Review of the manuscript submitted to *Biogeosciences* by Amelie Stieg, Boris K. Biskaborn, Ulrike Herzschuh, Andreas Marent, Jens Strauss, Dorothee Wilhelms–Dick, Luidmila A. Pestryakova, and Hanno Meyer
Diatom shifts and limnological changes in a Siberian boreal lake: impacts of climate warming and anthropogenic pollution

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RC1: 'Comment on egusphere-2024-2470', Anson Mackay, 12 Sep 2024

Thank you for your comprehensive and constructive review. As the general summarising points are discussed in more detail below, we will address them in the corresponding sections as appropriate, starting at the specific comments ('Specifics'). Any changes based on the comments in the Reviewer's PDF that have not already been addressed in the specific comments are outlined at the end of this document.

Stieg review BioGeosciences

1. ***Does the paper address relevant scientific questions within the scope of BG?***

BG seeks interdisciplinary studies that look at the interactions between e.g. biological, chemical and physical processes. Steig et al. do this by taking a palaeolimnological approach to investigating biological and organic geochemical records in a relatively remote region of eastern Siberia.

1. ***Does the paper present novel concepts, ideas, tools, or data?***

High resolution diatom analyses and organic geochemical analyses are well established in palaeolimnology, including multiproxy studies. However, there is novelty in these data in the presentation ofd ^{30}Si of the diatom silica in the Lake Khamra sediment record. Records of d $^{30}Si_{diatom}$ are still relatively uncommon in lake sediment records, yet they have the potential provide much needed information on e.g. lake biogeochemistry, dissolved silica utilisation etc. What makes their record stand out even more, in my opinion, is that the data are presented at a decadal resolution for the past 200 years.
I think that there is also a missed potential of presenting these d $^{30}Si_{diatom}$ records alongside their d$^{18}O_{diatom}$ records from a previous publication – having joint isotopes from the same diatom material, alongside the diatoms themselves, has great potential for quite deep lake-catchment ecosystem understanding.
Moreover, although d^{15}N data are presented, they are barely discussed at all, which I found surprising given the discussion on potential acidification of the lake, changes in atmospheric reactive nitrogen etc.
Just thinking about it more, their samples actually have 4 different types of stable isotopes investigated but little is made of this.

1. ***Are substantial conclusions reached?***

Substantial conclusions are reached, but I don't agree with one of the main findings that their record provides evidence for lake acidification.

1. ***Are the scientific methods and assumptions valid and clearly outlined?***

Largely yes, except for the use of chrysophyte scales to create an index to be interpreted as a record for lake acidification.

1. ***Are the results sufficient to support the interpretations and conclusions?***

Largely yes, but not for lake acidification

1. ***Is the description of experiments and calculations sufficiently complete and precise to allow their reproduction by fellow scientists (traceability of results)?***

Yes

1. ***Do the authors give proper credit to related work and clearly indicate their own new/original contribution?***

Yes

1. ***Does the title clearly reflect the contents of the paper?***

I don't think that their study really does focus on anthropogenic pollution. No pollutant data are actually shown (only referred to) and I question the validity of using a chrysophyte index as their evidence for lake acidification.

1. ***Does the abstract provide a concise and complete summary?***

It will need to be revised

1. ***Is the overall presentation well structured and clear?***

It makes sense to present data in order that these data are then discussed.

1. ***Is the language fluent and precise?***

Overall yes, but the authors needs to be more careful in use of e.g. 'statistical' when no statistical tests have been done, and 'various' which leaves the reader to make guesses. And sometimes statements are rather vague, or need further supporting evidence. I have highlighted these where relevant below and in the annotated PDF

1. ***Are mathematical formulae, symbols, abbreviations, and units correctly defined and used?***

I think I found one mistake in how the diatom fluxes are presented – details are given below.

1. ***Should any parts of the paper (text, formulae, figures, tables) be clarified, reduced, combined, or eliminated?***

I think that the figures need to be re-ordered so that they are presented in the order that they are subsequently discussed in. The discussion is overly long and could be more focussed

1. ***Are the number and quality of references appropriate?***

Largely yes, although I think that there are many more studies from Siberian lakes that have looked at recent environmental change. Also, I'd like to see better use made of studies that detail diatom autecologies -these will help in the diatom interpretations.

1. ***Is the amount and quality of supplementary material appropriate?***

If my suggestions for corrections are found useful, then I don't think any SM is necessary

Diatom shifts and limnological changes in a Siberian boreal lake: impacts of climate warming and anthropogenic pollution

Overall review

This is an interesting palaeolimnology study that provides much needed information in lake ecosystem changes in a relatively remote region of eastern Siberia. The authors take a multiproxy approach to understand how environmental change has impacted Lake Khamra over the past 200 years. The datasets are excellent, with radiometric dating suggesting that a decadal resolution has been reached throughout the time-period investigated. This is no mean feat, as lakes in these cold regions often have very slow accumulation rates, making records of recent change difficult to decipher. This is not the case here.

The main datasets presented include biology (diatoms) and organic geochemistry. These are palaeolimnological mainstays. However, there is also an excellent dataset of diatom silicon isotope analyses $d^{30}Si_{diatom}$, that is of decadal resolution. There is also a record of *Mallomonas* chrysophyte scales, but more on that below. The study details ecological and geochemical changes over the past 200 years that are concurrent with other studies in the region and further afield, even in different continents (e.g. North America, northern Europe) of the impact of warming on ice-covered lakes. A convincing key finding from Steig et al. is a shift in geochemical properties of the lake (ca. 1950 CE) that precede shifts in diatom communities a couple of decades later (ca. 1970 CE), trends that are exhibited in other cold regions of the northern hemisphere.

Less convincing however, is the suggested impact from pollution, causing the lake Khamra to acidify in recent decades – here I think the study is not critical enough in the evidence presented. Indeed, as detailed below, there a few parts of the study where a greater and more critical examination of the literature is required.

Overall, I recommend publication, but major revisions will need to be made. I provide more detailed comments below, and in the attached PDF.

Specifics

Title: As will be clear below, I'm not sure that the title is appropriate with regards to impacts of anthropogenic pollution. I didn't see any keywords on the manuscript, but I think the term 'multiproxy' would be a good one to add, either to the title or as a keyword.

ANSWER: As outlined below and also in response to the comments from Reviewer #2, we have revised our interpretative approach regarding anthropogenic pollution and acidification trend. We acknowledge that there is no new data presented that provide clear evidence for anthropogenic pollution, apart from $\delta^{13}C$-depletion, which is likely influenced by air pollution from fossil fuel combustion. We have modified the title accordingly:

"**Diatom shifts and limnological changes in a Siberian boreal lake: a multiproxy perspective on climate warming and anthropogenic air pollution**"

Copernicus does not include keywords in its manuscripts. However, your suggestion regarding 'multiproxy' is valuable, and we have incorporated it into the revised title. Thank you.

Introduction:

Line 49- : the statement "*These lake observations align with the proposed onset of the 'Anthropocene', a geological epoch beginning in the mid–20th century, introduced by Crutzen and Stoermer (2000) and further supported by Zalasiewicz et al. (2017).*" Is rather out of place, now that the Anthropocene has not been retified as a formal Epoch. Use of the term Anthropocene is still of course valid, but requires a more nuanced context.

ANSWER: We agree and have revised the sentence accordingly:

"*These lake observations align with the proposed onset of the 'Anthropocene' (Crutzen and Stoermer, 2000), though it has not been formally recognised as a distinct geological epoch (ICS, 2024, last access 18. November 2024).*"

Lines 56-57: I want to push back a bit on the statement "However, there is a notable gap in understanding how these global changes impacted remote and less studied regions like Siberia.". I think that there are a considerable number of studies showing evidence of recent environmental change over the past 150 years in Siberia that could be used to here, but I do agree that much more research is needed for this region.

ANSWER: We agree and have rephrased the paragraph accordingly, incorporating additional studies, particularly from the well-studied Lake Baikal region:

"*Globally lake surface temperatures have risen in recent decades (O'Reilly et al., 2015; Hampton et al., 2018), having numerous consequences like changing thermal stratification properties, longer ice-free periods and changing lake ecosystems like shifting biological communities (Smol et al., 2005; Saros et al., 2012; Hampton et al., 2017; Woolway et al., 2020). Extensive research on lakes in North America and Europe (Smol et al., 2005; Smol and Douglas, 2007; Rühland et al., 2008; Kahlert et al., 2020) has shown that climate warming and* *human activities lead to significant ecological changes on remote high latitude ecosystems. Lake Baikal, a thoroughly studied system, has experienced documented ecosystem changes driven by recent warming, including increased water temperatures and shorter ice-cover periods (Todd and Mackay, 2003; Mackay et al., 2006; Hampton et al., 2008; Moore et al., 2009; Hampton et al., 2014; Izmest'eva et al., 2016). However, a notable gap remains in understanding how these global changes impacted boreal lakes in remote and less-studied regions of eastern Siberia, such as the Republic of Sakha (Yakutia).*"

Lines 65-66: you attribute "*reduced lake ice cover duration*" and "*stronger thermal Stratification*" since the 1950s to Roberts et al. 2018, but both those statements are linked to work done by other studies that Roberts et al. have cited. I think my point here is to make sure that when you cite a study, you cite it for the research it itself has done.

ANSWER: Thank you for this comment. We have restructured this and the previous paragraph to ensure that the references correctly attribute the findings on changing lake properties in the Introduction (see our previous response for details).

Line 75: Leng and Barker reviewed oxygen isotopes in diatom silica, not silicon isotopes.

ANSWER: Thank you for pointing this out. Since we refer to both oxygen and silicon isotopes in diatoms to reconstruct past climate and environmental conditions, we have retained Leng and Barker (2006) for oxygen isotopes and added the comprehensive reviews by Sutton et al. (2018) and Frings et al. (2024) for silicon isotopes.

Line 82: I don't think I would call Hg a biogeochemical proxy

ANSWER: We agree and have rephrased the sentence accordingly: "*Paleolimnological studies in eastern Siberia, including Yakutia, have utilised diatom assemblages, alongside diatom isotopes, biogeochemical proxies such as organic carbon and nitrogen, and mercury analyses to investigate past environmental conditions.*"

Line 90: "…*the lakes investigated are either not located in Siberia*…" but this is not really true. For example, even from my own (and collaborators) and other Russian-led work, there are many studies looking at palaeo records in Siberia for past 150 years.

In this section of the Introduction, in providing a setting for their study, the authors justify why their study needs to be done by saying where other studies have been done, with few in Siberia. But the case made is not convincing as sources used are rather old and not comprehensive. I think the authors should have confidence that their own study importantly adds to a growing body of work from Siberia and the Yakutian region especially. Maybe a route through this would be to focus in on the Yakutian region more, as Siberia is vast, over many eco-regions N-S, E-W etc, with many competing drivers of environmental change.

ANSWER: Thank you for this comment. We agree that 'Siberia' is a broad term and have refined our focus on Yakutia and adjusted the paragraph accordingly:

"*However, many of these studies predominantly cover periods within the Quaternary, primarily focusing on the Holocene, and investigate long–term trends (e.g. (Cherapanova et al., 2006; Biskaborn et al., 2012; Pestryakova et al., 2012; Biskaborn et al., 2016; Biskaborn et al., 2021c; Firsova et al., 2021; Kostrova et al., 2021; Mackay et al., 2022; Biskaborn et al., 2023). While there is a growing body of work on recent environmental change in eastern Siberia, high-resolution studies covering shorter time periods in Yakutia remain rare. Besides a study in the remote boreal areas in Yakutia (Biskaborn et al., 2021b), notable study sites are located further south at Lake Baikal (Roberts et al., 2018), on Kamchatka (Jones et al., 2015), further west towards Europe (Palagushkina et al., 2020), or in the northern Urals (Solovieva et al., 2008). However, these sites are influenced by different climatic and ecological conditions that can differ from those in Yakutia."*

Lines 94-98: With the sentence starting "*Beside oxygen isotopes, silicon isotopes of diatoms…*" the rationale for Si can be better expressed as an indicator of changing productivity and nutrient utilization etc, which has been found in other lakes, including Baikal (see Virginia Panizzo-led articles).

ANSWER: Thank you for your comment. We agree with your suggestion and have revised the paragraph to better highlight the role of diatom silicon isotopes. We have also added relevant references to research from Lake Baikal to provide additional context:

"*In addition to oxygen isotopes, silicon isotopes of diatoms have been used as indicators of changing productivity and nutrient utilization. Predominantly, $\delta^{30}Si_{diatom}$ serves as a proxy for primary production in marine environments (De La Rocha et al., 1998; Sutton et al., 2018). In lacustrine systems, however, $\delta^{30}Si_{diatom}$ interpretation is more complex due to the combined influence of catchment processes (e.g. weathering, vegetation, soil development) and in-lake biogeochemical dynamics (e.g. silica cycling and sedimentation) (Sutton et al., 2018; van Hardenbroek et al., 2018; Frings et al., 2024). Research at Lake Baikal has explored the preservation and reliability of $\delta^{30}Si_{diatom}$ for paleo-reconstructions, as well as its link to diatom productivity and sedimentary processes (Panizzo et al., 2016; 2017; 2018). Despite these studies, spatial and temporal coverage of $\delta^{30}Si_{diatom}$ research in eastern Siberia remains limited. Furthermore, studies that analyse both oxygen and silicon isotopes of diatoms from the same samples are rare, with one example from northern Siberia focusing on the time period since the Last Glacial Maximum (Swann et al., 2010).*"

Line 99: be careful using terms such as "*significant*" when significant tests were not undertaken. This is done many times throughout the manuscript

ANSWER: Thank you for pointing this out. We will be more careful with the use of the term "significant" and have removed it wherever it was not appropriate, ensuring more accurate language throughout the manuscript.

Lines 104-107: with the objectives, I'm not sure that erosion input linked to climate variability has been shown, as no proxy for erosion has been specifically measured (possibly % dry weight might work here, but something like magnetic susceptibility would have been a better indicator). Overall, a much more critical approach needs to be taken on the impact of pollution on diatoms in the lake.

ANSWER: Thank you for this comment. It is correct that this study does not include a direct proxy for erosion. Due to limited sample material, we prioritised a variety of other proxies. However, we discuss erosion in relation to flux rates, such as organic carbon accumulation rates (OCAR) and changes in sedimentation rates (SR), considering hydroclimatic variability reconstructed from previously published $\delta^{18}O_{diatom}$ data from the same samples and meteorological records from the nearest weather station. Additionally, we use C/N ratios and $\delta^{13}C$ to assess the origin of organic material and distinguish between allochthonous and autochthonous sources, also in relation to diatom assemblage changes. We also discuss the factors influencing $\delta^{30}Si_{diatom}$, including potential contributions from bioproductivity or source changes due to catchment weathering.

The discussion on pollution and lake acidification has been revised as outlined further below and also in agreement with Reviewer #2. Nonetheless, we still find evidence of air pollution in the Lake Khamra sediment record, including elevated mercury levels (as shown in a previous study) and a decline in $\delta^{13}C$ in modern samples. Furthermore, we could show that Lake Khamra is not (yet) influenced by reactive nitrogen deposition seen in the $\delta^{15}N$ values.

We have reworded the objectives, excluding 'erosional input' to reflect these considerations:

'*Our specific objectives are to (I) identify historical lake ecosystem changes within a continuous diatom assemblage record spanning the last ~220 years, (II) evaluate the origin of organic material and distinguish between allochthonous and autochthonous sources in response to hydroclimatic variations using biogeochemical proxies, and (III) assess the impact of human–induced air pollution on the lake ecosystem.*'

Section 2: Overall, the methods section is excellent – really well written.

Thank you!

Line 159: state / summarise how water content and DBD were determined, (with full details given in Stieg et al. 2024a). For example were these determined through a scanner or through physical drying in an oven). I think it would be good to have MAR shown in one of the profiles.

ANSWER: Thank you for your suggestion. We have provided further details on how the water content and dry bulk density (DBD) were determined in the previous study:

"*In addition, data on the water content and the average dry bulk density (DBD) of the short core were determined from selected subsamples (n=24) using a 1 cm³ tool taken at regular intervals, with weights measured before and after freeze-drying. The mean DBD (g cm^{-3}) was then used together with the sedimentation rates (SR, cm a^{-1}) to calculate mass accumulation rates (MAR, g cm^{-2} a^{-1}) downcore (full details given in Stieg et al., 2024a).*"

Regarding the MAR values, we have now included the record in Figure 6.

Section 2.4: This really is an impressive highly-resolved dataset

Thank you!

Lines 222-223: the range of flora used are quite European focussed. Were any other guides used, especially Russian flora?

ANSWER: Thank you for pointing this out. Species identification was primarily based on the specified literature and online resources mentioned in the text. In addition, we have now included references to works used in collaboration with our Russian colleagues, which were applied to verify species identification and cross-check our counts.

"*Diatom species were identified to lowest possible taxonomic level <u>primarily using classical identification literature</u> (Krammer and Lange-Bertalot, 1991; Krammer et al., 1991; Krammer and Lange-Bertalot, 1997b, a; Hofmann et al., 2011), <u>supported by regional (Russian) floras</u> (Komarenko and Vasilyeva, 1975; Moiseeva and Nikolaev, 1992; Makarova, 2002). Diatom nomenclature and synonyms were applied using online databases such as Diatoms.org (https://www.diatoms.org; last access: 01. August 2024; Spaulding et al. (2021) and AlgaeBase (https://www.algaebase.org; last access: 01. August 2024; Guiry and Guiry (2024), for some species further supported with input by members of the diatom community online platform DIATOM–L (Bahls, 2015).*

The only approach I have an issue with is the use of a *Mallomonas* index as a proxy for lake acidification. Greater justification for lake acidification needs to be made, because I was quite surprised to hear that acidification could occur give that the catchment consists of "*Cambrian bedrock composed of alternating dolomite and limestone"*, rocks that are rich in acid-neutralising cations. In the introduction to the study, while the threats of climate change are well set out, it is not made clear if atmospheric pollution and lake acidification are real threats to the region.

ANSWER: In agreement with Reviewer #2's comments, we have revised our interpretation of the *Mallomonas* index and removed the references to lake acidification and nutrient enrichment, as our previous interpretation was based on a misreading of the cited studies. We have incorporated the references suggested by Reviewer #2. The sentence has been revised as follows:

"*In addition, silicified chrysophyte Mallomonas scales were counted without further specification to calculate the Mallomonas index, which measures Mallomonas in relation to diatom cells (M/D), to evaluate the degree of thermal stratification and the trophic status (Smol, 1985; Ginn et al., 2010).*"

Please find further details on the revised interpretation of the *Mallomonas* index in the comments below.

Section 2.6: lines 261-264, is there a mistake with the units?

If MAR = g/cm2/a, and concentrations are 10^7 valves/g, then that will mean DVAR = 10^7 valves /g/cm2/a. I assume the authors are multiplying by 100 to give 10^9 valves /g/cm2/a (although I'm not sure why!). But DVARs are then being expressed as 10^9 valves /**m2**/a and this is wrong - it should be 10^9 valves /**cm2**/a. The /m2 was calculated for reporting the changes in carbon fluxes. If you were to go from /cm2 to /m2 you'd need to multiply the diatoms by 10,000? Or am I missing something here?

ANSWER: We based our calculations on the approach outlined by Biskaborn et al. (2023) and used the same units (10^9 valves/m²/a). To achieve this, we first divided the concentrations of diatoms DVC (10^7 valves/g) by 100 in order to express the concentrations in 10^9 valves/g. You are correct that when converting from cm² to m², we would need to multiply by 10,000. Following this, we multiplied the DVC (10^9 valves/g) by the mass accumulation rate (MAR) given in g/cm²/a and then by 10,000 to convert the units to 10^9 valves g/**m²**/a. For simplicity, the calculation could be streamlined by multiplying by 100 directly.

We recognise that the description has caused some confusion. We rephrased the sentence accordingly: "*Diatom accumulation rates (DAR) were calculated by multiplying the diatom valve concentration (DVC in 10^7 valves g^{-1}) with mass accumulation rates (MAR, g cm^{-2} a^{-1}) according to Birks (2010) and convert it to 10^9 valves m^{-2} a^{-1}.*"

Line 263+ 266: as the data being correlated are from a sediment core, they are majourly affected by temporal autocorrelation and not truly independent. Therefore, Pearson correlation is not appropriate here. Moreover, as mixed data types are being correlated (and data not tested for normality), a Spearman-Rank correlation would be more appropriate. Lastly, I'd suggest a more rigorous p value as your data are not independent e.g. 0.005 at least. I'd actually question of correlating these data to such an extent is even warranted as I don't think this analyses adds to the study.

ANSWER: Thank you for this valuable comment. We understand that a Pearson correlation is not appropriate due to temporal autocorrelation and the lack of independent data points. As suggested, we attempted a Spearman rank correlation with a smaller p-value. However, we also acknowledge that the correlation analysis does not add additional value to our study. Therefore, we have decided to remove these correlations from the manuscript.

Lines 288-290: There is an interesting decline $d^{15}N$ since the 1960s -is this similar to studies of remote cold lakes in e.g. North America where a decline in also observed, and if so, is it related to increase in isotopically lower nitrogen from anthropogenic sources? (I note however that the magnitude of decline in Khamra is much lower than in other studies).

ANSWER: Thank you for pointing this out. Since this observation is part of the results section, we have now included it at the end of the following sentence:

"*The $\delta^{15}N$ record shows a generally low variability and follows mainly the mean value of +2.9 ‰ and varies in total by ±0.5 ‰, with slightly enriched values between approx. 1930 and 1970 CE <u>and a subsequent minor decline</u> (Fig. 2).*"

The comparison with other studies is addressed in the discussion section within 4.4 as follows:

"*In contrast to the recent marked increase in mercury levels and $\delta^{13}C$ depletion, $\delta^{15}N$ in Lake Khamra sediments has shown only minimal variation over the last ~220 years, fluctuating by ±0.5‰, with a slight decrease of ~0.3‰ since the 1970s (Fig. 5). Human activities, such as fossil fuel combustion and fertilizer production, are relevant sources of reactive nitrogen (Nr) that contribute to the deposition of $\delta^{15}N$-depleted nitrogen in lake sediments, typically in a range from 1–3‰ (Gruber and Galloway, 2008; Holtgrieve et al., 2011; Wolfe et al., 2013). Despite the possible influence of atmospheric pollution, no substantial $\delta^{15}N$ depletion is observed in Lake Khamra.*"

Figure 2: an effective way of comparing geochemical trends with the diatoms would be to plot here the PCA axis 1 and axis 2 sample scores. I see that these are presented in Figure 4.

ANSWER: In order to address the reviewer's comment, we have adjusted the order of the results so that the figures are discussed in the sequence in which they are presented (see other comment related to new arrangement of discussion). Additionally, we have revised the (synthesis)figure in the discussion (Figure 6) by incorporating relevant biogeochemical proxies (OCAR, $\delta^{30}Si_{diatom}$) to allow for a better visual comparison with diatom indices (e.g. DAR, selected abundances), $\delta^{18}O_{diatom}$ values, and meteorological data. We hope these modifications comply with the requested changes.

Figure 3: Are the authors certain *Fragilaria* cf. *gracilis* is tychoplanktonic (where was this information obtained from)? *Fragilaria* cf. *gracilis* has recently been suggested to actually be

F. radians. If that is truly the case, then this would mean *F. gracilis*/*radians* a planktonic taxa not tychoplanktonic. https://fottea.czechphycology.cz/artkey/fot-202202-0009_fragilaria_radians_k_tzing_d_m_williams_et_round_the_correct_name_for_f_gracilis_fragilariaceae_bacillari.php#:~:text=Fragilaria%20gracilis%20is%20one%20of,rivers%20to%20even%20eutrophic%20lakes.

ANSWER: Thank you for sharing this study and highlighting that it is likely a planktonic species. According to Hofmann et al. (2011), *Fragilaria gracilis* appears to be part of a broader and difficult-to-differentiate species complex around *F. capucina*. We have re-examined the species identification and conclude that we were most likely referring to *Fragilaria capucina var. gracilis* (Østrup) Hustedt 1950, which is also considered a synonym of *F. radians (Van de Vijver et al., 2022; Guiry and Guiry, 2024).*

We have updated the stratigraphic plot (Figure 2) accordingly and have now classified the species as planktonic. Additionally, this has led to a revision of the diatom zone description in the results, an updated the P/B ratio (Figure 3) and the PCA plot (Figure 4), which have been adjusted accordingly.

Overall a succinct account is given of the main changes in the diatom zones.

Thank you!

Figure 5: This is a nice figure. But I think the colours for the triangles for the two time periods could be more clearly contrasted (even with different shapes)

ANSWER: Thank you for this suggestion. We have revised the figure (Figure 4) and now use both different colours and shapes to enhance the contrast between the periods before and after 1950 CE.

Discussion

The Discussion is overly long at c. 5500 words alone. Perhaps my comments below will provide some scope for a reduction in length and a clearer focus. I think there is too much repetition between the results and discussion, and a stronger discussion would be one that links together the different proxies more effectively.

ANSWER: Thank you for your feedback. We have revised the discussion and shortened it (now ca. 4900 words including figure captions etc.) to improve focus and reduce repetition. Instead of discussing each diatom zone in detail, we now emphasise the major diatom taxa and key shifts, particularly around the 1950s (biogeochemical changes) and 1970s (diatom assemblage shift). Additionally, we have strengthened the integration of different proxies and refined the discussion on potential anthropogenic influences on Lake Khamra. Please see our detailed responses below.

Line 392: *Aulacoseira* are also common in lakes that stratify for part of the year, e.g. during ice cover or in warmer summer months. Not all *Aulacoseira* species are large, but there different sizes does have implications for resource competition for e.g. dissolved silica

ANSWER: Thank you for clarification. It is correct, that *Aulacoseira* species can also be found in lakes that experience seasonal stratification, and that not all species are large, with size differences playing a role in resource competition, particularly for dissolved silica. We address this topic in a different section of the discussion regarding Si-Isotopes (Chapter 4.3.1).

In this paragraph, we have rephrased the sentences and focused on the key point regarding the turbulence required to keep *Aulacoseira* in suspension or to resuspend their resting cells into the water column.

Line 399: "…*and a seasonal ice cover does not necessarily reduce its abundance*" – indeed! I don't think diatomists would ever think this the case because the majority of lakes are dimictic.

ANSWER: We have removed this part of the sentence.

Line 400-402: I'd agree with this statement, but changes in temperature between air and surface waters, and wind strength & direction are also very important in causing overturn, not just snow-melt.

ANSWER: Thank you for this note. We agree and added the following sentence: "*However, other factors, including atmospheric-surface temperature gradients and wind-induced mixing, are also likely relevant for sufficient lake overturn (Smol and Douglas, 2007; Winder and Sommer, 2012).*"

Line 415: The authors suggest that diatom concentrations *"likely indicates favourable growth conditions at Lake Khamra that enhance diatom productivity*". Possibly, but caution is needed here.

Concentrations are dependent on SARs, so if accumulation rates increase while diatom production remains constant, then diatom concentrations will decline without without needing to infer low-productivity lake. The authors know this, as elsewhere it is stated that DVC vary with SAR. Also, it looks like you are comparing DVC with an average of your whole core, with surface sediments from other cores, which will likely have higher SAR as sediments / core compaction is minimised at the surface layers.

I think it is important to make the decision early on in the study if you are going to present and discuss DVCs or DVARs. I'd recommend the latter, and DVCs do not therefore need to be discussed at all beyond the calculation being an intermediate stage to get to DVAR.

ANSWER: Thank you for this comment. We agree with your argumentation and have decided to focus exclusively on DARs in the discussion. However, we retain DVCs in the results to present all data transparently. DARs are subject to uncertainties and potential errors due to multiple calculation steps, as well as the high water content of the short core especially in the uppermost samples. Nonetheless, we acknowledge that DARs are more meaningful when discussing productivity.

We have removed the comparison of concentrations in this section and now refer exclusively to the DARs of our core, which remain consistently above the minimum of 201 $*10^9$ valves m^{-2} a^{-1}, reflecting sustained diatom productivity suitable for palaeoecological studies. Additionally, the high *F* index supports good preservation conditions.

Section 4.2:

I found this section quite difficult to keep track off. There is repetition with the results section, when changes in diatom abundances are described by zone.
There are a lot of inferences being made with regard to changes in diatom relative abundances, and it's hard to decipher which of these are likely to be real or simply a function of having species expressed as relative abundances (as one species increases, others must decline etc).
Rather than having a discussion based on the zones (which is rather descriptive), a higher-level discussion that just focuses on the main trends would be better.
When I look at the stratigraphy in Fig 3 in relation to your discussion I'd recommend that authors consider:

1. what are the ecology and habitat preferences of each of the taxa shown - draw on information from papers that look at their ecologies (lab, modelling, field).

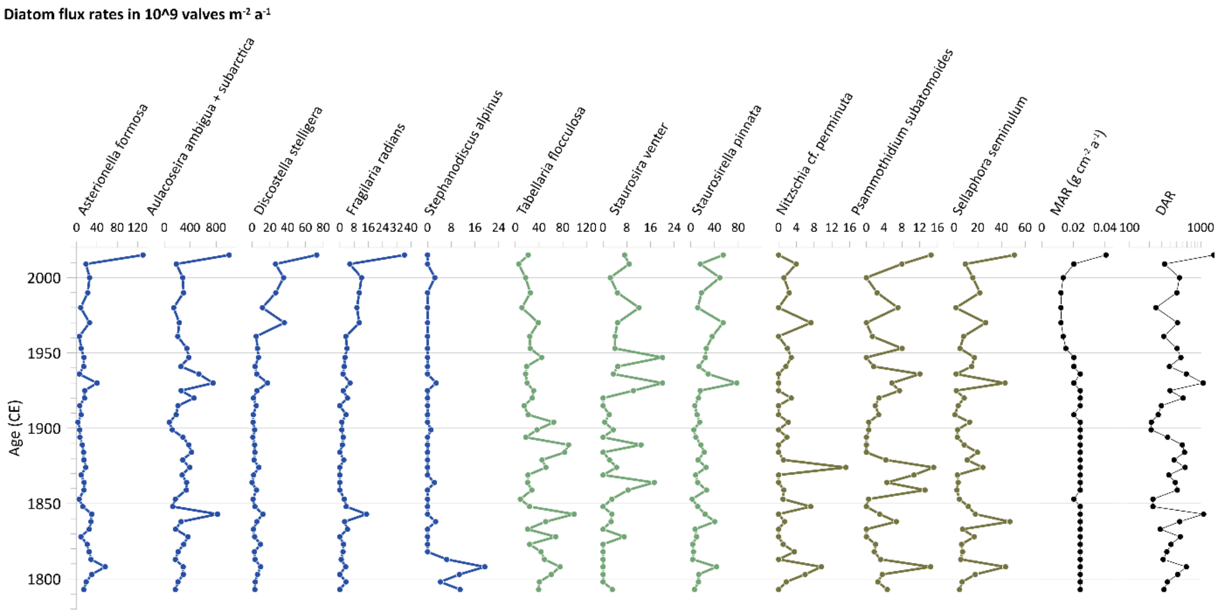
1. provide some summary plots on the stratigraphy: total planktonic, total tychoplanktonic; total benthic; PCA plots of axis 1 and axis 2 sample scores. These will all help with the interpretation and could help ensure over-interpretation of the percentage changes is not being done.

1. As you have calculated total diatom fluxes, how might the stratigraphy look if all the major taxa were shown as fluxes. This means that changes in their abundance are independent from each other.

ANSWER: We agree that discussing individual zones based on the cluster analysis leads to repetition with the results section. To address this and reduce the overall length of the discussion, as previously commented, we have streamlined the section to focus on the main trends. In particular, we now emphasise the two dominant *Aulacoseira* species in relation to hydroclimatic variability, as reconstructed from the previously published $\delta^{18}O_{diatom}$ record and meteorological data. The discussion now primarily highlights the major diatom shift in the 1970s, likely linked to climate warming and related changes in lake properties.

Additionally, we have revised Figure 6 to serve as a summary plot in the discussion, comparing key diatom indices (*Aulacoseira* abundance vs. *D. stelligera*; DAR) with biogeochemical proxies (MAR; OCAR), diatom stable isotope records ($\delta^{18}O_{diatom}$ and $\delta^{30}Si_{diatom}$), and meteorological data (T and P) from the nearest weather station. We have removed the total solar insolation and the charcoal record from Lake Khamra, as they are no longer central to our discussion or do not contribute to the visualisation.

To further support our interpretation, we have calculated individual diatom flux rates, as suggested, to visualise species changes independently of relative abundances (see preliminary figure below). This confirms an increase in *S. alpinus* at the onset of the record and in *D. stelligera* (and to a lesser extent *F. radians*) in the uppermost zone. However, we have decided to retain the classical stratigraphic plot in the results section as it is. We are open to include the diatom flux rates as an additional figure in the appendix if desired.



Lines 431-434: Not sure if this is a good comparison to make. *Stephanodiscus* species normally need less silica and do better in waters that are sufficient in nutrients - e.g. see classic Kilham et al. 1986 paper (and others) https://aslopubs.onlinelibrary.wiley.com/doi/abs/10.4319/lo.1986.31.6.1169

ANSWER: Thank you for this comment. We have incorporated information from Kilham et al. (1986) and revised the sentence to better reflect the ecological characteristics of *S. alpinus*.

Lines 459-460: Be really careful of interpretation. I would not rely on a PCA biplot to inform competition ecology between your species. Go to source material for those species ecologies.

ANSWER: We agree. However, we have shortened this section as outlined above. As a result, the specific reference to the PCA biplot has been removed.

Lines 499-503: There is far too much conjecture here for so many reasons. The authors have not provided the evidence to support the claims being made as they are not comparing like (ecosystem) with like (ecosystem).

ANSWER: This paragraph has been removed.

Lines 527-529: A lot of caution is required here when making comparison to Biskaborn et al. 2021b. for a couple of reasons:

First, the resolution of the stratigraphies in Biskaborn is *much* lower than for Khamra, and so there are major uncertainties in tying together what look to be similar patterns.

Second, their interpretation requires greater critical consideration when interpreting the data presented. The two shifts between major taxa - one around 1850 CE or so, and one in the 1970s - must be responses to different drivers.

- The shift at c. 1850 CE I think will be climate related, with the northern hemisphere broadly moving out of the cooler Little Ice Age. While many countries were industrialising at that time, you would need to provide economic evidence that this was also occurring in your region to cause acid rain. I'm not an expert in this region, but I don't think this exists, and no evidence is presented to show that it does exist. We see similar shifts in diatoms across Lake Baikal for example, that had little industry around its catchment.
- The shift in the 1970s is likely related to global warming and possibly increased reactive nitrogen from pollution. You have perhaps $d^{15}N$ evidence of this, although the decline is rather small.

ANSWER: We agree and added the difficulties with comparing the Bolshoe Toko record (Biskaborn et al. 2021b) with the results of Lake Khamra:

"A similar shift in diatom composition has been observed at Lake Bolshoe Toko, a comparable remote boreal lake in eastern Siberia, after 1850 CE, interpreted as a response to climate warming (Biskaborn et al., 2021b). <u>However, the much lower stratigraphic resolution at Lake Bolshoe Toko introduces age uncertainties, making direct comparison with the Lake Khamra record difficult. The observed age offset in diatom shifts could refer to different climate related drivers.</u> Further diatom assemblage shifts are observed at the south basin of Lake Baikal at ca. 1970 CE (Roberts et al., 2018), as well as at pristine Arctic and sub–Arctic lakes in the northern Urals with most distinct shifts after 1970 CE (Solovieva et al., 2008). Our data align with the later onset of diatom assemblage changes at lower latitudes compared to the earlier changes observed in Arctic regions, which is attributed to an earlier warming and a quicker response of Arctic ecosystems compared to lower latitudes (Smol et al., 2005; Smol and Douglas, 2007)."

In line with comments from Reviewer # 2, we omitted the interpretation of acidification at Lake Khamra, including the discussion of the $\delta^{15}N$ data in the last chapter of the discussion (4.4). These data indicate <u>no increase</u> in atmospheric reactive nitrogen deposition at Lake Khamra, which further supports warming as an influencing factor on diatom assemblage changes.

Section 4.3:

I wonder if it would be better to layout the changes in organic geochemistry in the lake / catchment system before the discussion of the diatoms. Then relate the diatoms to changes in the broader organic geochemistry environment?

ANSWER: Thank you for this suggestion. We address this point along with another comment regarding the figure structure further below. To improve clarity and coherence, we have decided to reorder the Methods and Results sections along with the corresponding figures so that they align more closely with the flow of the Discussion. This ensures a more logical structure while maintaining the focus of our research questions, as outlined at the end of the Introduction.

Lines 571-572: The authors make the assumption that because climate is inferred to be drier at this time period, then erosional input from the catchment is reduced, but no evidence is produced for a decline in erosional material. While it may be the case that slower accumulation rates are linked to less catchment input, given the planktonic diatoms dominate the profile it could be as likely that sediment accumulation is linked to primary production in the lake, and if conditions are cooler and drier, then there may be less primary production.

ANSWER: Thank you for this valuable comment. We have revised the paragraph to clarify the connection between $\delta^{18}O_{diatom}$ values and drier conditions, as also suggested in the comment in the pdf. Additionally, we acknowledge the possibility that, besides reduced erosional input from the catchment, lake productivity may have declined during this dry and cold phase. We have incorporated this aspect into the discussion.

"*At ~1950 CE, the $\delta^{18}O_{diatom}$ values reach a maximum (Fig. 6), interpreted as a dry anomaly with reduced input of isotopically light winter precipitation into the lake (Stieg et al., 2024b). This reconstructed anomaly aligns with the overall dry period inferred from persistently below-average annual precipitation starting from the 1930s, at the onset of meteorological recordings, and continuing until the 1970s (Fig. 6; details in Stieg et al., 2024b). Additionally, a charcoal record from the same lake identifies enhanced fire activity during the 1950s (Glückler et al., 2021), coinciding with the $\delta^{18}O_{diatom}$ maximum, providing further evidence of this dry anomaly. At the same time, mass accumulation rates (MARs) drop (Fig. 6), which could be linked to a reduction in erosional input, further supported by the flattening of the age-depth relationship from the 1950s onwards (Fig. A1), indicating lower sedimentation rates. The concurrent decrease in organic carbon accumulation rate (OCAR) to a minimum of 11.5 g m^{-2} a^{-1} between 1950 and 1970 (Fig. 5 & 6) suggests lower erosional input of organic material, a reduced primary productivity, or a combination of both. In addition to the dry phase, declining annual temperatures, reaching a minimum between ~1950 and 1970 CE (Fig. 6), indicate simultaneously cool conditions. While Aulacoseira remains dominant, it decreases especially between the 1950s and the 1970s. Diatom accumulation rates (DARs) decline after the peak in the 1930s indicating rather a reduction in primary productivity around the 1950s. This is likely linked to cold annual temperature anomalies and less nutrient refreshment due to reduced turbulence and inflow, as Aulacoseira begins to decline. In contrast, total organic carbon (TOC) and total nitrogen (TN), which represent the fraction of organic matter and are closely related to primary productivity (Meyers and Teranes, 2001), both show a peak between 1950 and 1970 CE (Fig. 5), indicating higher bioproductivity. It is plausible, that low sedimentation rates could result in a relative increase in TOC and TN concentrations within the sediment, explaining the contrasting tendency.*"

Line 592: I think that Fig A5 should actually be shown in the main body of the paper.

ANSWER: We agree with this suggestion and have moved the figure from the appendix into the main body of the paper. It is now presented as Figure 7.

Lines 604-606: again, be careful of over-interpreting data. At this site, ice cover, snow cover, depth of mixing etc are all going to be much bigger determinants of the amount of light the planktonic diatoms can use than changes in solar variability!

ANSWER: We agree with this argument and have removed the sentence accordingly. Additionally, we excluded solar variability from Figure 6.

Line 669: Fig 2: It's odd having the figure for geochemistry being so far away from its discussion. I'd recommend re-ordering the figures, or reordering the structure of the discussion.

ANSWER: As mentioned in a previous response, we have reordered the figures to ensure they appear in the text closer to their discussion.

Line 670-671: The Suess effect can be detected on $d^{13}C$ values from c. 1850CE and the start of the industrial revolution....

ANSWER: This is correct. We rephrased the sentence accordingly to focus solely on the situation at Lake Khamra.

Lines 687-690: This is quite a conclusion to make. If we look at the $d^{30}Si$ profile and e.g. *A. subarctica*, they both show a concurrent response at 1830 CE for example. $d^{30}Si$ values do not need to change very much to infer quite substantial changes in resource availability and nutrient cycling.

ANSWER: Thank you for your comment. We have revised the paragraph to clarify the relationship between *Aulacoseira*, its high silica requirements due to the highly silicified frustules, and the $\delta^{30}Si_{diatom}$ isotope signal. We reworded our statements regarding $\delta^{30}Si_{diatom}$ interpretation as follows:

End of chapter 4.3.1:

"*Overall, interpreting $\delta^{30}Si_{diatom}$ in lacustrine systems is more complex than in marine environments. At Lake Khamra, $\delta^{30}Si_{diatom}$ appears to be influenced by multiple factors, including shifts in diatom assemblages and catchment alterations that affect silica sources and utilisation.*"

End of chapter 4.3.2:

"*Instead, elevated $\delta^{30}Si_{diatom}$ would suggest enhanced bioproductivity based on the classical interpretation of this proxy (De La Rocha et al., 1997). Aulacoseira has a high demand of dissolved silica for their heavy and thick frustules, which could have led to a moderate $\delta^{30}Si$ enrichment of the reservoir during phases of diatom bloom. Furthermore, we assume that, in contrast to the post–1950s period, the catchment was not yet affected by wildfire disturbances, leading to a difference in the isotopic signature of pre and post 1950s. Less influence of isotopic light $\delta^{30}Si$ plant material from soil erosion potentially resulted in a higher $\delta^{30}Si$ level of dissolved silica, which is reflected in the overall elevated $\delta^{30}Si^{diatom}$ (Fig. 6). This highlights the complexity of $\delta^{30}Si_{diatom}$ as a proxy, linked likely to both in-lake biogeochemical processes and catchment dynamics at Lake Khamra.*"

Section 4.4

Lines 700-701: A statement is needed here as to what this level of (Hg) pollution is, contextual wise. Is it similar to background increases across the northern hemisphere? Or does it indicate local origin? Are the concentrations high enough to indicate major sources of pollution that could have caused the suggested impact on your lake?

ANSWER: We agree and have revised the section on Hg concentrations at Lake Khamra, referring also to conclusions made in a previous study (Stieg et al. 2024), and included additional context regarding background levels and comparisons with other study sites:

"*A previous study (Stieg et al., 2024b) identified a marked increase in mercury concentrations in the same lake sediment samples, tripling since ~1930 CE, with fluxes rising more than fourfold since the 1990s. Mercury in the lake can originate from both natural sources, such as permafrost (Rutkowski et al., 2021) and biomass burning (Burke et al., 2010; Driscoll et al., 2013), as well as from anthropogenic sources, such as emissions (Wang et al., 2004; Streets et al., 2011). The observed increase at Lake Khamra is suggested to be at least partially related to industrial growth and associated atmospheric mercury emissions in Asia and Russia (Pacyna et al., 2016; Sundseth et al., 2017; Eckhardt et al., 2023), comparable to mercury accumulation rates observed at Lake Baikal post-1850 CE (Roberts et al., 2020). Here, atmospheric fallout since the 1980s is also suggested to have likely contributed to rising mercury accumulation rates, in addition to mining activities in the vicinity of Lake Baikal (Roberts et al., 2020). Similarly, a rapid mercury increase since 1850 CE has been linked to atmospheric pollution at another remote boreal lake in eastern Siberia (Biskaborn et al., 2021), suggesting that Lake Khamra is likely also influenced by anthropogenic mercury deposition.*"

Lines 703 - : I do have a problem with the use of the *Mallomonas* scales as part of an acidification index in this lake.

- While lakes in base-poor catchments may acidify due to acidifying pollutants, the catchment has dolomite in it, which surely would buffer against potential acidification
- if increasing *Mallomonas* scales are also indicative of increasing nutrients, then would freshwater acidification also occur?
- the increase in *stelligera* is not a diatom I recognise from an acid flora.
- pH 6.07 is not a low pH; lake acidification becomes problematic when pH values fall below a critical level of 5.6.
- Remember a correlation between Hg and *Mallomonas* scales does not mean cause and effect. Is the amount of Hg increasing in the lake likely to be an indicator of enough acid rain to cause lake acidification?
- A lot of this discussion of acidification is based on the conclusions drawn by Biskaborn et al. 2021b. Biskaborn show that Mallomonas scales are actually only found at depth in two of their four cores. And in the other two cores, scales are found only in the surface sediments and therefore cannot represent any acidification trend (for example in their dated PG2203). So during their 'industrial period' (their term), there are no *Mallomonas* scales apart from the very surface layer. In PG2208, there are scales down-core, but these start to increase before the period of 'industrialisation', and therefore cannot be attributed to acidification either.

Personally, I'm not convinced that the Briskaborn study shows (indirect) evidence of acidification at all. But regardless of my opinion on a separate study, the authors of this current study need to be more critical in the evidence presented for lake acidification.

ANSWER: In agreement with Reviewer #2 and as outlined in the comments above, we have revised our argumentation and interpretation of the *Mallomonas* increase in recent sediments. We have omitted the previous interpretation of acidification. Linked to the argumentation outlined by Reviewer #2, we interpret the recent rapid increase as a response to warming and hence likely increased thermal stratification at Lake Khamra. We included relevant studies here, see chapter 4.2.1:

„*Additionally, we observe a rapid increase in the silicified chrysophyte Mallomonas scales since the 1990s, inferred from the Mallomonas index (M/D) (Fig. 3). Chrysophytes are common in oligotrophic environments (Smol, 1985). Their motility, enabled by flagella, allows them to thrive in stratified lakes by maintaining their position in the photic zone. This gives them an advantage over non-motile, colonial diatoms such as Aulacoseira (Ginn et al., 2010; Mushet et al., 2017). The observed increase in chrysophytes at Lake Khamra suggests changes in the lake's mixing regime. Further it provides evidence for a likely longer ice-free period and enhanced thermal stratification during summer months in recent decades. Similar increases in scaled chrysophytes have been reported in other lake systems, associated with climate warming and increased thermal stability (Paterson et al., 2004; Ginn et al., 2010; Favot et al., 2024).*"

To maintain focus on anthropogenic influence from atmospheric pollution, we have removed the discussion of *Mallomonas* from this section (Chapter 4.4). Instead, we have incorporated information on $\delta^{15}N$ and discuss nitrogen deposition, which we conclude is likely negligible at Lake Khamra. We now argue that rising temperatures, driven by ongoing climate warming, have a greater impact on lake properties and diatom assemblages than anthropogenic atmospheric nitrogen deposition.

Line 721-724: The increase in *A. formosa* is very small (c. 4 to 6%?) - this is likely just natural fluctuation. Moreover, it is certainly smaller than the increase around 1810 - 1820 CE. What is the explanation for the larger fluctuation here - it can't be acid rain.

ANSWER: We have adjusted our argumentation in line with the comments of Reviewer #2. We acknowledge that the increase in *A. formosa* is small and does not provide strong evidence on its own. The generally stable $\delta^{15}N$ values throughout the core suggest no clear indication of atmospheric nitrogen deposition at Lake Khamra. Therefore, the recent, modest increase in *A. formosa* abundance is likely linked to ongoing climate warming at the lake, as suggested in the study by Sivarajah et al. (2016).

"*For example, increased abundance of A. formosa in oligotrophic alpine lakes in North America has been linked to atmospheric nitrogen deposition (Saros et al., 2005; Saros et al., 2010). At Lake Khamra, we observe only a slight increase in planktonic A. formosa in the most recent diatom zone 5 (Fig. 2). We suggest that this slight increase in abundance is more likely a response to climate warming and related changes in like water mixing and thermal stability rather than nitrogen enrichment by atmospheric deposition (Sivarajah et al., 2016). This further supports the argument that Lake Khamra is primarily influenced by recent climate warming, which is altering the lake's properties, rather than by atmospheric nitrogen deposition from human sources.*"

Citation: https://doi.org/10.5194/egusphere-2024-2470-RC1

Changes based on comments in the PDF:

(Comments already addressed in the sections above are not listed again.)

Abstract:

- Line 21/22: 'through various biogeochemical proxies' has been taken out. Used proxies are described more specific in the sentences thereafter.
- Line 24: 'combining a variety of proxies on the same sample material' has been changed to 'a multiproxy approach'.
- Line 25 & 26: TOC and TN have been defined first.
- Line 26: TN refers to 'Total Nitrogen'.
- Line 28: Aulacoseira species include: *Aulacoseira subarctica* and *Aulacoseira ambigua.*
- Line 28: 'significant shift' changed to 'major', as suggested.
- Line 29: *Discostella stelligera* was written in full when it was first time mentioned.
- Line 29: include 'both *Aulacoseira* taxa' here, to refer to the two species mentioned above.
- Line 30: Rephrased the sentence for clarity and split it into two.
- Line 32: Abstract has been revised.
- Line 32: Abstract has been revised.
- Line 33 & 34: Abstract has been revised.
- Line 35: changed to 'in order to'.
- Line 38: Abstract has been revised. Sentence is not included anymore.
- Line 39: Sentence moved further upward and interpretation changed: "*A rapid increase in chrysophyte scales (Mallomonas) since the 1990s further supports an increasing thermal stratification of the lake driven by rising temperatures.*".

Introduction:

- Line 64: Sentence has been rephrased, starts with 'Evidence suggests ..'
- Line 72: 'highly resistant' was removed; we wanted to express that the frustules generally are well-preserved in lake sediments, even though dissolution can occur and has to be assessed for each lake sediment individually (*F* index).
- Line 82: 'isotopy' changed to 'isotopes'.
- Line 88: yes, thank you! We changed it to 'further west towards Europe'.

Study site:

- Line 117: 'distant from direct human impact' has been taken out.

Methods:

- Line 197: 'thereby' is taken out.
- Line 213: sentence has been rephrased also regarding the comment of Reviewer#2: "Preparation of slides for siliceous microfossils followed Battarbee et al. (2001)." (now Chapter 2.3).

- Line 258: We have written out the abbreviations here in brackets and only introduce the abbreviation in the next paragraph (diatom valve concentration DVC, diatom accumulation rates DARs).

Discussion:

- Line 405: Unfortunately, no data on recent macrophytes at Lake Khamra is available. The only relevant study is by Baisheva et al. (2024), which analysed macrophytes using sedaDNA at Lake Khamra. However, this record extends back to the Late Pleistocene and has a much lower resolution, providing no information on the current macrophyte composition.
- Line 408: suggestions accepted, done.
- Line 410: We slightly restructured this chapter, deleted this sentence and discuss Hill Numbers in a later part of the discussion.
- Line 429: sentence has been deleted.
- Line 459/460: sentence has been deleted.
- Line 467: chapter has been restructured and shortened, sentence has been deleted.
- Line 472-474: sentence has been deleted.
- Line 482: We agree that there was some repetition with the results part. We restructured the chapters, sentence was deleted.
- Line 485: Sentence was deleted due to restructuring.
- Line 493: We discuss $\delta^{30}Si_{diatom}$ in the biogeochemical section of the discussion, also in relation to abundance changes of *Aulacoseira.*
- Line 500-503: Sentence was deleted.
- Line 507 & 508: replaced the word 'significant' (2x) as it was not tested statistically.
- Line 525: deleted the word 'precisely'.
- Line 540: Sentence has been deleted.
- Line 542: *F.cf.gracilis* was renamed to *F. radians*, a planktonic species.
- Line 550: replaced the word 'significantly' with 'notably'.
- Line 551: We agree and deleted this part of the sentence ('overprinting the influence of a simultaneous precipitation increase.').
- Line 567: replaced 'significantly' with 'substantially'.
- Line 583: Sentence has been removed as DVC values are not part of the discussion anymore.
- Line 598: We agree and deleted the numbers to focus more on OCARs in the next sentence.
- Line 616 -618: We deleted the sentence.
- Line 665: We agree that the interpretation of $\delta^{30}Si_{diatom}$ at Lake Khamra is not straight forward or influenced by a single process. We rephrased our conclusions on $\delta^{30}Si_{diatom}$ accordingly: "*At Lake Khamra, $\delta^{30}Si_{diatom}$ appears to be influenced by multiple factors, including shifts in diatom assemblages and catchment alterations that affect silica sources and utilisation."*
- Line 697: We agree and changed the heading for the section to: 'Anthropogenic influence on Lake Khamra linked to atmospheric pollution'
- Line 698: deleted 'significant' and provided more information on Hg levels from previous study Stieg et al. 2024b.
- Line 703: Interpretation of *Mallomonas* has been revised and paragraph has been restructured.