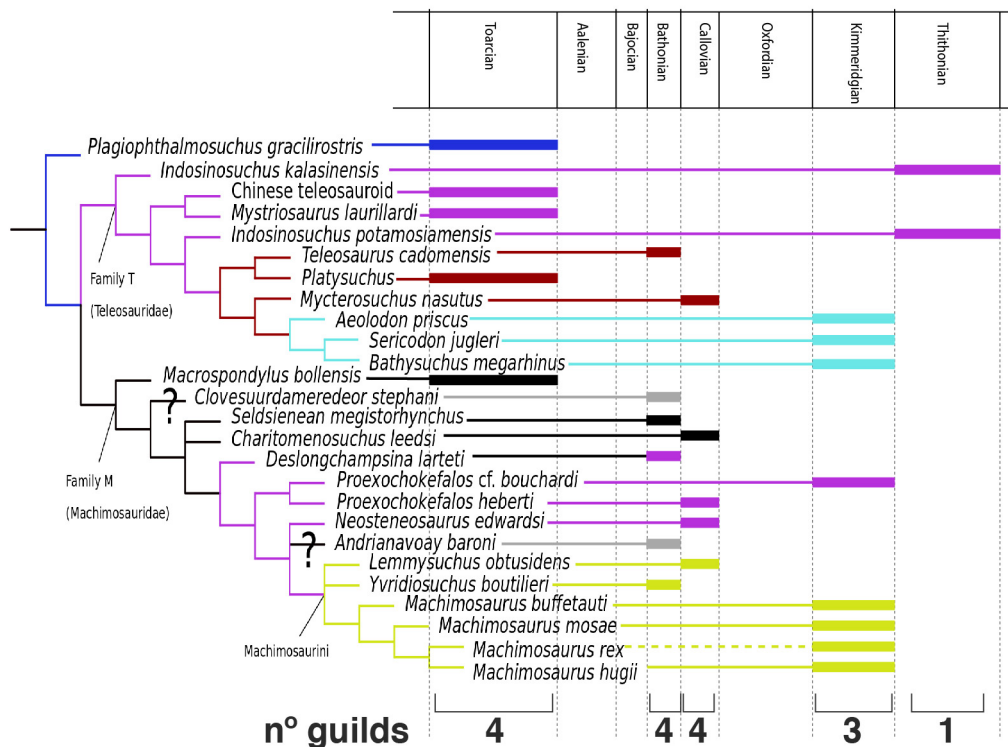


Line 4387 to 4434

Authors describe the occurrence of the different ecomorphologic guilds co-existing in the same time period, and even provide evidence about the appearance/disappearance of certain ecomorphotips along time. But in a more macro-evolutionary viewpoint, it remains unclear if the ecomorphologic diversity of teleosauroids changed or remained stable along 70 Ma. In other words, was the number of guilds constant though time?

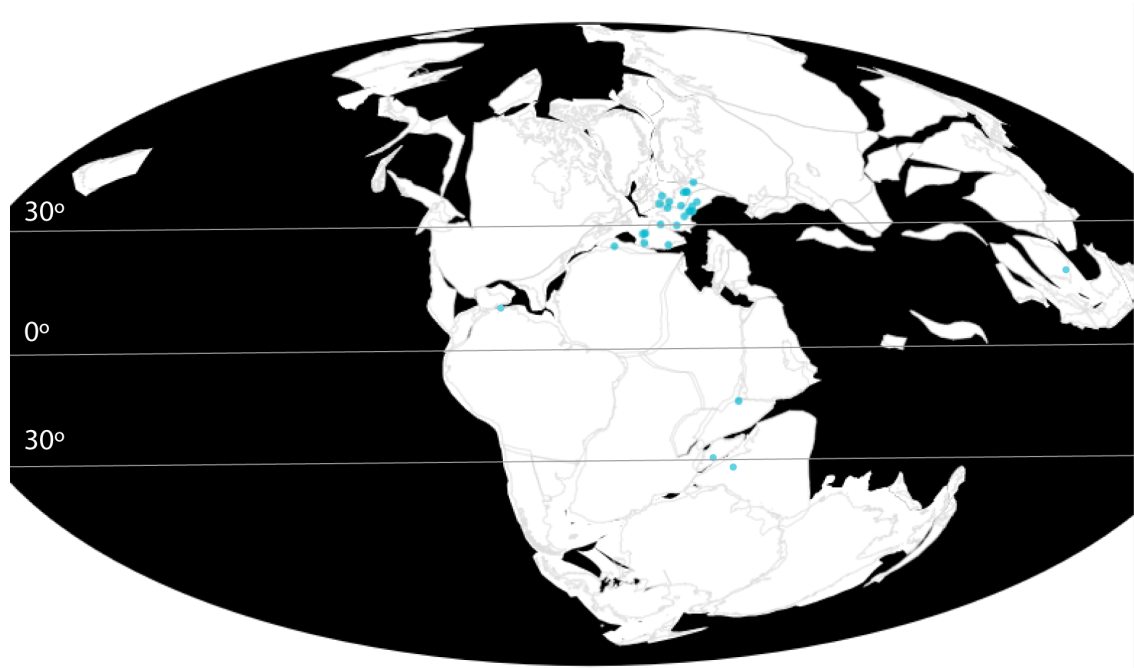
Could that ecomorphologic diversity reflect any specific niche partitioning strategy among teleosauroids that live at the same time?

In this regard, it would be useful if authors could provide a time-calibrate phylogeny showing the time distribution of each guild. (please, see my proposal below).



Line 4467 - Biogeographical distribution

I have my concerns about the porpoise of this section. It seems that it is focused on describing the current geographic distribution of the fossil remains rather than paleobiogeographic distribution of teleosauroids during the Jurassic. The alleged global distribution of the clade seems inconsistent when data is included in a global paleogeographic context (see attached figure below ,modified from <https://www.paleobiodb.org/navigator/>).



Despite of the results might be influenced by sampling, outcrop availability or other biases; a particular patter can be observed. As I said, I am not an expert in teleosaurids, but it looks like that teleosaurid are concentrated around the Jurassic tropic belts. This biogeographic distribution is even more noticeable when considering paleo-climate data (Rees et al., 2000).

Rees PM, Ziegler AM, Valdes PJ (2000) Jurassic phytogeography and climates: new data and model comparisons. In: Huber BT, Macleod KG, Wing SL (eds) Warm climates in earth history. Cambridge: Cambridge University Press, pp 297–318

Additionally, it seems that they primarily diversified and dispersed around the Tethys Sea.

Consequently, if authors want to keep this section in their study, I suggest changing the direction of the paleobiogeographic section. I also recommend changing current Fig. 64 for a paleogeographic map (*midd-late Jurassic*) showing the biogeographic distribution of teleosauroids.

Line 4585 - *Palaeoenvironment*

To me, this section is confusing because I cannot see the main goal of the author in this part. Do they want to prove evidence of “convergent evolution” among certain teleosauroids and extant crocs?

Most of the text seems to be focused in justifying the occurrences of teleosaurids in fresh-water deposits, especially *Indosinosuchus* and the Chinese teleosauroid.

However, in my opinion the results fall in the speculative spectrum rather than prove strong evidence of adaptations to certain environments.

Consequently, I suggest removing this part from the manuscript.

Line 4651 - *Teleosauroid histology*

This is a short section that can be described as a review about teleosauroid histology. Given that authors do not provide any new data, and that the results of this discussion seems to be disconnects from the rest of the study (which main goal is the systematic phylogenetic review of the group), I suggest to remove this part from the manuscript.

Finally, I have additional comments:

Line 35- add comma after Buffetaut and Hua

Line 271- review the forma of reference, they should go without parenthesis.

Line 341 and 342- review the forma of reference, they should go without parenthesis.

Line 1033- ilial or iliac process?

Line 1265- Something is missing after *Indosinosuchus kalasinensis*, the name of the author? sp. nov.?

Line 1328-add Jäger between *Macrospondylus* and 1832

Line 1337- delete "s" from Figs.

Line 1473- Autopomorphic features of *Se. megistorhynchus* are observed in lateral view. However, in Fig. 15 the specimen is displayed in dorsal view. It would be appreciate if authors could include a picture in lateral view showing the diagnostic feature.

Line 1565- "*nasutus*" in italic

Line 1792- delete "s" from Figs.

Line 2004- delete "s" from Figs.

Fig. 19 seems a bit unfocussed, can it be changed?

I am sure that by doing little changes on the current manuscript, the study should have no problem to be accepted.