Responses to Assoc. Prof. Mgr.Daniel Nyvlt

Dear Assoc. Prof. Mgr. Daniel Nyvlt,

Firstly we would like to thank you for taking the time to review our manuscript entitled "Sediment and carbon accumulation in a glacial lake in Chukotka (Arctic Siberia) during the late Pleistocene and Holocene: Combining hydroacoustic profiling and down-core analyses" in detail. We are especially grateful for all your comments and suggestions particularly during this time of the global pandemic. We appreciate highly that you consider the scientific significance and importance of our manuscript for increasing our understanding of carbon storage in Arctic glacial lake systems. In the following response, we provide detailed replies to each individual comment and provide our proposed changes and adjustments to the manuscript that will be carried out and shown within the revised manuscript version. As such your comments are highlighted in black and *italicised* and our replies are highlighted in blue. We hope that are you satisfied with our replies and our proposed changes!

Thank you again once for taking the time to review manuscript,

On behalf of all the authors,

Stuart Andrew Vyse

Reviewer comments and author responses

It should be noted the the MIS1 starts at 14.7 ka in the marine isotope stratigraphy. Therefore your "Mid-to-Late MIS2" should read "Mid MIS2-early MIS1"

Thank you very much for noting this. We are sorry for the incorrect usage of the marine isotope stratigraphy. We will adapt the phrasing throughout the manuscript so that it reflects your comment to "**Mid MIS2-early MIS1**" within our revised manuscript version. We will also alter the position of the boundary between LU-II and LU-I following your suggestions later within the comments and hence the age ranges will be changed accordingly.

Lakes act also as sinks of atmospheric deposition, which is not necessarily of a material derived from its catchment...

We agree with this and the importance of aeolian deposition that can be derived from further afield than the lake catchment area. We will account for this by rewording and adding in extra reference to this in line 40 so that it reads as follows: "Lakes act as sinks of clastic sediment derived from local catchment weathering processes **as well as from atmospheric deposition** and as such gradually accumulate sediment mass over time (**Dietze et al., 2014**; Hinderer and Einsele, 2001). We will add an appropriate reference to **Dietze et al., 2014** that considers aeolian processes within lake sediments from the Tibetan plateau.

important for what?

Sorry for the poor wording. "Important" will be replaced with "environmentally sensitive" so that line 55 reads as follows: "The region of Chukotka (Arctic Siberia) represents an

environmentally sensitive area with limited lacustrine environmental reconstructions (Lozhkin and Anderson, 2013)".

do not use however in two subsequent sentences...

Sorry for this. We will remove the "however" and subsequent comma from line 71 to avoid double occurrences. Lines 69 to 72 will thus read as follows: "The reconstruction of accumulation rates in these syntheses has however been avoided due to significant reworking of carbon material within permafrost landscapes (Strunk et al., 2020; Windirsch et al., 2020). The role of Arctic Siberian glacial lakes as sediment and carbon sinks has not yet been accounted for."

V-shaped valleys are generally considered fluvial in origin, glacially eroded valleys are described as U-shaped valleys. You should probably better describe this to avoid any confusion. It looks as a U-shaped valley in the Fig 1a.

We agree with this. The valley is in fact U-shaped and not V-shaped as initially written and thus we accept your suggestion and will change this to a "U-shaped valley" in line 114. The line will be changed to read as follows: "Lake Rauchuagytgyn (67.7922° N, 168.7312° E) is situated within the glacially eroded **U-shaped**, Rauchua mountain valley...".

...Cretaceous extrusive and intrusive igneous rocks consisting of silicic-intermediate lithologies by andesite...

...to have it in English

Thank you for making this small change here. We accept the suggestion and will change line 122 to read as follows: "The bedrock surrounding the lake and within the catchment is predominantly composed of cretaceous extrusive and intrusive igneous **rocks** consisting of silicic-intermediate lithologies dominated by Andesite (Zhuravlev and Kazymin, 1999)".

"moraines", rather than "moraine structures"

Thank you for the suggestion. We will change it to read "moraines" so that line 124 reads as follows: "Catchment evidence for glaciation includes **moraines** to the north of the lake that denote the maximum extent of glaciation".

...average July and January temperatures of 13 °C and -30 °C, respectively...

We will add in "respectively" so that line 126 reads as follows: "The Arctic continental climate of the area is characterized by mean annual temperatures of -11.8 °C and average July and January temperatures of 13 °C and -30 °C, **respectively** with low annual precipitation of ca. 200 mm (Menne et al., 2012)."

It would be very helpful to add glacial cirques in the Figure 1a

Thank you for this suggestion. We agree that Figure 1a would benefit from the addition of some extra information regarding the position of glacial cirques alluded to in section 2. We will now add some of the most clearly identifiable glacial cirques from satellite data to the map in the revised manuscript version to account for this. In addition, we will add reference again to Figure 1a in line 125 where glacial cirques are mentioned.

In inset is shown the situation of Lake Rauchuagytgyn (1) compared to other studied regional lakes: (2) Lake Ilirney and (3) Lake El´gygytgyn (ESRI 2020).

We agree with your suggestion and will alter the caption text to read "**In Inset is shown** the situation of Lake Rauchuagytgyn (1) compared to other studied regional lakes: (2) Lake Ilirney and (3) Lake El´gygytgyn (ESRI 2020)."

Enlarged orthophoto map of the lake...

We will change the caption text to integrate this change. The revised text will read as follows: "**Orthophoto map** of the lake and surrounding features."

Simplified bathymetric map...

Thank you for the comment. This comment is however no longer relevant as we will remove the older bathymetric map (Figure 1c) in response to your suggestions in the following comments which replace this with an overview polygon with hydroacoustic profiles.

Could you please add the hydroacoustic profiles paths in any of the detail map? I think the map in Fig. 1c could be enlarged in profiles could be included without the detailed relief of the lake surroundings.

Thanks for the comment. We show the hydroacoustic profiles when presenting the interpolation results in figure 3b but we will modify figure 1 in response to your suggestion. As such we will remove the bathymetry and the relief of the lake surroundings that was presented in figure 1c. In the place of figure 1c, we will show an enlarged lake polygon with the plotted hydroacoustic profiles. We will also alter the figure caption to mirror this to "**(c) Lake polygon with hydroacoustic profiles retrieved during 2018**".

Why do you present an older bathymetry here when bathymetry is one of your main findings in thsi study. I find this unnecessary.

This comment has been acknowledged in the responses above. We will remove this bathymetric map as you are correct that it is unnecessary to have an older bathymetric map. We will replace the older bathymetric map with a lake polygon with plotted hydroacoustic profiles in figure 1c.

show the profiles paths in the map

This comment is acknowledged above in previous responses and will be plotted on a lake polygon in the revised manuscript version as Figure 1c.

What do you mean by the basal sediment within the basin? I think they might be the Pleistocene pre-lacustrine sediments of ?glacial, or ?(glacio)fluvial origin - it would be helpful to desribe it better.

Thank you for this comment. We meant the lowermost deposited sediments within the basin which are likely to be of mixed glacial and glaciofluvial origin. Though the core penetration was very limited into these sediments and it is difficult to say based on the very limited sedimentological data for certain what these sediments are and their age. To acknowledge this comment we will alter the description to "Further core penetration and retrieval was prevented by sand-pebble material at the core base most likely representing the **lowermost deposited sediments** within the basin.

How large the samples for radiocarbon were from the viewpoint of the core depth? It seems they were sampled as 0.5 cm thick what I guess from the Table 1, but I think this should be also stated here.

We agree with your suggestion. Indeed the samples were taken in 0.5 cm thick slices to retain as high a dating resolution as possible. We will now include a remark to this in the manuscript text to read as follows: "Due to the lack of suitable plant remains and low organic content of the retrieved sediment core, 25 bulk sediment samples **(0.5 cm thickness)** and one surface sample (0–0.5 cm **sediment depth**) were dated for radiocarbon....".

Why these two samples were not used for age-depth modelling? I see the same possible way in deleting the samples 3002 and 3003, or even only the sample 3002. Please explain better what reasons you have for ommiting these two samples. It clearly shows a higher sedimentation rate in this part of the lacustrine succession.

Thank you for your comment and input here. We found during the development of the age-depth model that including the two samples or even just sample AWI - 3001.1.1 would produce a sedimentation rate that was unreasonably high. Considering the nature of the sediment deposited during this interval, our sediment core yields limited sedimentological evidence for a drastically higher sedimentation rate across these depths, such as a turbidite event. Thus, we opted for a model that would - based on sediment characteristics - most likely represent a more realistic sedimentation rate for these depths. Moreover, it has been commonly found among Arctic lakes that input of older organic material influences more strongly radiocarbon reliability than younger ages, which supports the exclusion of at least sample AWI - 3002.1.1 from the age-depth modelling (Bronk Ramsey, 2008; Gaglioti et al., 2014; Abbott and Stafford, 1996).

In response to your comment, we have included an extra explanation between lines 162 and 165 as follows: "For modelling, we used 23 bulk sediment samples. Two samples (Lab-ID: AWI - 3001.1.1; AWI - 3002.1.1) were slightly older than their successive dates further down. This suggests possible reworking in these depths (81.25 cm and 114.75 cm, respectively) **and would lead to unrealistically high sedimentation rates when included within age-depth modelling that is not mirrored by sedimentological proxies**. We thus treated these two dates as outliers and excluded them from the modelling process."

Mn and Fe are also rock-forming elements...

Thank you for pointing this out. We will restructure lines 183 and 184 to read more efficiently as follows: "The elements (Aluminium (Al), Silicon (Si), Calcium (Ca), Potassium (K), Titanium (Ti), Manganese (Mn), Iron (Fe), Bromine (Br), Rubidium (Rb), Strontium (Sr), Zircon (Zr)) were selected for further processing".

?element proportions

We have now removed this sentence and hence this comment is no longer applicable. Based on suggestions from reviewer #1 we use instead the simple, non log-transformed elemental ratios which show the same patterns as the log-transformed ratios.

The boundary of 4 micrometers is used in most sedimentological literature for the clay/silt boundary. The 2 microns boundary is more common in geochemical studies.

Thank you for pointing this out. Indeed Udden (1914) and Wentworth (1922) put the division between clay and silt at 4 micrometers but later studies such as Friedman and Sanders (1978) set the boundary between clay and silt instead at 2 micrometers. As we have used the very commonly (cited 1928 times) used software "Gradistat" from Blott and Pye (2001) to process our grain-size data that utilizes the Friedman and Sanders (1978) boundary at 2 micrometers, we will opt to retain our clay/silt boundary here. We will add an additional citation to the grain-size classification of Friedman and Sanders (1978) to line 202 so that it reads as follows: "Intervals of 2 mm–63 µm, 63–2 µm and <2 µm, were used to define percentages of sand, silt, and clay respectively (**Friedman and Sanders, 1978**)".

The Folk and Ward method... upper case in names

Sorry for this. We will now capitalize these letters here. Line 202 will now read as follows: "The **F**olk and **W**ard method was used for mean grain-size calculation."

et al.

Thank you for noticing this. It will be corrected to in the revised manuscript version Avnimelech **et al.** (2001).

Do not start a sentence with a number. e.g.: Subsequently, 65 dried and milled...

We agree with your comment here and change line 214 to read as follows: "Subsequently, 65 dried and milled.....".

delete 3x the commas after al.

Thank you for seeing this. The three commas will be deleted after the al. in line 245.

Is the term shelf correct when referring to the lake? I would prefer the describe it as submerged paleoterrace.

Thank you for this comment. The term "shelf" has been used within published literature to describe similar features at other Arctic glacial lake sites. Examples may be observed in Lebas et al. 2019 "Seismic stratigraphical record of Lake Levinson-Lessing, Taymyr Peninsula: evidence for ice-sheet dynamics and lake-level fluctuations since the Early Weichselian" where similar terminology was utilised in reference to hydroacoustic and seismic data. We will thus opt to retain the term "shelf" here.

Please unify the use of Birtish vs. American English. The text is written in American English, but in the Figure appear "Palaeoterrace", which is written in British English.

Sorry for the mixed usage of British and American English within this manuscript. We will thoroughly check the manuscript for discrepancies existing between British and American English and the revised manuscript will be written only in British English with all discrepancies corrected.

northern-shelf - see my comment above

This comment is addressed in a previous response. As such we will retain the usage of "shelf" as it has been used to describe similar features within other Arctic glacial lake systems (see Lebas et al. 2019).

shelf - see my comment above

This comment is addressed in a previous response. As such we will retain the usage of "shelf" as it has been used to describe similar features within other Arctic glacial lake systems (see Lebas et al. 2019).

I would avoid using the term hummocks here, as hummocky-cross stratification/bedding is a sedimentary feature, which leads to a sediment morphology, rather than to an erosional landform as is the case here. Use evelations and depressions as not genetically-bounded terms here.

Thank you for this suggestion. We agree that this could be a confusing. We will instead opt to use the proposed terminology of "elevations and depressions" that you suggest. Lines 297-298 will thus read as follows: "AU2 possesses a volume of ca. 18055352 m^3 (0.018 km^3) with complex internal architecture with **elevation and depression-like** structures."

kilometers - either "km", or "kilometers" (lower case initial) - all parts of the figure

Thank you for pointing this out. We will now correct all the upper case initials to lower case initials in figure 3 within the revised manuscript version and check that this is the case in all other figures.

The same thickness scale in Figs 3b, c, d would help to visually see the differences. Now the pink is 6-7 m in 3b, 8-9,5 m in 3c and 13-15 m in 3d - this is not good for visual comparison.

Thank you for this hint to improve the readability of figure 3. We accept your suggestions and will adopt this in the new revised version of Figure 3. We will adjust this so that each interval is adapted to 1 m rather than the scale differences that you have noted.

the artifacts are coloured in white, which is also the colour for the most thick sequence in Fig. 3c and 3b. I think using grey colour for the gaps (artifacts) would be better, as this colour is out of the scale used.

Thank you for noticing this. We agree that is a little bit confusing within the figures and thus we agree with your suggestion and will change the colour of the artifacts to grey to account for this in the revised manuscript version.

these figures show that the delta at the inflow is rather of Pleistocene origin, but how you know that it is of Pleistocene age as both parts (Pleistocene and Holocene) were not delimited in this part of the lake basin as shown in Fig. 2b.

Thank you for this comment. It is difficult from the presented data to know the true age of the delta and hence we do not assign a strict age to the delta. We only know that processes have been operating at the delta during the Holocene due to the observance of active fluvial channels and coarser-grained surface sediments proximal to the delta that evidence input of fluvial material. What would certainly help us to know more information about the age of the delta would be reduced acoustic blanking in this area and perhaps the usage of another method such as seismic stratigraphy and the retrieval of cores through the delta sediments. Unfortunately, acoustic blanking, no additional seismic data, and no cores from this region hinder further interpretation as to the age of the delta here.

shelf - see my comment above

This comment is addressed in previous responses. As such we will retain the usage of shelf as it has been used to describe similar features within other Arctic glacial lake systems (see Lebas et al., 2019).

Is the age of 29 cal. ka BP modelled, or calibrated - I see this is a calibrated age of the lowermost sample - why do you mention modelling here? And how does Elias and Brigham-Grette, 2013 refer to this information? This is simply an inappropriate reference here, as the age is part of your results! Besides, references should not be used in the Results chapter, as it should contain your results and no reference are needed here! Some journals even prohibit the use of references in the Results chapter.

Thank you for pointing out this lack of clarity. It is common in the age-depth modelling domain that both calibrated radiocarbon samples and modeled ages are expressed as "calibrated years BP", in our case "cal. ka BP". Age determination samples are calibrated internally in the age-depth modelling software and hence the depth in between those samples are regarded as calibrated as well.

As stated in the current manuscript version we used the age-depth modelling software "Undatable" (Lougheed & Obrochta, 2019). In this software the lowermost point in the age-depth model does correspond with lowermost radiocarbon sample we took. Hence, we say "modeled" to emphasize that we derived the value from the age-depth modelling software instead of a separate calibration program, i.e. CALIB 8.2 (Stuiver et al., 2020).

Sorry for the inappropriate references here. We agree that results should not contain references wherever possible. We will subsequently move the references (Abbott & Stafford, 1996; Björck & Wohlfarth 2002) to the discussion section 5.2.3 where we will add some extra lines from lines 716 to 720 to consider possible reworking to read as follows: "from empirical equations of DBD and carbon content where discrete, volumetric measurements do not exist (Avnimelech et al., 2001; Kastowski et al., 2011) **as well as** varied approaches used for the measurement of sample carbon contents (Elemental analyser vs LOI) (Munroe and Brencher, 2019). **Limitations associated with radiocarbon dating of Arctic glacial lakes due to the absence of appreciable amounts of datable organic material as well as the influence of reworking processes associated with permafrost and glacial processes can also lead to uncertainty with regards to actual sediment ages and hence reconstructed accumulation rates (Abbott & Stafford, 1996; Björck & Wohlfarth 2002).**" We will also remove the reference to Elias and Brigham-Grette, 2013 from line 306 and also check for further uses of references in the results chapter that will be subsequently removed.

See my comment above regarding the two samples omitted from the age-depth model calculation.

Thank you for the comment. We have addressed this comment in a previous response. We excluded these samples on the basis of an older age of sample 3002 relative to lower samples that suggested reworking and input of old carbon. Moreover, the inclusion of samples 3002 and 3001 within age-depth modelling would produce unrealistically high sedimentation rates that would not be reconcilable from the sedimentological data that shows no evidence of a drastic increase in sedimentation rate in these depths. We have thus made changes to lines 162 to 165 to account for your suggestions.

Isn't the larger age scatter in Pleistocene samples (and age model), when compared with Holocene ages, connected with a larger age scatter after radiocarbon age calibration, because of larger uncertainties and less data for calibration curve calculation?

Thank you for your question. We are using the newest published calibration curve (IntCal 2020) in this study that was published by Reimer et al. 2020 and hence the calibration curve represents the most recent state-of-the-art dataset with reduced uncertainties compared with previous calibration curves. Moreover, the uncalibrated years already show scatter and therefore the calibration curve likely has little influence. It is more likely that the scatter is related to lake development processes possibly related to the presence of a catchment glacier within this interval. We have taken account of this scatter by including sigma ranges in sedimentation rate calculations.

a comma before but - "...low rates, but with..."

Thanks for pointing this grammatical mistake out. Line 315 will now be corrected to the following "demonstrates **low rates, but with** larger uncertainty".

"...uncertainty ranges (dark and light grey ribbons)." 1sigma is dark grey 2sigma is lighter grey

Thank you for noticing this. We will adopt your change so that the caption of figure 4 reads as follows: "1 and 2σ uncertainty ranges (**dark and light** grey ribbons)."

What do you mean by: Br/Al ration values demostrate their lowest values at any depth? It clearly has the lowest values in LU-III, not at any depth...

Sorry for this confusing sentence structure here. We agree with your suggestion and will change line 336 and 337 to read as follows: "Br/Al ratio values demonstrate their lowest values **within LU-III** alongside TC.....".

"cal. ka BP" - lowercase cal.

Thank you for noticing this. We will correct to the lowercase **cal.** as you suggest. We will also check occurrences throughout the manuscript to ensure "cal." is written in lowercase.

The Grain-size box should be logically made from the left by clay, then silt in the middle and sand to the right - it is very erroneous to put sand between clay and silt. Why the terms initiate with upper cases? - this is inappropriate here.

We agree with your comment and feel that the current presentation is not logical. We will follow your suggestion by altering figure 5 to show clay, silt, and sand in that order as suggested. The starting letters of the terms displayed in figure 5 will also be altered to lower case as suggested.

What do you mean by high grain-size? Is a coarse grain-size?

Sorry, here we meant "coarser mean grain-size". We will subsequently alter the figure caption text of figure 5. to the following "mean grain-size plot refers to three excluded data points (650.5, 341, 321 cm) of **coarser mean grain-size** (up to 25 µm)."

"cal. ka BP" - lowercase cal.

Thank you for noticing this. We will correct the lowercase cal. in Figure 6 as you suggest as was also the case for Figure 5. Again, we will also check this throughout the manuscript.

Why is the TOC displeyed twice? I do not see the need to show it in wt% and retain only the g OC cm-3

Thank you for the comment. We agree with you that we do not need to present TOC twice within figure 6. Removal of one of the TOC curves will also improve the overall readability of Figure 6. We decide in this instance to remove the TOC curve in g OC cm^{-3} as the wt% curve displays both the TC and TOC curves and is more traditionally presented within palaeolimnological studies.

Basing on all graphs I have seen, I would put the boundary between LU-II and LU-I slightly lower!!! It is impossible to put the boundary at the peak of TOC (and some toher proxies), I strongly recommend to put this boundary slightly lower to have the peak already in LU-I, not at the boundary and it will probably also fit better with the beginning of the Holocene (11.7 ka b2k, rather than your 11.5 ka BP). I would put the boundary in the mid-point of the PC1 score rapis increase, if it fits with lithological data. Please think about this change!

We thank you for this important suggestion and we agree with it. We will move the LU-II and LU-I boundary to the suggested mid-point of the rapid increase in PC1 scores at a depth of **346** cm that actually corresponds to an age of **11.69** cal. ka BP and hence more effectively represents the Holocene start. We will then correct this boundary in all figures and text passages that show and/or refer to this boundary to accommodate for this change throughout the manuscript. Thank you again for making this suggestion.

sand, silt, clay, mean GS - lower cases - both in text and in Figure 7

Sorry for the capitalization here. We will now change the first letters to lower case as per your comment in both the text in line 388 and in Figure 7.

What is Early MIS 2, Late glacial and Holocene and how it relates to LU-I, LU-II, or LU-III units? Please explain it better in figure caption.

Thank you for this comment. We will change the terms within figure 7 to LU-I, LU-II, and LU-III as the grouping is based on the lithological unit definition. We will slightly modify the caption of figure 7 to read as follows: "Principal component analysis **(PCA)** biplot of sedimentological, biogeochemical and accumulation rate data from core EN18218. **Samples are coloured and clustered according to their lithological unit definitions (LU-III, LU-II, LU-I).**"

palaeo - British vs. American English

Sorry for this. As we have mentioned previously within our responses, we will check the use of British vs American English throughout the manuscript and ensure that the manuscript is standardized in British English throughout.

This is incorrect use of a reference. Mangerun and Svendsen worked on Svalbard - how this relates to Chukotka? You should rather use here "(cf. Mangerud & Svansen, 1990)."

Sorry for this reference error. We agree with your suggestion and will use instead (**cf**. Mangerund & Svendsen, 1990) in line 397. We also follow all suggestions of reference changes to (cf.) notation where suggested.

rather "basal sediments" than "basement sediments"

Thank you for this suggestion. We will now refer to “basal” sediments within the revised manuscript version.

Again, the term "basement" is mostly used for solid rocks, rather than for sediments by geologists.

Ok, we will refer instead to “basal structures” in the revised manuscript version.

again - (cf. Lebas et al, 2019; Lebas et al., 2021).

We agree with your comment and will use now (**cf.** Lebas et al., 2019; Lebas et al., 2021) for the references in line 404.

Could you explain how can a glacigenic sediment be layered? Glacigenic sediments are those deposited directly from glacier without subsequent sorting, which means tills and tills are hardly layered. But glaciofluvial and glaciolacustrine sediments could be layered. Please make this clear.

Sorry, we understand that the uppermost portion of section 5.1.1 may be unclear as currently written. We will make adjustments to this section in order to address your comments here that will be included within the revised manuscript version.

Firstly, we will no longer refer to these sediments as “glacigenic”. You are correct that we see some layering within this unit that most likely relates to deposition under glacio-lacustrine conditions with input of sediments derived through catchment glacial activity. Our interpretation of this unit is based on similar published findings at Harding Lake presented by Finkenbinder et al. (2014) as well as glacio-lacustrine sediments deposited in Lake Silvaplana by Leemann and Niessen, 1994b. At Harding lake, following deposition of a basal pebble-diamicton prior to ca. 30.7 cal. ka BP at the base of unit 1, which was tentatively interpreted to represent braided river sediments at Harding lake, sedimentation from ca. 30.7 cal. ka BP to 15 cal. ka BP throughout the rest of unit 1 consisted of finer-grained deposition of highly minerogenic sediment of high magnetic susceptibility, high and variable dry bulk density (of very similar values represented for much of LU-III at lake Rauchuagytgyn), and an abundance of the element Titanium. These sediments were interpreted to represent lacustrine sediments deposited continuously within a perenially ice-covered lake that may have been shallow with enhanced minerogenic sediment input during the global last glacial maximum. Moreover, sedimentation rates within unit 1 at Harding Lake displayed comparable values to those calculated for LU-III at lake Rauchuagytgyn of ca. 0.01 cm/yr.

In response to these sediments being layered, studies of glacially influenced layered glacio-lacustrine sediments from Lake Silvaplana by Leemann and Niessen, 1994b have shown that catchment glaciation can lead to the synchronous deposition of fine-grained (clay & silt) suspension load within layers. This may be a similar mechanism to that acting at lake Rauchuagytgyn during this interval. Karlen and Matthews (1992) have similarly reported the occurrence of silt/clay bands found within glacio-lacustrine sediments from southern Norway that possessed low organic content with high magnetic susceptibility that likely represent

minerogenic sediment bands associated with catchment glaciation. Moreover, van der Bilt et al. 2015 investigated sediments deposited in a lacustrine setting in Svalbard and suggested that silt and clay sediments of high bulk density, magnetic susceptibility, and titanium concentration represented glacial suspended load that could be used to identify periods of glacial advance. Sediments that were associated with glacigenic suspended load for example in unit 1 of van der Bilt et al. (2015), demonstrated layering in the form of centimeter scale lamination.

The scatter in radiocarbon ages reported for LU-III may also relate to scatter caused by glacial reworking of organic material from the lake catchment that has been noted at other glacial lake sites during glaciation. Based on this, it is our interpretation that the sediments deposited within LU-III could represent glacio-lacustrine sediments that were influenced by the input of minerogenic glacial suspended load in the form of silt and clay from a catchment glacier.

In the revised manuscript version, we will thus make this distinction clearer and refer to "glacio-lacustrine" deposition as well as "minerogenic glacial suspended load" and include additional references to support our interpretations along with some restructuring to account for your comments.

How do you know that these are glacigenic sediments? Sediments and their facies should be described based on their texture and structure, not based on colour, elemental composition, or PCA. You should be more cautious when using sedimentological term without knowing the sedimentology of the material!

Thank you very much for your comment here, we have partially considered this comment in the previous response. We meant to refer to suspended sediments that have been derived through glacial processes acting within the lake catchment. In general, we originally meant to use the term “glacigenic” to refer to minerogenic glacial suspended load deposited within a glacio-lacustrine setting which is generally dominated by clay and silt and characterised by a high bulk density as has been described by Leeman and Niessen, 1994b, Bakke et al. (2005) and van der Bilt et al. (2015). Van der Bilt et al. (2015) for example, referred to glacigenic sediment when discussing the input of clay and silt recorded by lacustrine sediment cores in Svalbard. Moreover, Van der Bilt et al. (2015) also used multivariate statistical approaches (PCA) to effectively “fingerprint” the sedimentological and geochemical nature of glacigenic sediment in a similar manner to that what we have carried out in this manuscript. As mentioned in the previous response, studies of glacio-lacustrine sediments from southern Norway have also suggested that sediment bands of silt/clay composition with high magnetic susceptibility represent glacigenic sediment deposited within a lacustrine environment (Karlen and Matthews, 1992).

again, Van der Bilt et al., 2015 did not work on Chukotka, so "cf. Van der Bilt et al., 2015" would be better to refer to.

Thank you for pointing this out. We will now refer to (**cf.** Van der Bilt et al., 2015).

The principal question is - was there a lake at that time? Probably not, as you wrote about glacigenic sediments. So, why you write about lake here? It is all about the sound interpretation of individual sedimentary facies described in the section, without that it is very hard to follow your story!

Sorry for this lack of clarity. We will now carry out changes to this section to make our story clearer as mentioned in the previous few responses. It is our opinion that apart from the coarse-grained basal sediments, that LU-III predominantly represents glacio-lacustrine sedimentation that was influenced by the input of minerogenic glacial suspended load from a catchment glacier. The lake was likely also covered with a quasi-permanent surface ice layer that acted to reduce sediment input and shares similarities to continuous lacustrine deposition that was interpreted for unit 1 at Harding Lake in Alaska (Finkenbinder et al., 2014).

Which of the two Kokorowski et al. 2008 this is? They should be refered to as Kokorowski et al. 2008a and 2008 b both in the text and in the list of references.

Sorry for this. We meant to refer to Kokorowski et al. 2008**a** in this case. We will ensure that at each reference to Kokorowski et al. 2008, the correct **a** and **b** notation is added throughout the manuscript. We will also add this notation to the references of both Kokorowski et al. 2008 papers within the references section in lines 1063 and 1066.

?or in line with non-lacustrine environment?

Thank you for this comment. We have addressed this comment in previous responses addressing section the upper part of section 5.1.1. We envisage a glacio-lacustrine environment for much of LU-III.

again - "cf. Baumer et al., 2020;..."

Ok, thank you for pointing this out. We will now alter the references here to **cf.** i.e. (**cf.** Baumer et al., 2020; Biskaborn et al., 2019; Fritz et al., 2018; Heinecke et al., 2017; Naeher et al., 2013).

Is really Zr/K a proxy to coarse-grained lithology? What do you mean by "coarse" here? For most geoscientists coarse would mean gravel. Generally, all facies described in this study are rather fine-grained. Therefore, you might explain it better here. Generally, the Zr/K index increases with an increase of coarse silt to medium sand fraction, what we found in numerous our studies from Central Europe, Svalbard, Greenland, or Antarctic Peninsula. For detection of even coarses grains (fine sand to fine gravel), the Si/Al index is used, as quartz is a predominant in sand to fine gravel fraction and aluminosilicates are common in clay to medium silt fractions.

Thank you for this comment. We agree that the sediment deposited within the lake is generally fine-grained in nature and should not be referred to as "coarse". In the revised manuscript we will be more cautious with the application of "coarse" and will subsequently alter lines 425 and 426 to read as follows: "**Finer** grain-sizes directly measured by laser diffraction are supported by indirect, XRF-derived grain-size proxies **for finer**, clay-dominated sediment (K/Ti)." In our context, we meant the coarsening of the fine-grained sediments that is represented by the Zr/K ratio and likely reflects the increased contribution of coarse silts and sand fractions to the grain-size signal during the Holocene. The Zr/K ratio as you said, is likely related to the increase in the proportions of coarse silt and very coarse silt and some sand fractions that is observed within Holocene sediments. Equally if the Si/Al ratio is used as a grain-size proxy we see increasing values of Si/Al within Holocene sediments that may also be related to grain-size increases. Si/Al may however relate additionally to biogenic sediment due to the occurrence of diatoms within the lake sediments.

Harding Lake

Sorry for this spelling mistake in line 436. We will follow your suggestion and change to "Harding Lake". We will also check for further misspellings of "Lake" throughout the manuscript.

Is really 0.15 mm a-1 a low sedimentation rates? It equals to 15 cm/ka, which is higher that what is described above. It would be helpful to show comparison to Holocene sedimentary rates in Tajikistan.

Thank you for pointing this out. Sorry, we meant to say lower Pleistocene sedimentation rates in comparison to the Holocene at Lake Karakul. We will alter the phrasing of these lines to make this clearer and also add in the Holocene sedimentation rate for comparison as suggested. Line 440 will now read as follows: "A similar finding at lake Karakul, Tajikistan of **lower** sedimentation rates during MIS2 **since ca. 29 cal. ka BP** (0.15 mm a^{-1}) **when compared with Holocene sedimentation rates (0.84 mm a^{-1})** was also explained by reduced sediment input during MIS2 compared to the Holocene alongside reduced organic matter accumulation (Heinecke et al., 2017)".

Rauchuagytgyn

Sorry for the spelling mistake. This will be corrected in line 442 and will be checked for correctness throughout the rest of the manuscript.

Late Glacial

Sorry for the spelling mistake here too. Will be changed in line 443 and checked throughout the manuscript.

Last Glacial Maximum

We apologize again for the spelling mistake here. It will be changed in the revised manuscript version in line 446 and checked throughout the manuscript.

"glacial erosion", rather than "glacial denudation"

Thank you for this suggestion. We agree and will change "denudation" in line 449 to "**erosion**" in the revised manuscript.

When was the glacier 25 km long? This is important to describe it here. If it was during the LGM than the early MIS2 facies are very probably glacigenic and not lacustrine. This is what is not well described and proven in the entire manuscript.

Thank you for this comment. According to Glushkova, 2011, the glacier was likely a length of ca. 25 km and was suggested to have been so during the Sartan glaciation which is synonymous with glaciation during MIS2. This was somewhat of a relative approach, as no absolute dating methodologies were utilized and up to now, have not been available. Some dating methods, for example using cosmogenic nuclides on glacial features in the catchment, may provide some additional information regarding the timing, but is unfortunately not available for the Rauchua river valley. We will follow your suggestion and add this information in the revised manuscript version so that lines 454 to 457 read as follows: "Remote sensing based studies of Chukotkan glacial geomorphology and structures within the Rauchua valley have suggested that the catchment glacier was likely a passive glacier,

ca. 25 km in length that extended along the length of the Rauchua river valley and discharged into the Rauchuagytgyn basin **during marine isotope stage 2** (Glushkova, 2011)." We have addressed the rest of the comments regarding glacigenic suspended load input to the lake basin in previous responses. We consider sediments deposited within LU-III to be generally of glacio-lacustrine origin.

...not contributed significant sediment volume to the lake...

Thank you for this suggestion. We will alter lines 457 to 459 to read as follows: "Thus, the catchment glacier may have been predominantly non-erosive during the early MIS2 and **not contributed significant sediment volume** to the lake basin supporting the low rates discussed here (Gurnell et al., 1996).

See my comment in the abstract for the timing of MIS2

Thank you. We have noted your comment within the abstract and have agreed with your suggestion and will alter this throughout the manuscript to "**Mid MIS2-early MIS1**". The discussion title in line 472 will now be altered to "5.1.2 **Mid MIS2- early MIS1** accumulation during progressive climate amelioration (ca. 23.4–11.69 cal. ka BP)". We will also check for more occurrences within the manuscript and adjust accordingly.

(cf. Lebas et al., 2021;...)

Sorry for this. We will now correct the reference to (**cf.** Lebas et al., 2021) in line 477.

I do not agree completely with this! Alexis Dreimanis made already 80 years ago pioneering studies (summarised e.g. in Dreimanis and Vagners 1971 In: Goldthwait RP, ed.: Till, a Symposium, Ohio State University Press) of what terminal grade (grain-size) are produced by glacial grinding and milling and found for most minerals that the final granolometry is in fine to middle silt fraction. Clay-sized grains are assumed to be sourced from clay minerals within the bedrock only (Haldorsen 1983 Norsk Geologisk Tidsskrift), but in principle most of the glacially derived material lies in silt fraction (e.g. Haldorsen 1981 Boreas). This means that rock flour is principally silt-sized. Besides, when applied 2 microns as the boundary between clay and silt then most of the glacially grinded and milled terminal grades would terminate in silt fraction, i.e. >2 micrometers.

Thank you for your comment. We appreciate your input on the interpretation here and we recognize that these studies have suggested that rock flour is principally silt-sized. Despite this, it has been recognized more recently from the exceptional, ca. 155 ka Owens Lake record, that glacially produced rock-flour can be found within the clay-size fraction (Bischoff et al. 1997). To account for your comment, we will alter lines 477 to 480 to read as follows: "A clay maximum and grain size minimum at ca. 550 cm (ca. 22.7 cal. ka BP) may suggest initial increases in lake water-depth through glacial melt additions that could have led to the observed reduced SRs and MARs this time. This may be supported by the high values of K/Ti (clay contribution) and low values of Zr/K (proxy for coarser grain-sizes) (**Kilian et al., 2013**; Kříbek et al., 2017; Cuven et al., 2010)." We will also include an additional reference of K/Ti as a proxy for clay contribution (**Kilian et al., 2013**).

This may be supported...

We will change this in line 480 in the revised manuscript version.

...controlled by inflowing rivers...

Thank you for noticing. We will alter this in line 490 in the revised manuscript.

Late Glacial

Sorry for the misspelling. This will be corrected in line 506 in the revised version.

Late Glacial

Sorry for the misspelling. This will be corrected in line 510 in the revised version.

What do you mean by "small grain-size fining"?

Sorry for this poor wording. We meant a small reduction in sediment mean grain-size. We will subsequently adopt this wording to make it clearer so that line 519 reads as follows: "A reduction in accumulation rates ca. 12.6 to ca. 11.5 cal. ka BP, broadly associated with **a small reduction in sediment mean grain-size.....**".

Younger Dryas

Thank you for noticing the lack of capitalization of "Younger". It will be corrected for in the revised manuscript version.

This sentence does not provide any real information - what do you mean by a "more limited Younger Dryas event"? Is it meant as a glacial event, a climatic event - clarify this!

Thank you for this comment. We will phrase this differently in the revised manuscript version and will remove the "more limited Younger Dryas event" as it does not add any real information as you stated. We have also corrected for the Kokorowski paper as we were referring to the Kokorowski et al. 2008**b** paper here. As such lines 523 and 524 now read as follows: "These findings are consistent with recent regional and transregional records that suggest a **spatially variable Y**ounger **D**ryas **climatic event** in **F**ar and **E**ast Russia and parts of **E**astern Beringia (Anderson & Lozhkin, 2015; Kokorowski et al., 2008b; Lozhkin & Anderson, 2013; Lozhkin et al., 2018)".

Younger Dryas, Far and East Russia, Eastern Beringia

Sorry for these misspellings. They will be corrected in the revised manuscript version and have been addressed in the response to the previous comment. We will also check for correct capitalization of these words throughout the manuscript.

So, why you have put the boundary between LU-II and LU-I to the higher TOC value? Because the organic proxy values are decreasing now, as the highest value is at the boundary.

Thank you for the comment. We have addressed the boundary between LU-II and LU-I in previous responses. The boundary will now be shifted to **346** cm (**11.69** cal. ka BP) and hence the highest TOC will no longer be directly at the boundary between LU-II and LU-I as was previously the case. Please be aware that we will also correct all the ages referring to the previous LU-II and LU-I boundary within section 5.1.3 to account for the movement of the boundary position to 346 cm.

(Figs 5, 6, 7) - no dot after Figs and spaces after comma

Many thanks for pointing this out. We will follow your suggestion and remove the dot and add spaces after each comma in line 529.

...for a local Holocene thermal maximum...

We agree that we should refer to a "Local" Holocene thermal maximum. We thus accept your suggestion and will utilize it in the revised manuscript version so that lines 541 and 542 read as follows: "which show evidence for a **loca**l Holocene thermal maximum ca. 10.6–7 cal. ka BP (Andreev et al., 2021)." We will also include this for all occurrences of "Holocene thermal maximum".

...greater sand proportion may relate... why so complicated?

Sorry for making this sound unnecessarily complicated. We will restructure lines 543 and 544 to make them easier to read by removing "to the grain-size distribution". The lines will thus read as follows: "Increasing early Holocene sediment and mass accumulation rates alongside greater sand contribution may relate to the input of coarser grained fluvial detrital input from a paraglacial....."

Late Glacial

Sorry for the misspelling. This will be corrected in line 547 in the revised version.

Late Glacial

Sorry for the misspelling. This will be corrected in line 551 in the revised version.

Westerlies

Thank you for noticing. We will correct by removing the capitalization in line 554.

Last Glacial Maximum

Will be corrected by capitalization in the revised manuscript in lines 554 and 555.

?finer grain-size

Thank you for the suggestion. We will now alter "lower grain-size values" to "finer grain-size" so that lines 563 and 564 read as follows: "alongside **finer grain-size** may reflect some local environmental change".

Why is the reference given twice in one sentence?

This was a referencing mistake. We will remove the second occurrence of the reference on line 584 (Francke et al., 2013) to correct for this.

?a shortening of the summer open water season.

Thanks for the suggestion. We agree with your wording and will change line 588 to the following "This could tentatively be interpreted to represent a **shortening of the** summer open water season."

...coarser fluvial and alluvial detrital material...

Many thanks for the suggestion. We agree with your suggestion and will adopt the phrasing so that line 594 will read as follows: "paraglacial processes that resulted in an input source of **coarser fluvial and alluvial detrital material** into the southern sub-basin...".

again, Peter Doran have not worked here - cite it as follows: cf. Doran, 1993...

Sorry for this. We will change the citation to your suggestion so that line 595 will read "mass accumulation rates (**cf.** Doran, 1993; Smith and Jol, 1997)."

Figs

Thanks for pointing this out. We will remove the decimal point from line 597 so that it will read as follows: "front (site EN18220) (Fig. 1 & **Figs** S5, S6)". As aforementioned, we will also check the entire manuscript for incidences and correct for these too.

....sediment transport into the deeper...

We accept your suggestion and will change line 608 to read as follows: "This likely represents low deposition due to **sediment transport into the deeper basin** and feasibly.....".

Are really the lakes in Greenland boreal?

Thank you for the question. The use of Boreal should have referred to lakes studied in North America and northern Europe and not to Greenland, where the studied lakes were predominantly of proglacial or bedrock-catchment type (Perren et al., 2009). We will subsequently alter line 616 to read as follows: "Comparisons must therefore be drawn to Boreal lakes from North America and northern Europe, **as well as to proglacial and bedrock-catchment lakes from Greenland**." We will also check the usage of Boreal for Greenlandic lakes throughout the manuscript.

ice free

Thanks for noticing. We will now remove the capitalization in line 627 so that is reads as follows: "accumulation calculated for Finnish Boreal lakes that became **ice free** at the Holocene start".

Great figure to compare boreal to polar northern hemisphere lakes' OCAR!

Thank you very much for this nice comment to figure 8.

yedoma - lower case sediments - lower case

Sorry for the incorrect capitalization within the figure caption of figure 9. We will remove the capitalization in the revised manuscript version so that it reads as follows: ".....Bykovsky thermokarst lagoons and Central Yakutian **yedoma** deposits. Rauchuagytgyn **sediments** possess.....".

I do not think that yedoma and alas are local names, therefore they should be written with lower case initials.

We agree with your suggestion as yedoma and alas refer to permafrost deposits and not to areas. We will hence remove capitalization of "yedoma" and "alas" in lines 662 and 663 so

that they now read "(**yedoma**: 0.057 Mt, 5.27 kg m-3, **alas**: 0.032 Mt, 6.07 Kg m-3)" and check this throughout the manuscript.

kg - lower case

Sorry for this. It will be corrected in the revised manuscript version in line 663.

Aeolian

This will be corrected in line 685 to remove the capitalization.

What about to apply the geomorphological concept of connectivity?

Thank you for this great suggestion. We feel however that applying this concept fully would be beyond the scope of the current manuscript. That being said, we will certainly add a mention of this concept within section 5.2.3 as well as an additional reference to the paper of Singh et al. 2021 "Geomorphic connectivity and its application for understanding landscape complexities: a focus on the hydro-geomorphic systems of India".

The geomorphological concept of connectivity will be mentioned from lines 704 to account for this as follows: "**As geomorphic systems are hierarchical and operate at multiple spatio-temporal scales according to the concept of geomorphic connectivity, the diverse linkages and interrelationships between different catchment components and processes likely plays a large and complex role in regulating sediment and carbon dynamics at lake Rauchuagytgyn (Singh et al., 2021)**".

...future changes in sediment and carbon...

Thank you for noticing this mistake. We will change line 709 to read as follows: "....could lead to future changes **in** sediment and carbon dynamics that are yet.....".

two times within in the sentence - maybe to changes as follows: ...palaeoenvironmental context of a Chukotkan...

We will follow your suggestion and adjust lines 724 and 725 as follows: "This study aimed to improve the understanding of accumulation rates and pools within a palaeoenvironmental context **of a** Chukotkan Arctic glacial lake.....".

See my comment in the abstract for the timing of the MIS2

Thank you for this. We have addressed this in previous responses and will follow your suggestions for this. As such, line 733 will read "**Mid MIS2-early MIS1 accumulation (ca. 23.4–11.69 cal. ka BP)** reflects the increasing influence of paraglacial processes, longer surface ice-free summers, a thickening catchment active layer, and increasing moisture availability. Carbon accumulation increased throughout and accompanied progressive climate amelioration."

References used in this author response

Bakke, J., Lie, øyvind, Nesje, A., Dahl, S.O., Paasche, ø.: Utilizing physical sediment variability in glacier-fed lakes for continuous glacier reconstructions during the Holocene, northern Folgefonna, western Norway, Holocene, 15, 161–176. https://doi.org/10.1191/0959683605hl797rp, 2005.

Bronk Ramsey, C.: Deposition models for chronological records, Quat. Sci. Rev., 27, 42-60, https://doi.org/10.1016/j.quascirev.2007.01.019, 2008.

Dietze, E., Maussion, F., Ahlborn, M., Diekmann, B., Hartmann, K., Henkel, K., Kasper, T., Lockot, G., Opitz, S., and Haberzettl, T.: Sediment transport processes across the Tibetan Plateau inferred from robust grain-size end members in lake sediments, Clim. Past, 10, 91–106, https://doi.org/10.5194/cp-10-91-2014, 2014.

Finkenbinder, M.S., Abbott, M.B., Edwards, M.E., Langdon, C.T., Steinman, B.A., Finney, B.P.: A 31,000 year record of paleoenvironmental and lake-level change from Harding Lake, Alaska, USA, Quat. Sci. Rev., 87, 98–113, https://doi.org/10.1016/j.quascirev.2014.01.005, 2014.

Friedman, G.M. and Sanders, J.E.: Principles of Sedimentology, Wiley, New York, 1978.

Gaglioti, B. V., Mann, D. H., Jones, B. M., Pohlman, J. W., Kunz, M. L., and Wooller, M. J.: Radiocarbon age-offsets in an arctic lake reveal the long-term response of permafrost carbon to climate change, J. Geophys. Res. Biogeosciences, 119, 1630–1651, https://doi.org/10.1002/2014JG002688, 2014.

Karlen, W and Matthews, J.A.: Reconstructing Holocene Glacier Variations from Glacial Lake Sediments: Studies from Nordvestlandet and Jostedalsbreen-Jotunheimen, Southern Norway, Geografiska Annaler. Series A, Physical Geography, 74, 327–348, 1992.

Kilian, R., Baeza, O., Breuer, S., Ríos, F., Arz, H., Lamy, F., Wirtz, J., Baque, D., Korf, P., Kremer, K., Ríos, C., Mutschke, E., Simon, M., De Pol-Holz, R., Arevalo, M., Wörner, G., Schneider, C & Casassa, G.: Late Glacial and Holocene Paleogeographical and Paleoecological Evolution of the Seno Skyring 1060 and Otway Fjord Systems in the Magellan Region, Anales Instituto Patagonia (Chile), 41, 5–26, http://dx.doi.org/10.4067/S0718-686X2013000200001, 2013.

Leemann A, Niessen F.: Holocene glacial activity and climatic variations in the Swiss Alps: reconstructing a continuous record from proglacial lake sediments, The Holocene, 4(3): 259-268, doi:10.1177/095968369400400305, 1993.

Lougheed, B. C. and Obrochta, S. P.: A Rapid, Deterministic Age-Depth Modeling Routine for Geological Sequences With Inherent Depth Uncertainty, Paleoceanogr. Paleoclimatology, 34, 122–133, https://doi.org/10.1029/2018PA003457, 2019.

Singh, M., Sinha, R., Tandon, S.K.: Geomorphic connectivity and its application for understanding landscape complexities: a focus on the hydro-geomorphic systems of India, Earth Surf. Process. Landf., 46, 110-130, 2020.

Stuiver, M., Reimer, P. J., and Reimer, R. W.: CALIB 8.2, http://calib.org, 2020.

Van der Bilt, W., Bakke, J., Vasskog, K., D´Andrea, W.J., Bradley, R.S., Ólafsdóttir, S.: Reconstruction of glacier variability from lake sediments reveals dynamic Holocene climate in Svalbard. Quat. Sci. Rev., 126, 15, 201–218. https://doi.org/10.1016/j.quascirev.2015.09.003, 2015.