Review of the manuscript submitted to Climate of the Past by Amelie Stieg and colleagues:
Hydroclimate extreme events detected by a sub-decadal diatom oxygen isotope record of the last 220 years from Lake Khamra, Siberia

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RC1: 'Comment on cp-2023-85', Anonymous Referee #1, 13 Nov 2023

The manuscript by Stieg et al is focuses on new paleoclimate data (mainly diatom d18O) from a small lake (Lake Khamra) in eastern central Siberia. Overall, there is currently a lack of paleo records from the region (especially over the last two hundred years), so this new data will make a valuable contribution to the discipline. However, there are a number of issues that the authors need to consider before the manuscript can be accepted.

- Currently the paper is very long and has a number of very short paragraphs that are closer to (long) bullet points rather than a normal paragraph. I encourage the authors to examine this and to also consider ways in which the manuscript could be written more concisely in all sections.

ANSWER: We have complied with the request to write the manuscript more concisely overall and to revise its structure. At the current status the text has been shortened by about 9 %.

- The methodology describes the diatom taxonomy counts that were completed. However, only the results of one taxa are shown in the Results/Discussion sections. The full diatom record should be published and discussed, with the data made available in the Supplementary Information.

ANSWER: We understand the request to present and discuss the entire diatom data set. However, the full diatom record, including all counts, will be discussed in detail in a second manuscript focussing on bioproductivity at Lake Khamra, as a part of the PhD thesis of A. Stieg. Due to given reason we decided to exclude the data referring to the diatom taxa in the present manuscript. As a consequence, we also excluded the diatom ratio as well as the abundance of *A.subarctica* in Figure 5. We use the information on the planktonic dominance of the diatom data and its main diatom species *A. subarctica* only once in the discussion part (5.2) to compare the reconstructed water temperatures with the growth conditions of this main diatom species:

"*The calculated water temperature range (+4.8 to +8.7 °C) seems realistic in the early summer months when the ice cover of Lake Khamra starts to melt (May is the first month in which monthly mean Tair>0°C, 1928-2019CE, Vitim, Fig. 2). Further analyses on the short core EN18232-1 including diatom assemblages, which will be the focus of a following-up study and are not included here, have so far shown that planktonic diatom species dominate in abundance. Overall, the planktonic diatom genera Aulacoseira (Thwaites) is by far the most abundant and consists mainly of the planktonic species Aulacoseira subarctica (O. Müller), which growth conditions (Gibson et al., 2003) agree very well with the reconstructed water temperature range. It is assumed that in lake systems the effect of diatom species-dependent isotope fractionation, the so called vital-effect, is negligible (Shemesh and Peteet, 1998; Rosqvist et al., 1999; Shemesh et al., 2001; Leng and Barker, 2006; Chapligin et al., 2012b). Since the genus Aulacoseira builds up heavy and highly silicified diatom frustules (Laing and Smol, 2003), it likely also dominates the diatom biomass. Other diatom species in the assemblage probably have smaller effects on the biogenic silica production, and hence on the isotopic signal.*"

- The ecology of Aulacoseira subarctica is linked (indirectly) to wildfires. The case for this is not explained well in the manuscript. In places the ecology of the diatom is linked to Krammer et al., 1991, but this reference is a taxonomic guide. A more appropriate and detailed description of this taxa is needed.

ANSWER: As explained in the previous answer, we have decided to exclude all taxa-specific relations in the present manuscript. Nevertheless, we will consider the helpful comment and include appropriate references in the upcoming manuscript to better explain the ecology of *A. subarctica*.

- The Hg data should be expressed as an accumulation rate rather than as a concentration. The authors then need to make a clearer justification for how this data is linked (for some time periods) to regional processes such as wildfires, rather than global atmospheric deposition.

ANSWER: As suggested by the reviewer, the Hg accumulation rates (HgAR, µg/m²/a) were calculated additionally. For this purpose, the sedimentation rate (SR) and the mass accumulation rate (MAR) were determined beforehand (equations can be found below). The SR is based on the age-depth-model results by using the mean ages. The water content of the short core EN18232-1 is high (82-94%), especially in the upper part since 1930CE (88-94%), where THg increases rapidly. The dry bulk density data of the short core is quite variable, which might be a result of the high and fluctuating water content. A homogenisation of the entire wet sample material prior to density determination was not possible due to certain non-destructive analyses, such as the diatom assemblages. Therefore, we decided to calculate and use the mean dry bulk density of the short core to calculate HgAR:

Sedimentation rate (SR) in cm a^{-1} according to Pfalz et al. 2022:
$SR(x_i) = depth(x_i)-depth(x_{i-1}) / age(x_i)-age(x_{i-1})$

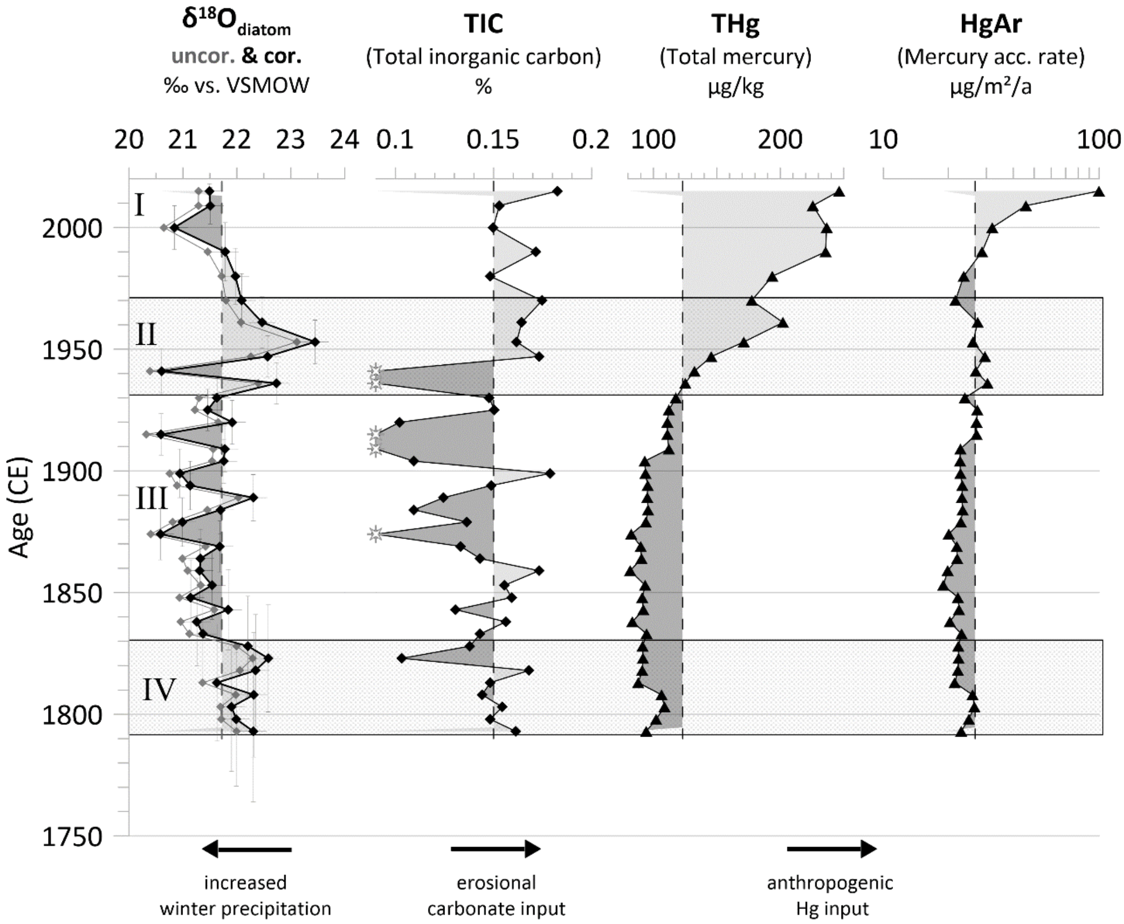
x_i = sample depth
x_{i-1} = previous layer

Following calculation according to Biskaborn et al. 2023:
Mass accumulation rate (MAR) in g cm^{-1} a^{-1}
MAR = DBD * SR

DBD = Dry bulk density (mean value), in g cm^{-3}

Mercury accumulation rate (HgAR) µg m^{-2} a^{-1}
HgAR = THg * MAR*10

The calculation of the HgAr has now been included in the methods part and the data is presented in a revised Figure 5 (see below). As for THg, we identify a rapid increase of HgAR especially since the 1970s, which we link mainly with an anthropogenic impact by air fallout on this remote ecosystem. Furthermore, the rapid increase in the last centuries is comparable to measurements made at Lake Baikal (Roberts et al., 2020, see further answers given to Rev#2).



Revised ‚Figure 5' including HgAr data beside THg and excluded the diatom ratios and the abundance of A. subarctica.

We decided to keep the total mercury concentrations (THg, µg/kg) as well, to allow comparison with other cited studies in the text (e.g. Biskaborn et al. 2021; Rutkowski et al. 2021). Furthermore, THg might be valuable in our study, as we follow a gapless approach and THg was measured directly on the identical sample material in line with all other proxies, independent from any potential age uncertainties. Moreover, the age-depth model, dated with ^{210}Pb-^{137}Cs, indicates a continuous age increase with depth, and mixing of material seems rather unlikely, which increases the reliability of the mercury concentrations measured of each sample.

We rephrased the following paragraph in the discussion part, linking mercury concentrations and accumulation rates with possible regional processes such as wildfires, and added a new reference (Driscoll, 2013):

"*Additionally, the first peak in THg around 1960CE (Fig. 5) might be related to changed conditions in the catchment area. Anthropogenic mercury inputs from increasing industrial combustion, transported by air as discussed in Sect. 5.3., is likely the main Hg source. Mercury can not only be released into the air from burning plant material (Driscoll et al., 2013), but can also accumulate in burned soils where it could subsequently be transported to the lake by erosion (Burke et al., 2010). For the first time within the Lake Khamra record, the accumulation rates rise above the mean (26.6 µg/m²/a) between 1930CE and 1950CE, reaching 30.4 µg/m²/a (1936CE). However, since the increase in accumulation rate is rather small, we assume the main Hg accumulation still is linked to the atmospheric Hg deposition from anthropogenic sources.*"

Driscoll, C. T., Mason, R. P., Chan, H. M., Jacob, D. J., and Pirrone, N.: Mercury as a Global Pollutant: Sources, Pathways, and Effects, Environmental Science & Technology, 47, 4967-4983, 10.1021/es305071v, 2013.

Roberts, S., Adams, J. K., Mackay, A. W., Swann, G. E. A., McGowan, S., Rose, N. L., Panizzo, V., Yang, H., Vologina, E., Sturm, M., and Shchetnikov, A. A.: Mercury loading within the Selenga River basin and Lake Baikal, Siberia, Environ Pollut, 259, 113814, 10.1016/j.envpol.2019.113814, 2020.

Minor points:

- The introduction could refer to Figure 1, to help individuals not familiar with locations discussed in that section.

ANSWER: Thank you. The figure reference has been inserted at appropriate positions in the introduction.

- The three lines of text above Figure 2 can be removed and instead added to the caption.

ANSWER: We agree. The three lines of text in Figure 2 have been removed and are included in the text below the Figure 2 (but not in the caption to avoid repetition).

- Lines 132-136: This information would be better if moved to the Methods.

ANSWER: As suggested, the information on meteorological data has been moved to the methods, and included in Sect. 3.1 which was renamed to "*Water isotope samples of Lake Khamra and meteorological data*".

- Line 159-160: Is this text needed?

ANSWER: We agree, the information in this sentence is not absolutely necessary to understand the data. We removed the sentence.

- Line 229-231: Remove this text as no silicon isotope data is included in the manuscript.

ANSWER: This is correct, we agree and the sentence has been removed.

- Line 235: Does the d18O data of 16.1‰ on the three samples refer to a 100% contamination sample obtained from the SPT residues? The text in this paragraph is not clear.

ANSWER: Yes, this is correct. We assume Rev #1 refers to the sentence in line 245. The text in the paragraph has been reworded to provide more clarity:

"*Contamination assessment of all processed samples (n=39), was carried out by a JEOL M-IT500HR analytical scanning electron microscope (SEM) with an integrated Energy-Dispersive X-ray Spectroscopy (EDS) system supplied with a Peltier element cooled SD detector (SDD). (…) Detected elements are given as oxides with weight percentages. The focus is on the SiO_2 content as an indication of the purity of the processed sample and on Al_2O_3 as an indicator of contamination with clay fractions.*

(…)

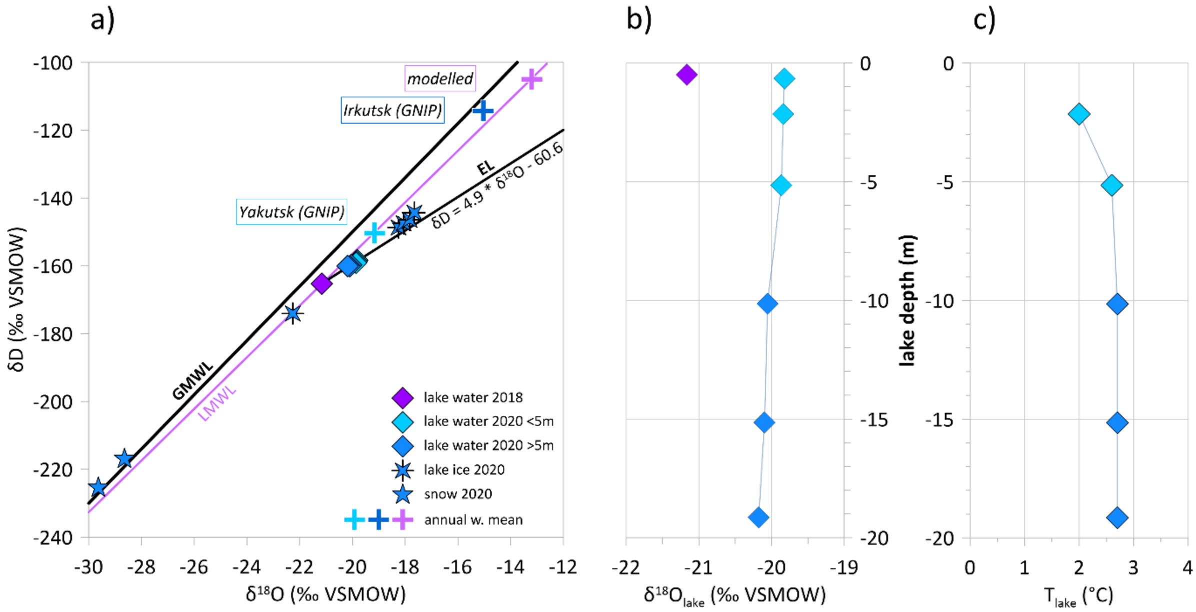
"*All $\delta^{18}O$ measurements ($\delta^{18}O_{meas}$) were contamination corrected ($\delta^{18}O_{corr}$) following a geochemical mass-balance approach (Brewer et al., 2008; Swann and Leng, 2009; Chapligin et al., 2012a):*

$$\delta^{18}O_{corr} = (\delta^{18}O_{meas} - \frac{c_{cont} * \delta^{18}O_{cont}}{100}) / (\frac{c_{diatom}}{100}) , \quad (1)$$

where $\delta^{18}O_{cont}$ represents the average $\delta^{18}O$ value of three samples of the heavy minerogenic fraction from the first heavy liquid separation of >2.50 g/cm³ ($\delta^{18}O_{cont}$ = +16.1 ±0.40‰, n=3), assumed as 100% of contamination. The percentages of contamination (c_{cont}) is based on the individual Al_2O_3 content of each sample divided by the mean Al_2O_3 content of the contaminants (11.3 ±0.39% Al_2O_3, n=3) using the EDS results, where c_{diatom} gives the degree of purity (100% - c_{cont})."

- Figures 3a and 6 could be combined.

ANSWER: As suggested be Rev #1, the figures have been combined (see Figure 2 below). As a consequence, Figure 6 has been removed, which reduces the number of figures to seven, instead of eight.



Revised Figure 3 combined with former Figure 6.

RC2: 'Comment on cp-2023-85', Anson Mackay, 01 Dec 2023

General comments

This study presents original, new diatom isotope data from a small Siberian lake in SW Yakutia. The quality of the isotope data is excellent; what few contaminants there were in the sediments, these have been taken account of robustly. The palaeolimnological record itself has been dated using 210Pb/137Cs analyses, and therefore the record presented is one of the few diatom isotope records that span the past couple of hundred years in relatively high resolution (compared to other Holocene records). The data therefore offer important insights in palaeohydrology in cold regions of the world.

The dating is robust, and I agree with their decision to the discard the radiocarbon dates. The interpretation of the likely control on the isotope values is methodical and supported by the data.

There were however a few instances where I thought the authors could have done more with some of the data presented, and where I thought the authors have over-interpreted their data especially in sections of the discussion that I detail below. My main suggestions given below are for the authors to both include more of the data they already have (diatoms) and to present some of the data in a form that is more useful for e.g. pollution histories, by taking into account sediment accumulation rates. Minor comments and corrections are included in the attached PDF

Specific questions / issues

Quality of the isotope data are excellent, both in terms of the purity of the samples obtained, but also the rigorous approach to determining contamination. Lines 249-250, the authors present data as z-scores to highlight any extreme values. But whilst this is very useful, I would not conflate an extreme value as being indicative of an extreme event, which is done in the discussion, notably Section 5.7.

ANSWER: Thank you very much for this comment and for approving the reliability of our diatom isotope data. We are aware that an identified extreme value in the isotope record (via z-score) is not to be considered equal with a hydroclimate extreme event. The extreme value in the isotope record is to be seen as the 'impact' of a possible hydroclimate extreme event. We agree that this was not always stated as clearly in the text (e.g. Lines 249-250).

Based on the discussed hydrological correlation of the oxygen isotope data (especially with winter precipitation), it can be assumed that hydroclimatic variability is likely decisive for the minima/maxima values in our diatom record. Further internal and external records are used to validate whether a hydroclimate variability could have led to this extreme value. Through the comparison especially with seasonal, meteorological data (at least in the younger section of the record), as well as an independent reconstruction of regional forest fires at a parallel core of the lake, we conclude two possible dry phases (likely modulated through a reduction of winter precipitation) led to these maxima values in the diatom record.

Our diatom isotope record has already a very high resolution (sub-decadal) compared to other studies, but even this resolution does not allow a more detailed investigation of the hydroclimatic past. We agree that the term 'hydroclimatic extreme events' should be evaluated statistically, which is difficult for an area with low data availability. We have therefore decided not to use the term 'hydroclimate extreme events' in our diatom record and removed the term from the manuscript to reduce any confusion. Instead we present the diatom isotope record as a new valuable hydroclimate proxy for the region and refer to “hydroclimate anomalies” instead, linked here with winter precipitation variability. We have adapted corresponding text passages and the title accordingly.

Age model:

- As we can see from Table 2, another reason for omitting the 14C dates from the age model are that the samples get progressively younger (not older) the deeper in the core esp between 25-26 and 34-35, so either there has been in inversion (but that is not borne out by the 210Pb/137Cs record) or the dates are indeed affected by other processes as discussed.

ANSWER: Thank you for this further argument for omitting the ^{14}C dates. We have added this in the text within the section '4.2 Age-depth model' as follows:

"The four ^{14}C ages of the bulk sediment show a clear offset to the ^{210}Pb and ^{137}Cs chronology, with significant older ages that nearly progressively get younger with depth (see Table 2).

(...)

Possible reasons were discussed such as an influence of dissolved carbonates, the so called hard-water effect (Björck and Wohlfarth, 2001; Philippsen, 2013), input of old organic carbon (Colman et al., 1996; Vyse et al., 2020) or mixing processes within the sediment (Biskaborn et al., 2012) which could have led to discontinuous ^{14}C age development with depth. However, since the ^{210}Pb and ^{137}Cs dating results are very uniform to a depth of 26 cm and no striking hiatus could be detected in the record, a mixing process is rather unlikely."

TIC data:

- Fig 5 shows that the TIC data in the core vary between only 0.1% - 0.2%, yet values of up to 1.8% are quoted, so I think the scale used on Fig 5 is wrong for TIC. Nevertheless, even small increases to 1.8% show that the amounts of DIC present in the sediment are very low, and too low in my opinion, to make robust interpretations as to what changes in relative abundance means in terms of erosion of material into the lake. For example, an increase from 1 to 1.8% would not be robust enough.
- Erosion is probably better interpreted from other proxies such as % dry weight to magnetic susceptibility measurements (but these do not form part of this study).

ANSWER: Thank you for this notification. The scale for TIC in Figure 5 is correct. Unfortunately, a typing error has been made in the result description which we corrected:

"*From the onset of the record at about 1790CE to the 1950s, TIC measurements remain constant at or slightly below the overall mean of 0.15% (Fig. 5). At ca. 1875CE, 1910CE and 1940CE five samples are below the detection limit of <0.1%. At about 1950CE, TIC switch to above mean values, including a maximum of 0.18% in the uppermost sample."*

We are aware that the TIC values of the short core are very low. Nevertheless, we see comparable values in a deglaciation period recorded by a sediment record of Lake Bolshoye Shchuchye in the polar Urals (Lenz et al., 2021). We decided to interpret TIC as a possible proxy for erosion more cautiously in all sections and provide some background information on TIC (please see ANSWER to Line 610 – 620).

Lenz, M. M., Andreev, A., Nazarova, L., Syrykh, L. S., Scheidt, S., Haflidason, H., Meyer, H., Brill, D., Wagner, B., Gromig, R., Lenz, M., Rolf, C., Kuhn, G., Fedorov, G., Svendsen, J. I., and Melles, M.: Climate, glacial and vegetation history of the polar Ural Mountains since c. 27 cal ka BP, inferred from a 54 m long sediment core from Lake Bolshoye Shchuchye, Journal of Quaternary Science, 37, 818-835, 10.1002/jqs.3400, 2021.

Mercury data:

- as the authors have a robust radiometric age model, I'd recommend replotting the Hg data as fluxes as well, as these will be more directly comparable to other studies with respect to interpreting any increasing or decreasing trends in pollution.

ANSWER: This comment overlaps with the suggestions of Rev#1. As discussed in the answer for Rev#1, we agree and decided to present Hg fluxes (HgAr) in addition to the mercury concentrations and plot them in combination (please see the revised Figure 5 above) as we consider both valuable. For a precise description of the calculation, please see the answer given to Rev#1.

Diatom data:

- Diatom methods are well accounted for.
- However, ideally a comprehensive diatom stratigraphy needs to be shown not just *subarctica* only. Remember that because these are relative abundance data, the decline or increase in any one species is also tied to other species in the dataset.
- For example, on lines 517-518, I don't think that this is a valid conclusion without knowing (i) what the other species are, and (ii) relative contributions of other abundant species to either total biomass (or preferably biomass accumulation rates). For example, in our paper Mackay et al. 2013 that you quote, a non-dominant species in terms of relative abundance (*Stephanodiscus grandis*) completely dominates the biomass of the diatom flora, leading to those authors concluding that changing assemblage composition alone in terms of relative abundance is likely to have little impact on the isotopic variation.
- Quite a lot of interpretation is made with respect to changing P/B ratios, and sometimes interpretations made are a bit circular. For example, in lines 659-660 the authors state "…*overall rather good conditions for planktonic diatom species, comparable to the abundance of A. subarctica*", but isn't *subarctica* responsible for most of the variation in P/B ratio in the first place?
- I'd be really interested to see if any fragilarioids are present in the assemblage – many of these will be tychoplanktonic so can live in both in littoral and deeper open waters, which would add in a different dimension to the interpretations given here.

ANSWER: As requested by Rev#1, we understand and agree with the request to discuss the complete diatom data set of the short core EN18232-1 in order to make comprehensive statements related to taxonomy. However, in the course of the PhD thesis of A. Stieg a second manuscript is currently in preparation, including the full diatom assemblage and further lake internal proxies, to investigate changes in bioproductivity in this pristine lake ecosystem in respect of recent climate change.

We understand that displaying any data on the relative abundance of only one diatom species leads to a demand. Therefore, we have decided to exclude the complete diatom data in the present manuscript to discuss the full diatom taxonomy in the follow-up study. We thank you for the suggestions regarding the diatom assemblage, which will be considered in the next study.

With regard to line 517-518, we have adjusted the text as we still see an advantage in mentioning the dominant diatom genus (species) *Aulacoseira (subarctica)* for the interpretation of the isotopic signal. However, on basis of your input we changed our argumentation of a negligible vital-effect based on the biomass dominating species due to the heavy silicified diatom frustules instead of arguing with a very weak positive correlation between $\delta^{18}O_{diatom}$ and the relative abundance of *A. subarctica*. This corresponding paragraph is inserted below and is in line with the response to Rev#1:

"*The calculated water temperature range (+4.8 to +8.7 °C) seems realistic in the early summer months when the ice cover of Lake Khamra starts to melt (May is the first month in which monthly mean Tair>0°C, 1928-2019CE, Vitim, Fig. 2). Further analyses on the short core EN18232-1 including diatom assemblages, which will be the focus of a following-up study and are not included here, have so far shown that planktonic diatom species dominate in abundance. Overall, the planktonic diatom genera Aulacoseira (Thwaites) is by far the most abundant and consists mainly of the planktonic species Aulacoseira subarctica (O. Müller), which growth conditions (Gibson et al., 2003) agree very well with the reconstructed water temperature range. It is assumed that in lake systems the effect of diatom species-dependent isotope fractionation, the so called vital-effect, is negligible (Shemesh and Peteet, 1998; Rosqvist et al., 1999; Shemesh et al., 2001; Leng and Barker, 2006; Chapligin et al., 2012b). Since* *the genus Aulacoseira builds up heavy and highly silicified diatom frustules (Laing and Smol, 2003), it likely also dominates the diatom biomass. Other diatom species in the assemblage probably have smaller effects on the biogenic silica production, and hence on the isotopic signal."*

Technical corrections & issues of interpretation

Lines 50-51: I think the statement about extreme events being linked to ocean currents and large-scale dynamics needs a source.

ANSWER: We agree and following references have been added to the sentence: "*Hydroclimate extreme events, including drought periods or intensified precipitation, are linked with ocean currents and large-scale dynamics* (Churakova Sidorova et al., 2021; Marshall, 2021; Seneviratne et al., 2021; Watanabe et al., 2023)*."*

Line 53: the citation Ye et al. 1998 is perhaps too old for the statement "recent decades" given the timescales being considered here. Is there a more recent study that can be used instead of, or as well as?

ANSWER: We agree. The following references have been added to the sentence: *"The hydroclimate across Siberia has undergone notable alterations in recent decades, but differs regionally. While snow depth decreased over most southern parts of Russia, it increased in the northern parts including northern central Siberia (Ye et al., 1998;* Bulygina et al., 2009; Ghatak et al., 2012; Sato et al., 2022*)."*

Lines 275: the statement "…*the uncertain species were counted once as benthic (ratio 1) and once as planktonic (ratio 2) species*" is an odd thing to do. Could uncertain taxa be tychoplanktonic taxa? But as it goes, it's not possible to evaluate this statement without knowing what the diatoms are. Most species habitats are actually known.

ANSWER: 'Uncertain' was probably phrased misleading. We meant species that are facultatively planktonic. However, as discussed above we decided to exclude the diatom taxonomy data in the present manuscript including the diatom ratios. Nevertheless, we will revise the habitat preference carefully before publishing the diatom assemblage data in a second manuscript.

Line 355 (+ others), when quoting dates probably better to give these as approximate dates given uncertainty in the age model, e.g. c. 1940 CE, 1915 CE and 1875 CE (should dates be expressed as Common Era?)

ANSWER: Thank you. We agree in expressing the dates as Common Era (CE) and added this where it was necessary and expressed specific dates as approximate dates.

Lines 395 / Figure 5: I wonder if it would not be better to align all the evidence for increased erosion either left to right (my preference) or right to left. If former chosen, then reverse x-axis for the diatoms. But for me it's not clear how changing abundance of *A. subarctica* (or P/B) could be both erosion and nutrients, and how the very small changes in P/B (1) can be anything really.

ANSWER: We revised Figure 5 (please see above) and excluded all diatom taxonomy related data.

Lines 578-579: The statement "*This signifies an offset in seasonality in our diatom record, as the winter season and its snowfall do not affect the lake water and the diatoms therein before the following summer*" is unlikely to be true - where snow is falling there may be reduced biological activity over winter, but as Hampton et al. 2017* show, plankton under ice-covered lakes were more abundant than expected, and that winter conditions act as a strong antecedent to conditions the following summer (this might also give a more nuanced account for interpretations made in lines 628-629)

ANSWER: We do agree, that snow cover and ice transparency can have an influence on bioproductivity below the ice, as discussed in Hampton et al. 2017. However, the wording was probably not clear. In this section, we meant the relationship between snowmelt water and the diatom isotope signal, as the meltwater only influences the isotope signal of the lake water and hence the diatom isotopy in the following year. We cited comparable studies which also observed an effect of snowmelt water on diatom isotopy:

"*Most precipitation during SON season is expected to fall as snow (monthly mean T_{air} Vitim: September: 6.4°C; October: -3.3°C; November: -18.0°C; 1929-2018CE) with highly depleted $\delta^{18}O$ values (Fig 3a). The snow can accumulate in the catchment and lead to a depletion of $\delta^{18}O_{lake}$ and, thus, of $\delta^{18}O_{diatom}$ when it reaches the lake as meltwater in the next spring season. The influence of snowmelt and its varying amount as controlling factor of the diatom isotopy has been observed in other lacustrine $\delta^{18}O_{diatom}$ records in northern latitudes (Mackay et al., 2013; Rosqvist et al., 2013; Broadman et al., 2022; Meyer et al., 2022).*"

Line 600: This is not in the scope of this study, but future work might involve some electron microscopy to identify the carbonate minerals in the sediments. But I do agree that old carbon being in-washed into the lake would impact the radiocarbon dates as observed.

ANSWER: Thank you for your suggestion to take a closer look on the aspect of carbonate minerals in the sediment. Unfortunately, there is no original sample material from this short core to carry out these analyses. Nevertheless, we agree it would be interesting to analyse carbonate minerals in the sediment of Lake Khamra, which could be done on parallel sediment cores, but would go beyond the scope of the study.

Lines 608-609: Might also be interesting to compare to Baikal (eg Roberts et al. 2020*) but also to represent the data as fluxes, taking into account SARs (see my comments above). This would give more nuance as to if Hg is coming from e.g. Russian or Asian sources.

ANSWER: Thank you very much for this suggestion. We compared the mercury fluxes to the ones of Lake Baikal (Roberts et al., 2020) and observe similarities, especially with the south basin and the Selenga Delta. Additionally, we see a possible linkage between rising Hg fluxes at Lake Khamra and the Hg emissions since the 1990s, originating from the Asian industrialisation. We added the corresponding references. For comparison of the values please see the answers to the comments of your pdf below.

"The mercury accumulation rates (HgAr) show a more than fourfold increase in phase I, corresponding to mercury fluxes observed at Lake Baikal post 1850CE, especially in the south basin and the Selenga Delta (Roberts et al., 2020). We assume that the rising HgAr at Lake Khamra, especially since the 1990s (Figure 5), may be correlated with the concurrent industrialisation in Asia, dominated by China and India, and the associated Hg emissions (Pacyna et al., 2016; Sundseth et al., 2017), as discussed in Roberts et al. (2020)."

Lines 610-620: I found this paragraph quite speculative (i) without showing a more detailed diatom stratigraphy, (ii) assuming that the really small changes in in DIC are related to erosion into the lake. I really don't think the data allow for this interpretation.

ANSWER: We agree with this comment. As mentioned above, we removed all diatom related data and corresponding discussion parts in this manuscript. Nevertheless, we rephrased our argumentation according to TIC values and provide further information for possible sources as follows:

"Increased snowmelt runoff might increase (soil) erosion within Lake Khamra's catchment. Occurrence of dolomite and limestone in the bedrock (Chelnokova et al., 1988) could be a possible source for total inorganic carbon (TIC) in the sediment (Fig. 5), probably contributing to the observed ^{14}C age offset (see 4.2). A study of the Russian lake Bolshoye Shchuchye uses elevated TIC values as a proxy for erosion and sediment supply from detrital carbonate in the bedrock (Lenz et al., 2021). Overall, the measured TIC values at Lake Khamra are low, but the consistently above-average TIC values in phase I are comparable to a deglaciation period described at Lake Bolshoye Shchuchye, supporting our argument of possible erosional input from the catchment (Fig. 5). Nevertheless, there are possible other sources for TIC in lake sediments, like biogenic carbonate sources, including carbonate fossils. As they are absent in the Khamra sediment samples it is rather unlikely as TIC source. Since the pH value (pH=6.07) of the modern Khamra lake indicates rather acidic water and the lake water temperature is rather cold, an autochthonous calcite precipitation as source of TIC (Cohen et al., 2003) seems rather unlikely, too. However, to exclude the presence of carbonate minerals with certainty, an SEM analysis of the sediment would be required (Last and Smol, 2001), which is beyond the scope of this study."

"TIC shows a shift from below to above detection limit at around 1950CE, almost simultaneously with the change from a minimum to a maximum in $\delta^{18}O_{diatom}$ (Fig. 5). However, an assumed enhanced erosional input from the catchment linked with elevated TIC, cannot be explained by the observed overall precipitation deficit in phase II."

Section 5.5: Overall in this section, I thought that there was over-interpretation and sometimes contradictory explanation of the diatom data in comparison to other proxies / archives selected.

ANSWER: Thank you for carefully reading our manuscript. We will take the opportunity to rephrase our argumentation, when interpreting the diatom data in a subsequent manuscript. In the present, we removed all diatom related data and corresponding discussion parts. Furthermore, we formulated the interpretation of the other internal proxies more cautiously.

Line 704: to be honest, I struggle to see a well-defined maximum here, leading to risk of over-interpreting what the datasets may indicate

ANSWER: We agree that a maximum cannot be clearly identified in the figure alone. In our argumentation we refer to the description within the publication of Glückler et al, 2021, in which the results are described as follows: "*Similarly, the robust CHAR sum and its components show increases within phase 4 (Fig. 3d, e), with two maxima in the robust peak component around the early 1800s and 1950CE and a following decrease in CHAR (Fig. 3d–e)."*

Lines 757-758: doesn't this statement contradict the interpretations given in Lines 705-710?

ANSWER: Thank you, we realised that we probably have not expressed ourselves clearly enough in this paragraph. We rephrased it to provide more clarity:

"*Although the temperature and insolation trends in phase II (rather warm, high insolation) and IV (rather cool, low insolation) are opposite, but in both phases the Khamra $\delta^{18}O_{diatom}$ values are isotopically enriched, we conclude that temperature and insolation likely play a minor role as prevailing influence on our diatom record. The good agreement of the Khamra $\delta^{18}O_{diatom}$ record with the local charcoal record and the winter NAO index contribute to identify past hydroclimate variability. This strengthens the argumentation that variations in precipitation drive the $\delta^{18}O_{diatom}$ signal, extending to the earlier phases prior to meteorological observations."*

Section 5.6: this is a useful section and quite robustly discussed

ANSWER: Thank you for this positive feedback!

Section 5.7: Given the nature of the data, I'm not convinced that a separate section on identifying extreme events is warranted, and much of the discussion could be absorbed into previous sections. Also, identifying extreme events is usually done using sometime kind of statistical analyses. It is the case that z-scores are used to highlight extreme values (e.g. = ± 3) but an extreme value is not an extreme event.

ANSWER: Please check the answer given to your first comment on this topic, thank you.

I found that some of the interpretations were not supported by the data. For example, on lines 870-871, sedimentary TIC (especially at these concentrations) is not a measure of water quality, and would erosion lead to an increase in salinity per se? This is not what's observed in other lakes. An increase in erosion may lead to an increase in sediment accumulation rates, but these are not estimated here.

ANSWER: As answered in the comments above we decided to exclude diatom taxonomy related argumentation and rephrased our discussion on TIC values carefully.

Line 876: but is the charcoal peak really that pronounced at around 1800? It looks like a small gentle increase to be, and certainly not part of any extreme event

ANSWER: The ‘classic’ charcoal record is not plotted in any figure of the manuscript and the argumentation on the charcoal peak at around 1880CE relies on the publication of Glückler et al., 2021 and information given there in: “*An outstanding peak around 1880CE (…)”,* “*Together with the potential of more intense fires producing larger charcoal particles (Ward and Hardy, 1991), this could mean the two previously noted peaks of CHAR (ca. 1880CE at 19.5–20.5 cm depth and 650CE at 124.5–125 cm; both dominated by high shares of type F particles >500 µm) are evidence of higher intensity fires burning conifer trees more severely and within a few kilometres of the lake shore.”*

We agree that not showing the ‘classic’ charcoal record causes confusion, and since we related this peak to the relative abundance of *A. subarctica* we decided to remove this sentence and only focus on the “robust CHAR” charcoal record, which is plotted in Figure 7.

*Papers cited:

Hampton, S.E, Galloway, A.W.E., Powers, S.M., Ozersky, T., Woo, K.H., Batt, R.D., Labou, S.G., O’Reilly, C.M., Sharma, S., Lottig, N.R., Stanley, E.H., North, R.L., Stockwell, J.D., Adrian, R., Weyhenmeyer, G.A., Arvola, L., Baulch, H.M., Bertani, I., Bowman, L.L. Jr., Carey, C.C., Catalan, J., Colom-Montero, W., Domine, L.M., Felip, M., Granados, I., Gries, C., Grossart, H.-P., Haberman, J., Haldna, M., Hayden, B., Higgins, S.N., Jolley, J.C., Kahilainen, K.K., Kaup, E., Kehoe, M.J., MacIntyre, S., Mackay, A.W., Mariash, H.L., McKay, R.M., Nixdorf, B,. Nõges, P., Nõges, T., Palmer, M., Pierson, D.C., Post, D.M., Pruett, M.J., Rautio, M., Read, J.S., Roberts, S.L., Rücker, J., Sadro, S., Silow, E.A., Smith, D.E., Sterner, R.W., Swann, G.E.A., Timofeyev, M.A., Toro, M., Twiss, M.R., Vogt, R.J., Watson, S.B., Whiteford, E.J., Xenopoulos, M.A. (2017) Ecology under lake ice. *Ecological Letters*, 20, 98-111 doi: 10.1111/ele.12699

Roberts, S., Adams, J.K., Mackay, A.W., Swann, G.E.A., McGowan, S., Rose, N.L., Panizzo, V., Yang, H., Vologina, E., Sturm, S. Shchetnikov, A.A. (2020) Mercury loading within the Selenga River Basin and Lake Baikal, Siberia. *Environmental Pollution* 259, https://doi.org/10.1016/j.envpol.2019.113814

Changes according to comments in the pdf:

(Comments that are answered in the upper section already are not listed here again.)

- Line 45: sentences have been reworded, 'globe' is changed to 'world'.
- Line 58: "*in the country*" changed to "*in Russia*": "Despite, Yakutia is one of the most vulnerable regions for forest fires <u>in Russia</u> (Kirillina et al., 2020) and underwent its most severe fire season of the past forty years in 2021 (Tomshin and Solovyev, 2022), highlighting an area of extremes."
- Line 62: New paragraph was inserted.
- Line 82: Thank you for this suggestion. We assume the Rev#2 meant this publication:
    - Broadman, E., Kaufman, D. S., Henderson, A. C. G., Berg, E. E., Anderson, R. S., Leng, M. J., Stahnke, S. A., and Muñoz, S. E.: Multi-proxy evidence for millennial-scale changes in North Pacific Holocene hydroclimate from the Kenai Peninsula lowlands, south-central Alaska, Quaternary Science Reviews, 241, 10.1016/j.quascirev.2020.106420, 2020.

  It is correct that in includes a comparison between instrumental climate data and proxy data from Sunken Island Lake on Kenai Peninsula/ Alaska for the period of 1900-2018CE. The record is mainly P-E driven. The study location is quite close to the other cited study of Broadman et al. 2022, also located on Kenai Peninsula, which provide a higher-resolution data set from Kelly Lake. The second is shown in the map (Fig. 1). We included the reference (Broadman et al., 2020) in section 5.3 and 5.5 as further example.

- Line 204: The sentence has been combined with the following one and was reworded: "*<u>Relevant steps include</u> the removal of organic matter by adding H_2O_2 (30%, 50°C, ~55 h), followed by HCl (10%, 50°C, ~16 h) to eliminate carbonates.*"
- Line 254: The relevance for TIC measurements was added: "*The inorganic carbon (TIC) content was determined of all 39 subsamples, to analyse it as a proxy for sediment supply from the carbonate-bearing bedrock in the catchment, as outlined in Lenz et al. (2021).*"
- Line 275: The ratio of planktonic to benthic diatoms was removed.
- Line 291 + 293: isotopically "*heavier*" and "*lighter*" have been replaced by isotopically "*enriched*" and "*depleted*"
- Line 395 + 399: Figure 5 has been adapted and diatom related plots have been removed. Please find the detailed answer above in the corresponding comment.
- Line 404: Please refer to the answer regarding the TIC values further up. Regarding the TOC and THg values, we checked the correlation between THg and TOC by performing a spearman rank correlation, which revealed only a moderate positive correlation of +0.47, $p<0.01$. Therefore, mercury probably follows the organic content in the sediment as Hg binds to lake organic carbon but cannot be explained completely by it and the increase in THg is likely related to external influences, too (likely human air pollution). However, as we intend to use the TOC values in the context of the second study on bioproductivity, the TOC values are not shown in this manuscript.
- Line 409 – 411: Comparison with Roberts et al., 2020: Converted our HgAr data (µg/m²/a) by dividing the value by 10 to get the equivalent value in ng/cm²/yr, Post 1850 CE:
    - South Basin (BAIK13-10): 0.26 ng/cm^2/yr in 1910 CE to 6.32 ng/cm^2/yr in 2013 CE
    - North Basin (BAIK13-19): 0.38 ng/cm^2/yr in 1880 CE to 0.43 ng/cm^2/yr in 2013 CE
    - Selenga Delta (SLNG04): between c. 1945 CE and c. 1995 CE, from 2.3 to 11.0 ng/cm^2/yr. Since c. 1995 CE, Hg flux at SLNG04 has declined slightly to 8.1 ng/cm^2/yr
    - Lake Khamra: 18.9 – 99.8 µg/m^2 a (ca. 1790 – 2015 CE) → 1.89 – 9.98 ng/cm^2/yr

   - Khamra HgAr values are clearly above the ones of the north basin and mainly comparable to the ones of the south basin of Lake Baikal and the Selenga Delta, even though HgAr stays below the peak at ca. 1995 CE of the Selenga Delta record.

  It would be interesting to compare the fluxes though, as higher concentrations at Kharma may be tied to lower sediment accumulation rates?

   - in supplement of Roberts et al. 2020: sedimentation rates are actually mass accumulation rates (MAR) given in g/cm²/yr
   - Khamra mean MAR: 0.02 g/cm²/yr
   - much lower than Selenga delta (c. 0.1 – 0.2 g/cm²/yr); lower than southern basin especially in the younger part (c. 0.01- 0.13 g/cm²/yr), comparable to north basin (c. 0.005 – 0.02 g/cm²/yr)
   - so correct, the high mercury concentrations at Lake Khamra are linked with lower sedimentation rates as at the south basin of Lake Baikal.
   - We include this information in the discussion part, see Answer to line 608 - 609.
- Line 412 -415: We decided to exclude the diatom data.
- Line 421: ‘precise’ was replaced by ‘careful’.
- Line 427: changed to ‘also affects’.
- Line 432: changed to ‘with an outflow’.
- Line 464-466: Sentence was rephrased to provide more clarity: “*Hence, the inflow of melt water from either snow and/or glaciers in the catchment, which is isotopically light water, could lower $\delta^{18}O_{lake}$, as seen in other studies in Eurasia (Mackay et al., 2013; Meyer et al., 2015; Kostrova et al., 2021; Meyer et al., 2022).*”
- Line 527: “counterbalanced” replaced by “compensate”.
- Line 528: We clarified the statement: “As a consequence, T_{air} has a stronger effect on $\delta^{18}O_{prec}$, linked with $\delta^{18}O_{lake}$ at Lake Khamra, than T_{lake} and therefore on $\delta^{18}O_{diatom}$.”
- Line 535-536: “The distinct seasonality of $\delta^{18}O_{prec}$ highlights its strong T_{air} dependency at the Lake Khamra region, following the high annual T_{air} range of 47°C (Fig. 2).”
- Line 612-13: The diatom data was excluded.
- Line 632: Paragraph including diatom data was removed.
- Line 633-634: Paragraph including diatom data was removed. Thank you for your suggestions, we are happy to consider them for the upcoming manuscript.
- Line 638-641: The TIC shift was rephrased and expressed more carefully. Correct, TOC was measured at the same time. The TOC record will be part of the upcoming manuscript.
- Line 657 -661: Paragraph including diatom data was removed. However, we keep in mind the suggestions made for the follow-up study.
- Line 668-669: Thank you. We agree to avoid over-interpretation; hence, the sentence was removed.
- Line 702: Sentence was rephrased and ‘$\delta^{18}O_{diatom}$‘ was inserted.
- Line 863: We excluded this sentence.