Dear Recommender,

We thank you warmly for being given the opportunity to revise our manuscript. First of all, we deeply apologize for such a delay in submitting the revised version, as a collateral damage of the dramatic events started in February 2022.

We have taken into account most issues listed by the three reviewers and we are providing detailed responses to your suggestions and theirs here below (denoted by “R: ”).

The revised manuscript shows edits and new elements, highlighted either in yellow (following Deng Tao’s suggestions), green (following Jérémy Tissier’s recommendations), or blue (our own changes).

He hope that this new version will fulfill the standards of PCI Paleontology and look forward to get your feedback on it.

Best regards,

Alexander Sizov on behalf of coauthors

**Moderate revision**

This manuscript was seen by three reviewers as well as myself. All provided detailed comments on the manuscript including pdfs with commentary. Overall, the manuscript appears to be a welcome addition to the paleontological literature, with the detailed description of a skeleton from Tagay and a phylogenetic analysis and revision of teleoceratines that the reviewers find convincing.

R: Thank you very much for this general comment.

I recommend moderate revision, as the changes to be made are largely in the text, though these are quite substantial. In particular the Comparison section is quite lacking, and it is not clear that so much description of the phylogenetic results is needed (we can see the relationships in the tree).

R: We do think that the thorough description of the tree is of interest (notably for readers who are interested in these aspects, such as the reviewer #1) and we have therefore left this part as it was in the original manuscript. On the other hand, we have considerably enhanced the comparison section.

One reviewer proposes that more taxa should be added to the dataset, and also suggests considering other morphological datasets.

R: Not agreed. In our opinion, these requests fall beyond the scope of the current paper.

Two reviewers would like images of the carpals and tarsals.

R: OK. This was a pretty relevant request! Done.

The figure captions are also all in need of improvement.

R: OK. Sorry for that. Done.

Measurements of the cranium and teeth are missing from the supplementary dataset S3, in which also almost all cells are blank. Please provide measurements for all listed taxa, or remove these rows.

R: We are sorry. This was a wrong (provisional) version of the table, not intended for submission or publication. We have replaced it by more convenient ones (with more measurements/without the blank rows).

Further details are needed on which specimens were used to score taxa in the analysis.

R: In the original submission, these details were already provided as notes related to every terminal taxon in the PAUP matrix itself (e.g., *Hyrachyus eximius* was “scored after Bin Bai et al. (2017) and personal observation (AMNH material) by POA”; Brachydiceratherium lemanense was “scored after Boada Saña (2007, 2008), Boada Saña et al. (2008), Becker et al. (2009), Jame et al. (2019), and personal observation by POA (notably Gannat material: Rhinopolis, FSL, MNHN; MHNT)”). We admit that they were not immediately available and therefore we have added a sentence in the text, in order to guide the readers toward this part of the document. Please note that this information is accessible through any free word-processing software (e.g., Wordpad, Notepad or TextEdit), notably for readers who do not use PAUP or .nex matrices in general.

Additionally, I suggest you upload the morphological matrix to a repository such as Morphobank which will make it more visible and easier to use for later workers.

R: Thank you for your suggestion. We have uploaded the final matrix to Morphobank accordingly.

Also please add page numbers to the next version.

R: Sorry for that. Our mistake! We have added the page and line numbers in the current version (R1). Also, a circus is

[Download recommender's annotations](https://paleo.peercommunityin.org/download/t_recommendations.recommender_file.8d7ed8ad6cc49e1c.323032322e30372e30362e34393839383776312e66756c6c5f4642732e706466.pdf)

**Reviews**

*Reviewed by Jérémy Tissier, 27 Jul 2022 21:16*

This is an excellent and very-well written manuscript on new rhinocerotid remains from the Early Miocene of Russia. It presents the first phylogenetic analysis of a major Rhinocerotid group, the Teleoceratina, which was greatly needed. The results are extremely interesting, and have profound taxonomical implications (monospecific *Diaceratherium*, “resurrection” of *Brachydiceratherium*), as well as palaeobiogeographical, which are all clearly justified by the phylogeny. As such, the title and abstract are totally appropriate for this manuscript.

R: Thank you very much for your kind comments.

All the necessary data are provided to replicate the phylogenetic analyses, with a lot of details, which is very helpful and appreciated.

I ran the phylogenetic analyses and obtained the exact same results as presented here, with the same parameters. The discussion on the phylogenetic results is very detailed and very useful.

R: Thank you.

However, I wasn’t sure if the new specimen had been coded on its own as a terminal in a prior analysis, before being merged with *B. shanwangense*, or not? If that is the case, it should be mentioned, as that would greatly support the identification of the new specimen.

Indeed, it is mentioned in the Conclusion that “the numerous associated features documented and scored in the Tagay rhinocerotid skeleton have allowed for assigning it to the same teleoceratine species (*Brachydiceratherium shanwangense*) as in Shanwang, eastern China.” But I could not find that in the rest of the text.

R: We are grateful to the reviewer for this remark. We had indeed scored the Tagay individual and *B. shanwangense* as two distinct terminals in a prior analysis (not included in the previous version) and we have now integrated the corresponding matrix, with 32 terminal taxa, and the resulting PAUP buffer. In that analysis, the Tagay skeleton and the previous hypodigm of *B. shanwangense* were differing in a single and only feature (char. 36: occipital crest concave in the former).

However, if that is not the case (i.e. the specimen was merged with *Brachydiceratherium shanwangense* prior to the analysis), then I would say that the “Comparison” section should be improved, and should justify the attribution of this specimen to the species *B. shanwangense*. Currently, there only seems to be one character (mentioned in this section) that supports it: the absence of p1/d1s. However, the d/p1 is also absent is some (or maybe all) specimens of *B. aginense* (Répelin 1917) and some of *B. lemanense* (e.g. from Wischberg, Jame et al. 2019). Although I fully agree with the identification, I think it should be further justified. In addition, very few *Brachydiceratherium* species (or even Teleoceratina) are mentioned in this comparison.

R: We have now included the prior analysis (with 32 taxa and Tagay/Shanwang skeletons scored separately; see above). Unfortunately, the comparison section provided with the original submission was not the last – updated – version but a drafted one. We are really deeply sorry for that. The comparison provided here.

I would also suggest mentioning, if possible, which specimens, or localities or even references have been used to score the taxa in this matrix. I think that is important for *Brachydiceratherium* in particular, due to its quite complicated taxonomic history, which led to confusions (see Boada et al. 2007 or Jame et al. 2019 for example).

R: In fact, this information was already available in the original submission (as notes related to every terminal taxon in the PAUP matrix itself. We have added a sentence in the text, in order to guide the readers toward this part of the document (see response to the Recommender for further details).

Otherwise, the text is is very clear, and the descriptions are very well made. The figures are also of very high quality, although I have a few suggestions:

- Fig. 1: I thought that a small map, locating the Tagay locality in a larger scale (e.g. Russia) would be useful

R: OK. Done.

- Fig. 3: the caption for fig. D is missing.

R: OK. Done.

- Fig. 4: there is a confusion with the position of the mesostyle and paracone fold. Also, change "Valley posterieure" to "posterior valley"

R: OK. Done.

- Fig. 5: Please mention the different views shown (ventral, anterior etc.)

R: OK. Done.

I would add however, that figures of the carpal and tarsal bones would be extremely useful. Although the descriptions are very good, a figure is almost always better than words. If necessary, they could be added in supplementary material instead of the paper.

R: OK. We do agree that these figures facilitate understanding the corresponding descriptions. We have added them in the new version, with classic views.

I have also noted a few minor corrections in the annotated pdf that should be addressed/corrected. I found one reference in the legend for fig. 13 that was not cited in the reference list (Jame et al. 2019), and the reference for Heissig 1972 should mention a or b, as there seem to be two.

R: OK. Thanks a lot. We have added Jame et al.’s work in the reference list and distinguished both monographs by Heissig (1972) in the text and in the reference list. Most issues detailed in the annotated pdf originate from shared documents with successive versions and edits.

Overall, I think my main suggestions would be to improve the comparison section, by detailing which characters (in addition to the d/p1 absence) support the identification of the new specimen to *Brachydiceratherium shanwangense* OR to mention whether this specimen was added as a separate terminal; to mention (even shortly) the material on which the scoring of some terminals has been made; and to provide figures for some of the unillustrated carpal and tarsal bones. This remains nonetheless an excellent manuscript, and I am looking forward to reading the final paper!

R: No further comments (see above for detailed responses) except that we thank you again for the depth of your review and the relevance of your suggestions.

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*Reviewed by Panagiotis Kampouridis, 15 Jul 2022 16:49*

The manuscript of Sizov et al. entitled “An early Miocene skeleton of *Brachydiceratherium* Lavocat, 1951 (Mammalia, Perissodactyla) from the Baikal area, Russia, and a revised phylogeny of Eurasian teleoceratines” represents a very detailed description of a partial skeleton of *Brachydiceratherium shanwangense* from the early Miocene locality of Tagay and an overview of the Eurasian teleoceratines. I believe that it is a very nice example of how such well-preserved material should be treated with detailed descriptions and many detailed photographs of every bone. Especially the figures of the cranial, mandibular and dental material are very well made with very nice drawings of the specimens, which make it much easier for the reader to understand the morphology.

This kind of comprehensive study of all available material, including almost all of the postcranium of the skeleton, is definitely extremely helpful for future studies of teleoceratines and fossil rhinos in general. The results of this study have an important impact on the phylogeny and biogeography of Eurasian teleoceratines.

The manuscript is well structured and well written. The methods the authors use to study the material is adequate.  The phylogenetic analysis performed by the authors is sound and appropriate for the group.

R: We really appreciate your general comments.

I only have few comments to make on the manuscript. The most important one concerns the Comparison of the material (p. 27-28). It is rather short and very limited concerning the taxa with which the material from Tagay in compared to. Especially since the description is so comprehensive, it would be great to also have a more comprehensive comparison including also a bit more information about postcranial features (where possible).

R: OK. As mentioned in our response to the Recommender and Reviewer #1 (see above), we have expanded the comparison in providing details on features of interspecific / intergeneric interest.

I would suggest to split the comparison in two. The first part would concern the comparison of the material across the Eurasian teleoceratine genera (*Prosantorhinus*, *Brachypotherium*, *Diaceratherium* and *Brachydiceratherium*) and demonstrating that the material belongs to the genus *Brachydiceratherium*. In the second part the material from Tagay can be compared within the genus *Brachydiceratherium* with the most important species to show that it belongs to Bd. shanwangense.

R: We deeply acknowledge you for this suggestion. As mentioned above, the comparison as provided in the original submission was not the up-to-date version, but a draft. We apologize for that. In fact, the comparison is organized by anatomic region (skull, mandible, forelimb, and hindlimb). We hope that this section would nevertheless read well as it stands.

Otherwise, first the taxonomy of the Tagay material could be established and then in the second part it can be compared more widely to other Eurasian teleoceratines.

R: See our response to the previous comment.

On the other hand, the phylogenetic analysis is discussed in very much detail. I think that it could be somewhat shortened, leaving out information about some not so relevant taxa. Although it’s not necessary.

R: Not fully agreed. Some readers may focus on a classic comparison (post-description), whereas others may prefer reading a node-by-node depiction of sequenced synapomorphies, at inter-tribal, intergeneric and interspecific levels in the phylogenetic section. We do think that both deserve to appear in the paper, especially as it is online only and there might be no strong constraints on length.

My last comment concerns the figure captions of the photographs. The figures themselves are very nicely done, however, the respective figure captions are too short, not giving any information about the views in which the specimens are seen. I would suggest to change all figure captions of the photographs of the material and mention in every one of them the taxon (*Brachydiceratherium shanwangense*) and the views in which each specimen is portrayed, to compliment the very detailed figures.

R: OK indeed. Sorry for having provided incomplete captions in the original version.

Some minor changes are also noted in the attached .pdf file of the manuscript.

R: Thank you. We have taken into account these complementary suggestions.

Thank you for considering me as a reviewer and I would like to congratulate the authors on this well written manuscript. I am looking forward to seeing it in its final form!

R: We are very grateful for your comments and suggestions. We have also modified typos and most stylistic suggestions you had provided in the annotated pdf.

Panagiotis Kampouridis

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*Reviewed by Tao Deng, 25 Jul 2022 10:18*

Because the manuscript has no line numbers, I upload the annotated original MS with my comments.

R: We deeply apologize for having uploaded the original MS without line numbers and we warmly acknowledge your constructive comments and suggestions, even if some of your remarks may appear tactless (e.g., “ridiculous”, “not reliable”). We have addressed most of them (i.e., added references, contextualized the study, improved the comparison, added figures for carpal and tarsal elements, and provided synthetic measurements). Nevertheless, here below you will find detailed rebuttals to the ones we are not agreeing on:

-We have chosen to use the current matrix, originally intended for Elasmotheriinae and a broad sample of non-Elasmotheriinae (Antoine, 2002, 2003). It has long proven to provide also consistent results on stem Rhinocerotinae (Antoine et al., 2003, 2010; Boada Saña, 2008) and stem Rhinocerotidae (e.g., Becker et al., 2013). Note that slightly reworked versions have also been used more recently for Rhinocerotina within Rhinocerotinae (Pandolfi et al., 2021; Antoine et al 2022) or other taxa among Rhinocerotidae (works by Geraads, Tissier, Sanisidro and colleagues). The fact that we “could not get reliable results” based on this very matrix or that it “is not reliable and acceptable” without more elasmotheriines is quite harsh as a statement – and, to our mind, unsupported.

-We acknowledge your suggestion to add more elasmotheriines and more rhinocerotid taxa, but we prefer to maintain our original taxonomic sample, as it was thoughtfully elaborated to address the question (phylogenetic affinities of Tagay rhinoceros; systematic arrangement of Eurasian teleoceratines).

-We do not agree either about Rhinocerotidae comprising Rhinocerotinae, Elasmotheriinae, and Aceratheriinae (with Aceratheriini and Teleoceratini) as being “universally accepted”, as it is at odds with phylogenetic results published by Antoine and other workers (e.g., Becker, Geraads, Sanisidro, Pandolfi, or Tissier, among others), depicting instead Aceratheriini as being subordinate to Rhinocerotinae. This is the classification favored in the “Database of Fossil Rhinoceros Species: Neogene and Quaternary Old World Localities” edited by Geraads et al. (2021), with most World specialists of rhinocerotids, including yourself, as coauthors. We have therefore maintained the arrangement, as is further coincides with phylogenetic results obtained in the current work.

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