Reply to referee 1

We would like to thank reviewers for detailed and constructive reviews and comments. We appreciate the reviewers' thoughtful feedback and insights, which helped us improve the clarity and comprehensiveness of our work. We have revised all the points the reviewers suggested accordingly.

Our point-by-point responses can be found below:

L15: "a critical gap" – I would argue that lots of studies on lake ecosystem response to (LGIT) climate change are already available; of course we can always learn more (and we do in the remainder of your paper!), but I wouldn't call this a critical gap myself.

➔ **Replaced with** "While modern impacts are well-studied, knowledge on the responses of lake ecosystems to climate change in the pre-anthropogenic times is still sparse.".

L29: "a… sources" – singular vs plural

➔ **fixed**

L53: could you explain how climate warming is reinforced by watershed dynamics?

➔ **Sentence was edited to: "**It is increasingly recognised that rapid climate warming can affect nutrient loading to lakes through changes in the watershed dynamics, e.g., by affecting hydrological patterns, weathering rates and mass fluxes, and thus, is an essential driver of freshwater and ecosystem deterioration (Meerhoff et al., 2022; Jane et al., 2021; Jeppesen et al., 2010)."

L62: why is only one of the research methods highlighted here? Given that more information is provided on L75 onward I think this can be left out. Alternatively, perhaps a wider range of methods needs to be discussed on L62 as well.

➔ We removed the text highlighting the hyperspectral imaging and kept it only in the paragraphs below.

L75: you start with methods, then discuss the site, and then at L81 discuss methods again. I suggest to combine the methods either before or after the site introduction.

➔ Significantly adjusted the structure to implement these two comments: methods and site descriptions are now clearly separated.

> **The text reads now as follows:** "To answer those questions, we investigate a sediment core from a small kettle hole lake on the Swiss Plateau, Amsoldingersee. This lake is located adjacent to Gerzensee in an area that responded in extraordinary detail to Late-Glacial climatic changes documented in the North Atlantic domain and Greenland (Eicher, 1987, Lotter et al., 1992, Ammann et al., 2013). Amsoldingersee contains the complete Late-Glacial sediment sequence, including early deglacial anoxic periods (Lotter and Boucherle, 1984).
>
> A multi-proxy approach which includes Hyperspectral Imaging, sedimentary pigments, XRF, CNS and P, Fe, Mn and P fractions, and pollen was applied on the sedimentary record of lake Amsoldinger. Hyperspectral Imaging offers a way to reconstruct primary production, hypolimnetic anoxia and compositions of major primary producer groups at unprecedented (µm-scale) resolution on long time scales (Zander et al., 2023). Sedimentary pigments were used to investigate changes in past producer communities (Leavitt & Hodgson, 2001; Bianchi and Canuel, 2011). Pigments of anoxygenic phototrophic bacteria APBs (purple sulphur bacteria) are used as indicators for lake stratification and hypolimnetic anoxia (Züllig, 1986; Zander et al. 2022. We use XRF, CNS, and sequential extraction of sedimentary P, Mn and Fe to diagnose potential chemical feedback during events of hypolimnetic anoxia (Tu et al., 2021)."

L120: "continuously sieved" – suggest to change to "Consecutive (or contiguous) 1-cm-thick sediment samples were sieved". As it stands you are implying that you never stopped sieving.

➔ **Done**

L126: Perhaps not for this paper, but why was no pollen counted for materials younger than the Allerød?

➔ Pollen was analysed only in low resolution and in a specific core section to gain an assurance that the pollen record of AMS is similar to other published pollen records from Swiss lakes. Swiss lakes are very well-studied for their pollen

successions, and the pollen records are well-described and established. That is also why we were able to use the first occurrences of pollen as regional chrono-stratigraphical markers, specifically for the Bølling and early Allerød, when the three key tree species occur (Juniper, Beech, and Pine). Thus, as many pollen records from the Swiss plateau (Gerzensee, Moossee, Burgaschisee, and many more lakes) already exist, adding one will not change our understanding of the paleoenvironment.

L212: Here it states RDA is carried out but it is unclear where this comes back later in the manuscript. The RoC results are reported clearly, but I didn't see the RDA explicitly reported on.

➔ **RDA now explicitly mentioned** in the first paragraph discussion section 5.2 reading as follows:

"Redundancy analysis indicates that aquatic primary production in Amsoldingersee (e.g., Tchl, total carotenoids) covaries with TOC and follows predominantly, but not exclusively, temperature and arboreal pollen (RDA, Fig. S9)."

L223: what does "consistent" mean in this context?

➔ Consistent between the different dating approaches which is now added. **Reads as follows:**

"The chronology (Fig. 2) is constrained by calibrated ^{14}C AMS ages of nine taxonomically identified terrestrial plant macrofossils (Table S1), three pollen-based biostratigraphic markers (Fig. S4), and the Laacher See Tephra (13,006±9 yrs BP, Reinig et al., 2021), yielding ages between 14.9 and 10.5 ka cal. BP consistent among the different dating approaches."

L224: add if this is 1 or 2 sigma

➔ **2σ, implemented**

L271: Results 4.2: I find the description a bit confusing. I understand you go by LT rather than chronologically, but: LT4 is split up into two separate parts, with a larger unit during the BA and then a short unit at the end of the YD. LT4 (B/A) is given an interpretation, but the upper LT4 unit is not interpreted. LT5 is then split into two subunits in the "title" (L281) of 5.21-5.19 and 5.18-5.09m, but these subunits are sequential and the difference between these two parts of LT5 is never explained. I understand that in the Results section you are taking a LT by LT rather than a chronological approach, but the reader would benefit from a clearer explanation of where the LT sequence differs from the chronological sequence.

➔ **Now clarified** in the text. Now every subsection of each lithotype has its own interpretation
➔ These difficulties arise because of the Laacher See Tephra layer which occurs with the lithotype 4-5 transition.
➔ **We have added** an introductory sentences clarifying the structure of the lithology description to the results section: "We describe the lithology in lithotypes, which are independent of depth, identified by the unconstrained clustering based on the similarities in the sediment geochemical composition. Therefore, a lithotype can be found at various depths (Fig. 3) where the sediment composition is similar and does not fully follow the chronological approach, e.g., Lithotype 4 and Lithotype 5)."

L316: "that align with" – this seems incorrect to me. Only the Cluster 1/2 transition aligns with a lithostratigraphic transition. Cl2/3 just post-dates the lithostratigraphic transition, and Cl3/4 is smack in the middle of LT5. It's actually very interesting to see that these indicators change at different points, so I would suggest emphasising that.

➔ **This is now explicitly mentioned** in the results section and emphasized in the discussion under the Younger Dryas header where the transition is of importance to strengthen the interpretation of the multiphasic YD.

L350: I suggest adding "statistically insignificant" rather than just "insignificant" as you do report on and interpret these, so obviously there's some interest in this

➔ **Done**

L399: You do clearly show the importance of PC1, explaining a major 67% of the variability in your dataset. PC2 and PC3 are then only explaining very minor proportions at 9% and 7%, respectively. I would personally suggest rather than showing a bi-plot of PC2 vs PC3, to show one bi-plot of PC1 vs PC2, and another of PC1 vs PC3. There might be good reasons not to do this, but I think this would also visually represent the overriding importance of PC1, which is now somewhat lost.

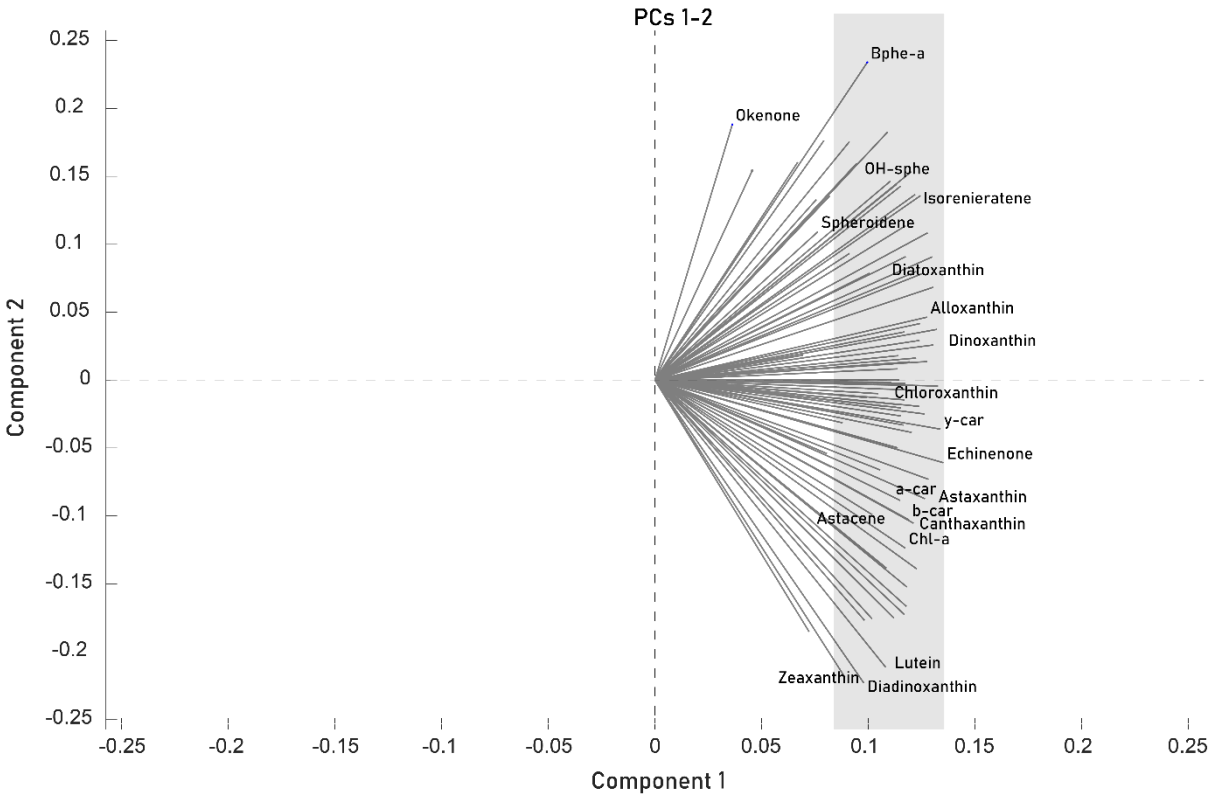
➔ We added a PC1 vs PC2 graph in the supplementary (this can be added as S10, as suggested by the reviewer). We have added an explanation in the text as to why PC1 is not used. As seen in the Fig S10, PC1 displays only the total productivity, e.g. it captures little of compositional changes. Because we are mostly interested in using pigment data

to asses compositional change, we are choosing to leave PC1 out of the analysis here, as total algal abundance is something different than algal composition.

➔ **Text modified to:** "In comparison, PC1 (63% of the variance, S10) follows total algal abundance (e.g. Tchl) and is, therefore, of little interest when assessing the changes in algal composition. On the other hand, when going beyond the dominant imprint of total algal abundance (PC1) we can pinpoint compositional changes."

➔ **Figure S10 added to Supplementary Material:**

➔ **PC1 vs PC3 was not added** because it would mean too much unnecessary appendices; besides if we add 1 vs 2 and 2 vs 3 then the reader has seen every pc; no need to add 1 vs 3 as well.



<u>L461:</u> could you explain how increasing CO2 could lead to impact on the landscape or the lake in absence of changes in temperature and vegetation composition? Similarly, are there any (e.g. geomorphological) reasons for increased landscape stability in the absence of changes to the terrestrial vegetation?

e revisited the data. If we look in detail at our pollen data (2 samples before and 2 samples after the lithological transition) we do not observe any compositional change. The *Betula* increase - that was recorded in Moossee (Rey et al., 2020) - only occurs later, at higher stratigraphical levels, excluding local vegetation-driven landscape stabilization as a cause for the transition. The uncertainty of the chironomid-inferred summer temperature reconstruction appears too large to exclude a subtle summer warming at that time.

<u>To answer the question:</u> It could be that the shift was caused by a local change in the geomorphology. We have several theories, but our evidence is inconclusive. Hence, these theories cannot be written in the paper. Therefore, we removed the CO2 curve from the paper.

➔ **The text now reads:**

"Amsoldingersee formed during H1, when retreating glaciers left a depression in the drumlin landscape (Fig. 1a). The sediment record starts with 2.65 m of laminated calcareous silty clay deposited into an early-deglacial oligotrophic perennial lake. Around 16.2 ka cal. BP, the sediment composition shifts to an organic gyttja. This transition marks an abrupt end of silty clay input into the Amsoldingersee basin. The sharp lithological transition can be explained as decreased sedimentation rates accompanied by increased organic matter deposition and a substantial reduction in detrital clastic input, suggesting increased landscape stability. No changes in local vegetation composition are observed and our data are insufficient to conclude on the cause of this lithological change."

<u>L516:</u> Out of interest, is there a role for wind (strength) here? Is the lake currently experiencing strong winds due to e.g. tunnelling along the valley?

- ➔ No, although there are weak thermal winds (drainage flow of cold air during night, only during anticyclonic synoptic situations) confined to the bottom of the Aare valley. They hardly reach up to the Amsoldinger plateau where the lake is found.

<u>L532-539:</u> Personally, I find it very interesting to see the 3-stage development of the YD. As you well explain, for a long time the YD was seen as a stable event. Later, the 2-stage event interpretation got more attention following the papers by Bakke et al. (200g Nat Geosci) and Lane et al. (2013 Geology). More recently, there seem to be more studies appearing that show evidence of an even more dynamic event, including a 3-stage YD in the SE of the UK as shown by Francis et al. (in press, JQS) as well as further afield (Fastovich et al., 2022; QSR). The Weber et al. (2020) paper that you cite also shows additional complexity during the YD. Sorry this is more a note than a comment, but I do find this result really interesting!

- ➔ Yes, it is interesting; I am recently part-taking in a YD working group led by Cecile Blanchet for this reason. If you are interested in joining, you can reach out to Cecile.

<u>L540:</u> why is there no interpretation of the early Holocene Lake environment? Surely, it's interesting to the reader to know how the transition into the Preboreal happened, and if circumstances (e.g. Si limited) in the Preboreal were similar to those of the (late) A/B?

- ➔ The focus of the paper is the Late-Glacial, with H1, the B/A warming and the cooling into the YD. The Transition to Early Holocene is beyond the scope of this paper and will be dealt with in a forthcoming paper that includes the entire Holocene and 20th C warming.

<u>L540:</u> "what drove communities" is a bit vague – do you mean community composition, overall productivity, or both?

- ➔ **Clarified and changed to: "**What drove the algal community composition?"

<u>Conclusions:</u> The order of PP and anoxia is reversed in the Conclusions compared to the Discussions (where PP (section 5.2) precedes anoxia (Section 5.3); I suggest treating them in the same sequence in both the Discussion and in the Conclusion.

- ➔ Changed the sequence in the conclusions.