

Reviewer 3

This manuscript provides detailed lipid biomarker analyses of exposed permafrost (including ancient deposits) from the recently exposed Batagay Megaslump, East Siberia. I generally found the paper to be a good read and the analyses were appropriate for the sample type, of which the deposit is quite novel and covers an impressive time window (up to 650 ka yr). A number of existing/in review studies have worked on this exposure, so it is good to see one which focuses on organic geochemistry/biomarkers.

To improve the manuscript further the palaeoclimate discussion section would benefit from some revisions by placing each time period more clearly in place with the known climate and environmental conditions at the time (palaeoclimate). Placing the results within a critical framework explaining where these results agree with (or disagree with) existing findings in each time period a little bit more clearly would be helpful and enable the interpretation of any further palaeoclimate interpretations of significance. A table might help here and a little bit of wider reading.

Thank you for your comments and feedback. We appreciate your suggestions on the palaeoclimate discussion. In order to have more focus on the interpretation of the biomarker results in a paleoenvironmental context, we added some more comparisons in the discussion (e.g., L239, L248, L251) to previous studies (Ashastina et al., 2017, 2018; Murton et al., 2017, 2021; Opel et al., 2019; Vasil'chuk et al., 2020; Courtin et al., in accepted) that focused on the palaeo-aspect. Note: the line numbers refer to the preprint version.

I was also wondering if the group had any compound-specific stable isotope analyses, for example on long chain alkanes or fatty acids to identify any differences between interglacial vegetation and its source/productivity? If they do, then these data should be included.

Unfortunately, we did not perform compound-specific isotope analyses on our samples. Thank you for the suggestion, we will keep this in mind for future work

The discussion would also benefit from a synthesis plot plotting regional data from other palaeobotanical proxies from the site (e.g. Ashastina et al., 2018; Opel et al., 2019) and more widely regionally. This will also assist with improving the palaeoclimate discussion section and show to readers more directly the similarities.

We considered your suggestion, but decided we would rather not incorporate such a table in our manuscript. As mentioned above, we added some more comparisons and linkages to previous palaeo-studies in the discussion, but think that a synthesis table is beyond the scope of this manuscript. A first approach of such a summary of the palaeo-climate findings was made by Opel et al. (2019) (Table 4). A full synthesis of palaeo-finding is furthermore complicated as different authors have worked in different years and parts of the slumps. The stratigraphic and

chronological relation between different sampling years and profiles hasn't been fully established yet, mainly due to rather poor age control beyond the radiocarbon range and because additional luminescence dating is currently in progress but results will not be available until the end of this year.

Finally, I note that the current title is quite descriptive in nature. An alternative approach is to revise the title to make it more impactful and focus on the main finding or outcome from the paper. More interrogation of the palaeoclimate discussion section will help here, but an example could be (from the abstract) to devise a title that focus on terrestrial character of the glacial periods, or the high microbial activity in the interglacial. The potential lability of the Holocene deposit (and its vulnerability) could also be an option as here it is possible to make comparisons with older deposits so effectively.

Thank you for the suggestion. We agree that the title could be less descriptive and instead provide some insights on the conclusions. We changed the title to "Molecular biomarkers in Batagay megaslump permafrost deposits reveal clear differences in organic matter preservation between glacial and interglacial periods".

Detailed comments

31. Do you think that the most recent Holocene deposited permafrost is most vulnerable to warming, leading to OM degradation?

No, from the depth-perspective the active layer deepening is not as important in this location as large amounts of all horizons are mobilised every year through deep permafrost thaw, i.e. the lateral thaw and subsequent thermo-denudation. The dominating factor for permafrost vulnerability clearly is the high ice content (mainly ice wedges) of the glacial age deposits.

36-37. Sentences with the same reference (Strauss et al., 2021) could benefit from merging or rephrasing to improve readability.

Changed accordingly.

74. Perhaps change 'herbs occur' to 'herbs are present'?

Changed accordingly.

96. Change 'the Spring Expedition' to 'a spring expedition'

Changed accordingly.

115-116. What instrument/technique is used for medium pressure liquid chromatography? I think you should include this.

Changed accordingly.

118. “biogeochemical and alkane parameters” Alkane parameters are ‘biogeochemical’ so rephrase?

We removed “and alkane” as suggested.

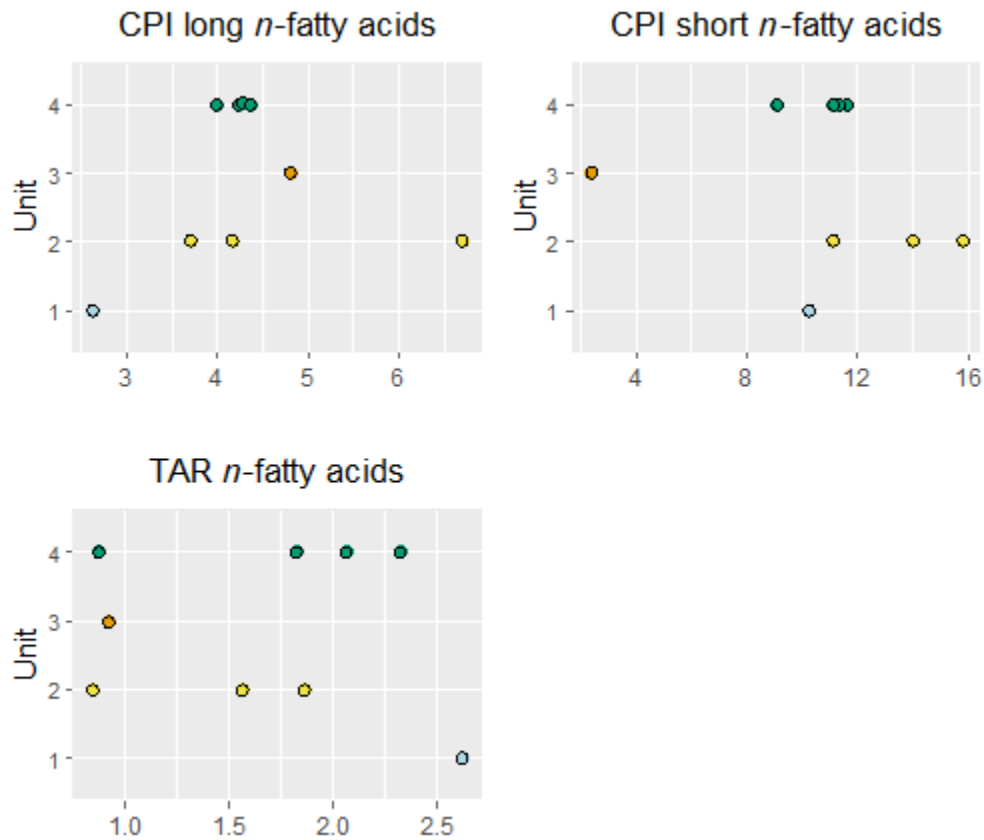
Table 1. References to the sources of these indexes could also be included in the table in brackets to enable easy source identification.

We added the references in the table as suggested.

157- 158. Should it read “Here, we found...”?

Changed accordingly.

Figure 2. You display a number of indices. You could also consider calculating and displaying TARFA (Meyers et al., 1993) and CPI for fatty acids (Matsuda and Koyama, 1977).



Yes, we also thought about these parameters, but they didn't add much new aspects to the story. Overall, the CPI of the long *n*-FAs looks rather similar to the CPI of the *n*-alkanes. Glombitza et al. (2009) showed that the CPI of the long FAs decreased with increasing maturation, similar to the CPI of *n*-alkanes. However, this was over the whole range of diagenetic transformation into early catagenesis. Such a broad maturation range cannot be expected for our samples here, which represents immature OM in the initial diagenetic transformation stage. Thus, as for the *n*-alkane CPI, the *n*-FA CPI here also represents rather variable OM sources, which was also observed by Glombitza et al. (2009) for the early diagenetic maturation range. The origin of short chain FAs is not very clear and they can derive from aquatic, terrestrial plant and bacterial biomass. Thus, interpretation is quite difficult and we have other parameters which are more specific such as the IA Index.

The TAR_FA simply shows the ratio between short and long *n*-FAs and could, thus, provide a hint on the ratio between aquatic/bacterial to terrestrial OM. Thus, the very low TAR_FA value in the Woody Layer might suggest that the short FAs are aquatic rather than bacterial, because

the IA Index is relatively high in this layer. However, the TAR_FA is, as explained, not very specific and we decided not to use this parameter in our study.

188. Please double check you are happy with use of the term ‘lenses’.

Yes, we prefer to use the same terminology as Murton et al. (2017) (“lenses of woody debris”), Ashastina et al. (2017) (“pronounced lenses”) and Opel et al. (2019) (“organic-rich unit ... in lenses up to 3 m thick”). We added “organic-rich” in the text.

201. Is the comma (,) necessary after ‘Above’?

Yes, because we talk about the Upper Ice Complex and not about the layer above that. For more clarity we removed the word “above”.

202 & 214. ‘medium high’ – how about moderately high?

Changed accordingly

Discussion first paragraph (223-229) – Make clear the differences are seen over contrasting climate periods (glacial-interglacial periods), showing how land-cover varied over different climate regimes.

We added “following different climatic periods (e.g., glacial and interglacial periods)” to the text.

233. ‘to some extent’ a bit vague. Can you explain to what extent or specify?

In order to avoid the confusion, we removed “and to some extent also quantitatively”.

240. Write ‘more terrestrial and less aquatic’ to correspond with preceding order of high ACL and low Paq.

Changed accordingly

248. Do we know which microbes were present if abundance and what is their function? Check with Courtin et al, (in review).

Thank you for your comment. Microbes were present in abundance in every investigated sample from Courtin et al., (accepted) with a core community of Actinobacteria and Haloarchaea in each sample. Still, distinctive patterns were identified between samples. In general, the microbial community consisted mainly of cold adapted and typical permafrost soil Bacteria but for example, in the lower part of the Lower Sand Unit, *Clostridium*, *Nocardioides*, or *Propionibacterium* were well represented suggesting higher degradation and organic matter cycling in the soil. More details can be found in Courtin et al. (accepted). We added this in the discussion (L239, L248, L251).

249. Pollen findings – could these unpublished findings/companion papers data be introduced

here in a summary synthesis plot (most impactful findings) to help with the biomarker comparison?

The pollen data do not cover the same profile as they originate from a reconnaissance sampling campaign in 2017 and therefore cannot be related accordingly.

Discussion of the time periods would be helped by a table showing time period, deposit, references and interpretation to make this section clearer and easier to follow.

As mentioned above we would like to keep the clear focus of the paper. In Figure A1 the results are visualised over depth and unit.

281. Suggest replace ‘stronger OM’ with greater or ‘higher OM’.

Changed accordingly

309. I think the role of diagenesis/preservation in the Holocene deposits could be introduced here.

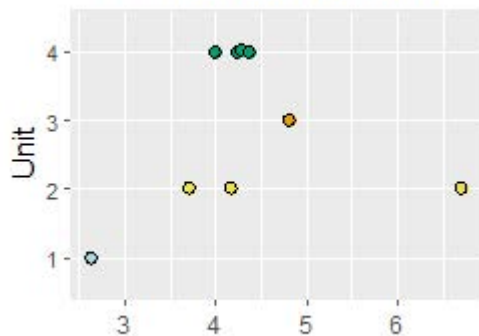
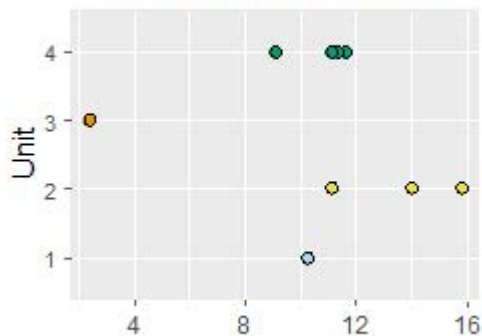
We are not sure what was meant by this comment.

321. I suggest to change ‘stated’ to suggested and ‘are’ to ‘could be’.

Changed accordingly.

365 onwards. Given the relatively wet conditions interpreted are you suggesting that the Holocene was a unique interglacial then, geographically at this position, compared with previous interglacial periods?

Thank you for this valuable comment. There were indeed indications from previous work by Kienast et al. (2007; 10.1016/j.gloplacha.2007.07.004) that the last interglacial was characterised by higher temperatures, longer growing seasons and drier conditions. This fits our findings. We added this information to the discussion: “Our findings point to drier conditions during the last interglacial compared to the Holocene, as well as more bioproductivity and microbial degradation, indicating higher temperatures. This fits nicely to the findings of Kienast et al. (2007).” (L335)

CPI long n -fatty acidsCPI short n -fatty acidsTAR n -fatty acids