

B. Chaplignin et al., A 250 ka oxygen isotope record from diatoms at Lake El'gygytgyn, far east Russian Arctic, *Clim. Past Discuss.*, 8, 1169-1207, 2012

Revised manuscript – Response to reviewers/referees

Answers to all comments are given below each comment in grey. Any sentences that have been added in the revised version due to these comments are marked yellow below.

Reviewer1: Anonymous

P1186, 116

Authors indicated that the $\delta^{18}\text{O}_{\text{diatom}}$ peaks often occur earlier than TiO_2 minima and or BSi maxima percentages and attributed that to delay of clastic sediment supply or productivity proxy records. However, I wonder the change of sedimentation or nutrient availability could delay from the atmospheric signal in the small lake. Authors might be able to discuss not only the residence time of water but also the nutrient availability response.

→ The change of $\delta^{18}\text{O}_{\text{diatom}}$ is dependent on the change of $\delta^{18}\text{O}_{\text{water}}$. The nutrient availability is responsible for the BSi [%] changes. It is not proven that different nutrient availability results in a different $\delta^{18}\text{O}_{\text{diatom}}$. Hence, we added some words about the possibility that the BSi [%] peak might result from a delayed nutrient availability response compared to the reaction of the lake water (residence time): “(...) which could indicate a more direct atmospheric signal responsible for $\delta^{18}\text{O}$ while there is a delayed reaction in the more indirect proxy records due to subsequent weathering (TiO_2) and nutrient availability (BSi).”

P1186, 122, P1188, 114, 125

The $\delta^{18}\text{O}_{\text{diatom}}$ was compared with LR04 or NGRIP. The age model of the studied core is made from magnetic susceptibility. If the $\delta^{18}\text{O}_{\text{diatom}}$ well correlated with these stacked curve or ice core records as authors mentioned, $\delta^{18}\text{O}_{\text{diatom}}$ variation is probably more suitable to make age model. Thus, the discussion about the timing between $\delta^{18}\text{O}_{\text{diatom}}$ and LR04 or NGRIP by using the present age model seems to be no meaning.

→ For the age model, the magnetic susceptibility was tuned to the insolation at 70°N and not to the $\delta^{18}\text{O}$ record, so this is “independent” from the LR04 or NGRIP record. The age model is well accepted and published by Melles et al. (*Science*, 2012) for the ICDP 5011-1 core composite (from which the first 5.6m were based on Lz1024). To our understanding, a good fit of these records with the $\delta^{18}\text{O}_{\text{diatom}}$ record from Lake El'gygytgyn supports the used age model. It is reasonable to assume a quicker response to atmospheric changes in lake systems compared to marine systems. Without comparing exact ages we see this from the abrupt/slow warming/cooling trends which are different in both records. This is stated in the text.

Melles, M., Brigham-Grette, J., Minyuk, P. S., Nowaczyk, N. R., Wennrich, V., DeConto, R. M., Anderson, P. M., Andreev, A. A., Coletti, A., Cook, T. L., Haltia-Hovi, E., Kukkonen, M., Lozhkin, A. V., Rosén, P., Tarasov, P., Vogel, H., and Wagner, B., 2012. 2.8 Million Years of Arctic Climate Change from Lake El'gygytgyn, NE Russia. *Science*, doi: 10.1126/science.1222135.

P1187, 121

The isotopic difference between the core top (+21.5‰) and Holocene Thermal Maximum ($\delta^{18}\text{O}_{\text{diatom}} = +23‰$ 8.9 ka) is about 1.5‰. If the $\delta^{18}\text{O}_{\text{diatom}}$ mainly reflect air temperature, it equals to 2.5°C of air temperature change. Is it reasonable for post HTM-cooling at the studied site?

→ In the mentioned line we describe that “A post-HTM cooling trend can be observed”. Post HTM-cooling trends were described in Swann et al. (2011) for Lake El’gygytgyn, in Popp et al. (2006) for North East Russia and in Bjune et al. (2009) for the Arctic Circle. Hence, a post-HTM cooling trend seems reasonable here as well. Generally, a potential temperature change of 2.5°C seems too high. However, we did not comment on the exact temperature change here as we discuss potential influences/variables (continentality, residence time,...) in detail when discussing the overall $\delta^{18}\text{O}_{\text{diatom}}$ amplitude (Page 1183, Line 5 and following paragraphs).

P1190, 14

Authors presented that there is no general trend between the relative Si-OH bonds percentage toward depth and oxygen isotopic compositions of diatom, and referred Moschen et al. (2006) that the rapid signal alteration during sedimentation is followed by only minor post-sedimentary diagenetic changes which are not detectable in the $\delta^{18}\text{O}$ data. I agree with author’s interpretation. However the main result of Moschen et al. (2006) is a silica dehydroxylation process as cause for the isotopic enrichment of the bottom sediment, and the isotopic compositions of the diatom on the bottom sediment and epilimnion is different. If authors would like to refer the Moschen et al. (2006), they should clearly note the possibility that the observed oxygen isotopic compositions might be rapidly altered value during settling and sedimentation.

→ We use only fossil diatoms from sediment material, but this is a good point. We agree with the reviewer here and added the required information in the text: “(...) A rapid signal alteration (early stage diagenesis, Moschen et al., 2006) during sedimentation can not be excluded. By this process, the $\delta^{18}\text{O}_{\text{diatom}}$ values would then reflect the deeper $\delta^{18}\text{O}_{\text{lake water}}$. However, the lake is well-mixed and no significant differences in $\delta^{18}\text{O}_{\text{lake water}}$ in the water profile can be observed (Chapligin et al., 2012). Hence, the results support the theory from Moschen et al. (2006) that a rapid signal alteration during sedimentation is followed by only minor post-sedimentary diagenetic changes which are not detectable in the $\delta^{18}\text{O}$ data (...).”

P1203, Fig.5

The figure is difficult to understand. Please add more information in the figure caption. For example, I could not understand what the up pointing arrow (Twater or Continentality) indicates.

→ We added some information in the figure caption for further clarity: “Fig. 5. $\delta^{18}\text{O}_{\text{diatom}}$ controls in the lacustrine environment. $\Delta^{18}\text{O}_{\text{uptake}}$ arrows mark direct fractionation mechanisms between $\delta^{18}\text{O}_{\text{diatom}}$ and $\delta^{18}\text{O}_{\text{lake water}}$ while $\Delta^{18}\text{O}_{\text{water}}$ arrows indicate atmospheric or hydrological processes influencing the $\delta^{18}\text{O}_{\text{lake water}}$ and thus, indirect mechanisms on $\delta^{18}\text{O}_{\text{diatom}}$. Small up pointing arrows indicate an increase of the respective parameter.”

Reviewer2: Anson Mackay

Introduction

P1171, Line 6: replace “an” with “a”; replace “emerging” with “growing”

→ corrected.

P1171, Line 14: Bezrukova did not measure any isotopic values. Also, this sentence does not really make much sense as $\delta^{18}\text{O}$ values are affected by a number of processes. It may be better to decompile this sentence. For example, we know that the majority of precipitation to the Lake Baikal region comes via the Westerlies during

summer months. In the lake itself, $\delta^{18}\text{O}$ is influenced mainly by inflowing rivers.

Furthermore, those with southern catchments have higher $\delta^{18}\text{O}$ values due to lower proportion of snow-melt (e.g. Afanasjev 1976; Seal and Shanks 1998).

→ We rephrased and split the sentence and corrected the citation: „However, Lake Baikal is located south of the Arctic circle with southern catchments having generally higher $\delta^{18}\text{O}$ values due to lower proportion of snow-melt (e.g. Seal and Shanks 1998). Though the majority of precipitation to this region comes via the Westerlies (Kurita et al., 2004) the climate is increasingly and the $\delta^{18}\text{O}$ values of precipitation are influenced by south and southeast cyclones in July and August (Bezrukova et al., 2008; Kostrova et al., 2012).

Seal, R., R.; and Shanks, W., C.;, 1998. Oxygen and hydrogen isotope systematics of Lake Baikal, Siberia : Implications for paleoclimate studies. *Limnol. Oceanogr.* 43, 1251-1261.

Kurita, N., Yoshida, N., Inoue, G., and Chayanova, E. A., 2004. Modern isotope climatology of Russia: A first assessment. *J. Geophys. Res.* 109, D03102.

Kostrova, S. S., Meyer, H., Chaplignin, B., Kossler, A., Bezrukova, E. V., and Tarasov, P. E., 2012. Holocene oxygen isotope record of diatoms from Lake Kotokel (southern Siberia, Russia) and its palaeoclimatic implications. *Quaternary International* (in press).

P1171, line 16: delete “long-term”

→ corrected.

P1172, Line 2+ The sentence starting “By taking:.” Should be altered. The observation that a palaeoclimate signal can be obtained here is “rare” is due to the lack of suitable archives, not so much the proxy.

→ By this the specific proxy $\delta^{18}\text{O}$ is meant. As either no records exist or lakes in the (Eastern) Arctic mostly contain no carbonates, the existing purified diatom samples from this records is one of the rare opportunities to gain a $\delta^{18}\text{O}$ signal from the eastern Arctic. This was added to the text: “(...) silica provides one of the rare opportunities to gain a direct and continuous $\delta^{18}\text{O}$ signal from paleo-precipitation beyond the LGM in the Eastern part of the Arctic.”

P1172, Line 17: be explicit here in terms of species-effect on isotope fractionation – but thought not to be important?)

→ We agree and added some words to this sentence. “Additionally, the species-effect on isotope fractionation is still not well understood for diatoms, but this effect was not observed for the prevailing diatom species in sub-surface samples (2 to 4 cm) at Lake El’gygytyn (Chaplignin et al.; 2012).”

P1173, Line 2: no capital letter needed for lake

→ The capital letter (Lake El’gygytyn) should be used as described in the common reference framework rules for all Manuscripts for this special issue. Lake El’gygytyn is a name of its own and therefore “Lake” whenever used in combination with El’gygytyn has a capital letter.

Methods

I like the fact that the authors have adjusted preparation protocols based on the size of the dominant diatom fraction. Given that these diatoms are relatively small, it must have been quite challenging to obtain ‘pure’ samples.

P1175, Line 5: I wonder if Swann and Leng 2009 is the best reference to use here,

and wouldn't Brewer et al. 2008 be more appropriate?

→ The Brewer et al. (2008) paper was the first article using geochemical mass-balancing to account and correct for contamination. However, the equation given there assumed that the measured $\delta^{18}\text{O}_{\text{diatom}}$ value resulted from a "pure" sample and only the contamination percentage times its oxygen isotope value had to be added. Swann and Leng (2009) refined and improved this equation by including the assumption that the measured the $\delta^{18}\text{O}_{\text{diatom}}$ value contains some percent of contamination. Chaplignin et al. (2012) used this equation and evaluated different techniques for contamination assessment. This is why the last two references are cited. Still, we added the original reference "Brewer et al. (2008)" and an "improved by" before the Swann and Leng (2009) citation.

Also Chaplignin 2012 should be Chaplignin et al. 2012.

→ corrected

P1175, Line 6: perhaps don't use "cont." as abbreviation for contamination, as cont. is usually used as an abbreviation for continued. Otherwise a robust account of contamination is provided.

→ The abbreviation cont. for contamination is shortly introduced. By this and by the context a potential misunderstanding (of cont.=continued) is eliminated. The abbreviation was used in Chaplignin et al. 2012, in the most recent article dealing with contamination issues. It was introduced as it is more convenient to read when dealing with contamination issues in the text often. Therefore, we would like to keep this term.

P1176, Line 14+: which of these bonds, if any, are influenced by, or are a product of diagenetic changes? i.e. what specifically will FTIR be expected to find in this respect.

→ This is stated in the text now. "A relative reduction in Si-OH groups compared to the Si-O-Si groups indicates a diagenetic change by a condensation reaction ($\text{Si-OH} + \text{HO-Si} \rightarrow \text{Si-O-Si} + \text{H}_2\text{O}$) typical for an alteration of amorphous silica by temperature and/or pressure towards a higher state of organisation."

P1176, Line 23: delete '-1' after 4cm?

→ No, this is correct. It refers to the instrument's resolution: "...measurements taken every 4 cm^{-1} for the spectral region between 3750 and 400 cm^{-1} ."

P1176, Line 26: provide a reference for the observation that absorbance peaks > 1500 cm^{-1} are not related to biogenic silica

→ We added a reference to the text: "All wavelength absorption peaks >1500 cm^{-1} were removed as these peaks are not related to biogenic silica (Fröhlich, 1989) or linked to (...)."

Fröhlich, F. (1989): Deep-sea biogenic silica: new structural and analytical data from infrared analysis – geological implications, Terra Nova, 1, 267–273.

Results

Nice account of contamination assessment and correction methods

P1179, Line 23: why are correlations given as r^2 values (coefficient of determination)? (cf. abstract where r values are quoted)

→ For the linear fit between one series of data points the common coefficient of determination " r^2 " is used. For comparing two downcore records the "r" values are used, as anti-correlations could be possible here. For example, when the different stable isotope records ($\delta^{18}\text{O}_{\text{Lake El'gygytgyn}}$, $\delta\text{D}_{\text{EPICA}}$, $\delta^{18}\text{O}_{\text{LR04}}$) "r" is applied which is correctly quoted in the abstract.

Discussion

Section 4.1.1. is nicely argued

P1181, Line 8-10: given quite marked changes in lake levels during e.g. middle Pleistocene, are changes in photic zone small due to bathymetry of the impact crater?

→ Yes, this is right. Despite the mentioned changes in lake level, the depth of the lake (app. 170m) allows the photic zone to be of similar depth throughout time.

P1181: Wilkie et al. 2012 reference is still in preparation. So either refer to as Wilkie et al. (unpublished data) or better still, include relevant isotope data in this manuscript. For example, it would be helpful to show $\delta^{18}\text{O}$ vs. δD plot of data from lake water, inflowing rivers and precipitation in relation to the global meteoric water line. Such a figure would also depict nicely the info given in the latter section of 4.1.2

→ The Wilkie et al. manuscript was submitted a month ago. The handling editor wrote us, that he passed it on with his okay to CPD already. So, the manuscript will be citable with a doi from CPD very likely within the next week. We will include the required information in the revised version or the proof's comments. As two authors of our manuscript are co-authors in the Wilkie et al. manuscript we prefer not to double the data and keep referring to it. The Wilkie et al. manuscript includes the term "modern isotope hydrology" in the title and is as such better suited to contain this data.

P1182, Line 21+: Nolan et al. do state "That is, in the modern record, general warming (local or imported) is more important by orders of magnitude than changes in storm tracks in controlling air temperature at Lake El'gytgyn". But this is during a time of unprecedented anthropogenic global warming. Do models also show this for periods not affected by AGW?

Also, there is evidence that during the last interglacial warm wet climates in northern Siberia persisted due to changes in AMOC influencing currents along the coast of northern Siberia (e.g. Velichko 1984).

Therefore, is the evidence really that robust that conclusions from Nolan et al. 2012 "suggest that these weather patterns have been relatively stable with time and are likely representative of this and other interglacial periods". I note that this paper is still undergoing the review process, so I'd be interested in seeing more evidence for such a claim. But otherwise a robust consideration of the potential controls on $\delta^{18}\text{O}$ is given

→ Which is why we would leave it like it is.

Two questions are raised here: (1) Is the modern, general trend more important than changes in storm tracks for controlling air temperature and (2) is the recent climate (influenced by "anthropogenic global warming") representative of past interglacials.

Ad 1: This is not based on our studies, we just use this information. However, the Nolan et al. paper received its final response, and despite being complex the manuscript got mostly minor revisions and after publishing it in CPD it will proceed to being published in CP. Our studies underline the point that the amplitude of short-term events and changes in $\delta^{18}\text{O}_{\text{precipitation}}$ in a constrained catchment will be lower in $\delta^{18}\text{O}_{\text{diatom}}$ due to the residence time of the 170 m deep lake functioning as a buffer and due to a low sedimentation rate and sampling resolution.

Ad 2: This question should be raised to M.Nolan in the Clim. Past Discussions. We refer to this article as it provides the background on this topic closest to the site. We added another reference for Beringia about the applicability of modern synoptic climate patterns to paleoclimate interpretation ("many synoptic controls that occur today also most likely

similarly occurred in the past”) by Mock et al. (1998): “Nolan et al. (this issue) suggest that these weather patterns have been relatively stable with time and are likely representative of this and other interglacial periods. **This is supported for Beringia by Mock et al. (1998).”**

Mock, C. J., Bartlein, P. J., and Anderson, P. M., 1998. Atmospheric circulation patterns and spatial climatic variations in Beringia. *International Journal of Climatology* 18, 1085-1104.

Page 1184, Line 10: “Apart from this study” is out of place. Omit.

→ corrected and deleted.

Page 1184, Line 11: Give the average Holocene resolution

→ this is mentioned in the text now: “(...) in relatively high resolution **(now until 2 ka BP: every 0.17 ka; 2k-22 ka BP: on average every 0.3 ka.; Fig. 6).”**

Section 4.2.1: Were both isotope studies done on the same material?

That the two records have significantly different isotope values especially for the Holocene period is important. The authors here do go through potential reasons, and each are dismissed. Nevertheless, there is an issue about reproducibility within any one site (lake) that should therefore be emphasised more. Furthermore, what other potential sources of error not discussed here have the authors considered?

→ We discussed all potential sources of error and tried to consider each one of them by thoroughly discussing these. George Swann is the author of the first study, co-author of this manuscript and agrees with what we have written in the text.

What are the main types of clays found in Lake E’s sediments. Could these be important?

The main types of clays can be found in Asikainen et al. (2007). However, this is not of major importance as the two different cores were both drilled in the center of the lake close to each other and dated. We added the information about the different cores: “Swann et al. (2010) examined the oxygen isotope composition of diatoms for the first 23 ka at Lake El’gygytyn in relatively high resolution (now until 2 ka BP: every 0.17 ka; 2k-22 ka BP: on average every 0.3 ka.; **core Lz1029;** Fig. 6). Hence, both records should show a similar influence of the clay composition, if any. The contamination was assessed and corrected for in both studies. In our opinion, this is intensely discussed.

Asikainen, C. A., Francus, P., and Brigham-Grette, J., 2007. Sedimentology, clay mineralogy and grain-size as indicators of 65 ka of climate change from El’gygytyn Crater Lake, Northeastern Siberia. *Journal of Paleolimnology* 37, 105-122.

Section 4.2.2: in Fig 7, need to state that the shaded parts of the stratigraphy indicate interglacial periods.

→ We added this to the caption for Figure 7: “**Shaded parts indicate interglacial periods.**”

However, the shaded area for MIS3 is different for Figs 7 and 8 because the shaded area in Fig 7 is too broad.

→ We are thankful for this comment. The shaded areas were corrected in both figures.

Also, in fig 7, swap TiO₂ and BSi, so that BSi can be better compared with the isotope record.

→ swapped.

In the discussion, discrepancies with respect to e.g. correlations (P1186, Lines 3-8) are given in terms of age periods (e.g. LGM). What do the data look like plotted on an age scale?

→ As we would like to show the $\delta^{18}\text{O}_{\text{diatom}}$ record against depth as well we remain with the plotted data shown in Fig. 7. However, we added the required information in the text: “The

anti-correlation of the overall record is mainly due to the first three meters of the core and the high peak of BSi at the time interval corresponding to the LGM (around 1.6 m depth).”

What is the correlation between TiO₂ and mag susc – I assume that one would expect these to show quite a high correlation.

→ This has been already done on a larger scale by Melles et al. (2012) and will be done in detail by Frank et al. (to be submitted in this issue; the submission within the next weeks got confirmed by U. Frank via e-mail). The focus of the manuscript should remain on the $\delta^{18}\text{O}_{\text{diatom}}$ record. It is beyond the scope and idea of this paper to compare all other shown records.

Frank, U., N. Nowaczyk, N., Melles, M., Minyuk, P., Müller, H., Rosén, P., Vogel, H. (this issue): A 350 ka record of climate change from Lake El'gygytyn, northeastern Siberia: Identifying different warm modes by means of cluster analysis.

> Contains: magnetic susceptibility, inorg. geochem., TOC, BSi, pollen

Melles, M., Brigham-Grette, J., Minyuk, P. S., Nowaczyk, N. R., Wennrich, V., DeConto, R. M., Anderson, P. M., Andreev, A. A., Coletti, A., Cook, T. L., Haltia-Hovi, E., Kukkonen, M., Lozhkin, A. V., Rosén, P., Tarasov, P., Vogel, H., and Wagner, B., 2012. 2.8 Million Years of Arctic Climate Change from Lake El'gygytyn, NE Russia. *Science*, doi: 10.1126/science.1222135.

> Contains: magnetic susceptibility, inorg. geochem., TOC, Si/Ti, pollen

P1185, Line 25-25: Colman ref is for BSi in Lake Baikal, and really only for interglacial periods. Given that Baikal is such a unique water body perhaps the authors could provide a bit of more detailed consideration of BSi in lakes over glacial – interglacial periods. For example, bottom waters of Baikal are oxygenated, and so different from anoxic bottom waters of Lake E.

→ We replaced the reference with a different reference: “BSi can be generally used as a proxy for nutrient availability and bioproductivity or primary production (Ragueneau et al., 2000)”. We do not want to go further into detail about the BSi in Lake Baikal and further potential drivers of this proxy, as this is beyond the scope of this paper.

Ragueneau et al. (2000): A review of the Si cycle in the modern ocean: recent progress and missing gaps in the application of biogenic opal as a paleoproductivity proxy. *Global Planet Change* 26:317–365.

P1185, Line 14-16: Colman et al. attribute increasing BSi in Baikal to increasing summer temperatures during interglacials. Moreover, I would be surprised if there was not a direct link between TiO₂ and precipitation, especially if a major source of TiO₂ comes from fluvial input (as stated in previously). So do the authors really think that increases in BSi are subject to “delayed reactions”? I’m not sure I understand how this would be manifested in a lake ecosystem. The authors suggest that $\delta^{18}\text{O}$ peaks “often” occur earlier than e.g. TiO₂ minima – how often? The majority of the time?

→ This is clearly described in the text: “ $\delta^{18}\text{O}$ peaks often occur earlier than TiO₂ minima or BSi maxima percentages (cf. $\delta^{18}\text{O}$ at 5.50-6.22 m depth vs. TiO₂ and BSi at 5.30-5.80 m; $\delta^{18}\text{O}$, 2.40-2.70 m vs. TiO₂ and BSi, 2.40-2.00 m)”. So, starting with the onset of MIS5 (around 6.5m depth) the TiO₂ minima happen earlier when being compared to $\delta^{18}\text{O}_{\text{diatom}}$. So, there appears to be a delayed response. This suggests that atmospheric temperature could be changing before internal lake/catchment processes change (i.e., sediment and lake

productivity). We will not speculate in detail about the potential reasons but mainly focus on the facts.

Section 4.3: P1186, Line 24: the NGRIP curve covers interglacial and glacial periods. The authors should state early on what LR04, NGRIP and EPICA data shown are representative of.

→ These curves are extremely prominent in the paleo-science community. As these are all $\delta^{18}\text{O}$ records, this topic is well introduced due to our diatom record being a $\delta^{18}\text{O}$ record, too. The regions where the records were retrieved are mentioned in the text passage which is as follows: “The diatom $\delta^{18}\text{O}$ record was compared with prominent climate curves such as the global marine $\delta^{18}\text{O}$ benthic stack LR04 (Lisiecki and Raymo, 2005; henceforth simplified to LR04) and the glacial $\delta^{18}\text{O}$ record from the North Greenland Ice Core Project (NGRIP; North-Greenland-Ice-Core-Project members, 2004; henceforth simplified to NGRIP). As this latter record ends at ~123 ka the δD Dome-C record from the European Project for Ice Coring in Antarctica (EPICA; EPICA members, 2004, 2006) henceforth simplified to EPICA) was used (...).” Therefore, we would prefer not to change this paragraph.

P1187: statistical evaluation has been undertaken between the isotope data and the climate proxy data. Were such evaluations done for the correlations between isotope data and BSi, TiO_2 and mag susc? How do the authors arrive at the conclusion that “a clear precipitation driven climate signal is preserved in the $\delta^{18}\text{O}$ record from diatoms: ”?

→ This is clearly stated in the text: P1186, Line 2: “Both, BSi and TiO_2 records do not correlate well with the $\delta^{18}\text{O}$ record ($\delta^{18}\text{O}$ vs. BSi, $r = -0.14$; $\delta^{18}\text{O}$ vs. TiO_2 , $r = -0.33$).” Hence, there is no good correlation or even an anti-correlation between the $\delta^{18}\text{O}$ record and the mentioned proxies. The conclusion to have a “clear precipitation driven climate signal” originates mainly from the discussion about the isotope controls. The good correlation with the EPICA and NGRIP record underlines and strengthens this conclusion.

P1187, Line 15+: how exactly does the correlation with obliquity support the proposed age model? Does it matter that the age model was tuned to insolation?

The $\delta^{18}\text{O}_{\text{diatom}}$ record was not used for calibrating the age model. Only the magnetic susceptibility was tuned to the insolation at 70°N (see above, second answer to first reviewer). So, the first observation is that our record supports the existing age model. The second observation is that the same peaks in the $\delta^{18}\text{O}_{\text{diatom}}$ and the 70°N insolation record (especially obliquity). Both points are clearly stated in the text (P1187, Line 15+).

P1181, Line 24: the $\delta^{18}\text{O}$ record is assumed to be one of temperature, but on P1187, $\delta^{18}\text{O}$ preserved a clear precipitation signal – can both drivers be determined from one proxy? But this section does show that the proxy has great palaeoclimate potential.

→ The isotopic precipitation signal’s main driver is the condensation temperature according to the discussion done in Section 4.1 $\delta^{18}\text{O}$ Isotope controls. So, the intention of this paper is by analysing $\delta^{18}\text{O}_{\text{diatom}}$ to reconstruct $\delta^{18}\text{O}_{\text{precipitation}}$ which is mainly influenced by condensation temperature. So the answer is yes.

Section 4.5 should ideally come before the palaeoclimate interpretation – i.e. demonstrate that there is little diagenetic effect, then following interpretations can be done with confidence.

→ The idea was to first properly introduce the $\delta^{18}\text{O}_{\text{diatom}}$ record before comparing any trend with the FTIR results. However, we agree here with the reviewer and changed the logical

order in most sections due to this comment and moved all sections with the topic diagenesis to earlier positions in all sections.

Figures:

Age scales between the Figures Fig 4b and the rest need to be the same units

→ We agree. The scale in Fig 4b was changed to “Age [ka]”.

References:

The references mentioned in this answer were added to the reference list in the revised version of the manuscript. Further references used in this answer are directly provided below the respective comments.