Dear Dr Francus,

We would like to thank you for your constructive review which helped us to improve the clarity of our work.

Our point-by-point responses follow:

General comments

Abstract: The introduction to the topic is quite long and could be shortened.

- ➔ **Action:** We rephrased and shortened the introduction to the topic accordingly. **Figures and call to figures.**

In brief, please make sure that what is described in the text is easy to find in the figures.

1. While the figures are well drafted, it is often quite difficult to find the information described in the text. For instance, the text of sections 4.2, 4.3 and 4.4 refers to the chronozones H1, Bølling, Allerød, YD and Holocene, but none of the figures called for (Figs. 3-6) contains that information. I know that these figures are already very busy, but would it be possible to add these chronozones to Figs 3-6? It would make the paper easier to follow.

- ➔ The figures 3, 4, 5 and 6 were edited and the chronozones were added.
- ➔ The figures 7 and 8 were switched for better readability

2. I understand that you have established three different stratigraphies (litho-, cluster-, pigment-) and that you also use the chronozones, but the text is not easy to follow for the reader who is not familiar with the study.

- ➔ We clarified at the beginning of the result section how the results are structured. Similarly, at the beginning of the discussion section, we state the structure of the discussion which uses the chronozones.

  "To describe our results systematically, two datasets, one high-resolution and the other low-resolution are presented using lithotypes derived from unconstrained hierarchical clustering (section 4.2) and pigment zones resulting from CONISS clustering (section 4.4), respectively."

3. Along these lines, section 4.4 is hard to follow, because it is not clear what figure the text is referring to: is it 6 or is it 7?

- ➔ **Action:** Adjusting the referencing of figures 6,7,8 throughout the text.

4. To make things even more complex, you also use the NGRIP stratigraphy (e.g., (GI-1d)). I understand why you do that, but is it really necessary? Since you are not really discussing the potential leads and lags of your lake with NGRIP?

- ➔ We agree with the reviewer's comment and removed the NGRIP stratigraphy from the manuscript.

5. Another example is at **lines 492-493**: one needs to switch from one figure to the other to follow your discussion. Here, you refer in the same sentence to PZ-2a, described in fig. 7, and to features outlined in fig. 9i.

➔ **Action:** Pigment zones were removed from the discussion text when referring to primary production or anoxia and only Figure 9 was used to support the discussion text.

Chronology

1. Calibrated dates. All the dates have been calibrated using Intcal20, but none of the dates mentioned in the text and the figures indicate "cal". Please add "cal" before all occurrences of BP.

   L228: here you should use kyr instead of ka, because it refers to some duration.

   L618: replace the two occurrences of "ky" by "ka", as they are dates and not durations (and add "cal" of course).

   ➔ **Action:** We corrected the age annotations thought the manuscript.

2. It is not clear how the chronozones have been defined. You write on **lines 126-127:** *"We combined 14C dates with well-dated regional biostratigraphic markers (Ammann et al., 2013; Rey et al., 2017, 2020)"*, but I'm not sure if I understand what you have done practically. Are you just relying on your age model, and using the dates of the chronozones defined by Ammann et al., 2013 (or the NGRIP chronology) to set the boundaries used here? Or do you use the changes in one of your indicators to set the boundaries of the chronozones?

   ➔ **Response**

   First, we established our chronology using 14C dates, 4 biostratigraphic marker layers (our own pollen data compared with the regional literature) and the LST.

   In the following and for better readability and orientation, we added (in figures and text) the chronozones and their ages as defined by Ammann et al. (2013).

   ➔ **Action: The text was slightly extended:** "We combined ^{14}C dates with well-dated regional biostratigraphic markers (Ammann et al., 2013; Rey et al., 2017, 2020) <u>by comparing pollen profiles in Amsoldingersee, with those of Gerzensee and Moossee. Previously reported ages for biostratigraphic markers (Table S1) were transferred to our core.</u> Pollen was analysed in contiguous 2-cm intervals (Moore et al., 1991) up to the LST. The chronozones and their ages (Figs. 3, 5, 6, 8 and 9) are taken from Ammann et al. (2013)..

3. Along those lines, is your age model accurate enough to briefly discuss the timing of these chronozones? (This is probably another paper, but maybe somebody will want to do that.)

   ➔ **Response:**
   Probably not; technically, the 2σ uncertainty in our chronology is on the order of 300 years (see text). The benchmark papers in that respect are Ammann et al. 2013 and van Raden et al. 2013 who used the high-resolution δ18O record of Gerzensee to line up with the NGRIP δ18O chronostratigraphy.

Section 4.2.

At the end of the description of each lithofacies, there is an interpretation. I'm not against including interpretations of the lithofacies here, but one needs to make sure there is no repetition in the discussion section.

L251: "of low aquatic primary production (PP) and high accumulation rates of fine clastic sediments (matrix effect)." belongs to a discussion section, not a result section (see comment about section 4.2 in the general comments).

- ➔ We adjusted the text of section 4.2 by removing discussion-like text and by keeping only data description and data interpretation to avoid repetition in the discussion.

Discussion

Why is the onset of the Holocene not discussed? It was announced somehow in the introduction by saying that your study would help to understand the response of lacustrine systems in a quickly warming climate. Yet, the early Holocene is usually considered to have a very fast warming. If it is the recovery to warming that is of interest, please write it down in the text of the discussion.

- ➔ The study was designed to investigate one entire temperature cycle from cold (H1), to warm (B/A) and back to cold period (YD); for that purpose, we selected the Late Glacial warming and Younger dryas cooling as the target time span. Indeed, a limited fraction of our data (XRF and HIS) captures also the onset of the Holocene, but HPLC pigment data and sequential P, Mn and Fe data are missing (and were not in the scope of our study). A similar study covering the onset and entire Holocene is under way.

Section 5.5.1.

You are discussing AO, GZ, YD in this section dedicated to the description of H1. This is not logical. Maybe should you add a "dust record section" that covers multiple time slices.

- ➔ Accordingly, we moved this part to a new section 5.1.4. The Late-Glacial dust record

Other minor comments

L47: one understands you are talking about species only later. I suggest writing: "(...) promote generalist species competitiveness (...)".

➔ **Implemented**

L105: I think you should specify that the DOE-1 corresponds to the Bølling because I'm not sure that the readership of Biogeosciences is familiar of the DOE stratigraphy.

➔ **Thank you for your suggestion, we adjusted the text as follows:** *"Dansgaard-Oeschger Event-1 (DOE-1) started with the Bølling warming…"*

L132-135: I'm not sure to understand what you have done here? You XRF scanned the core three times and measured the standard deviation of your measurements? Was this performed on clr-transformed data or on raw data?

➔ **On the raw data, this is now clarified in the text:**
"For each core, triplicate scans (15 mm long) were taken at the top, middle and bottom of the core to quantify and test the standard error of elements (n = 27). Raw data of Al, Si, K, Ti, Zr, Cu, Ca, Sr, Mn, Fe, and S had a relative standard error of <15% and were considered reproducible and thus used for further analysis."

L215: I have not seen the R codes on Github.

The code will be available upon paper acceptance.

L237: only XRF data are shown in S5.
➔ **Correct, adjusted accordingly**

L249: how was water content measured? This is not in the method section.

➔ **Added:** "Water content was calculated from the sediment weights before and after freeze-drying."

L301: please spell out what LT means in the caption.

➔ **Figure 3 caption edited:** "Lithotype (LT)"

L315: not all transitions align with lithotype changes.

➔ **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 multiphase YD.

L369-370: Could the degradation of chlorophyll also be due to anoxia?

➔ To our knowledge, anoxia would rather favour pigment preservation as pigments degrade by exposure to oxygen and light. Thus, the CPI is driven by those two factors.

L413-415: What I understand from these lines is that the degree of degradation (PC3) is due to high grazing pressure during anoxic events. I don't understand this, as anoxia should prevent the presence of grazing organisms. Is there something I'm missing here?

➔ Grazing takes place in the epilimnion, which is always oxic; whereas anoxia refers to the hypolimnion in a well stratified and productive lake. **We clarified the text:**

L424 "… zooplankton grazing in the epilimnion, essentially separating degradation pathways (Bianchi and Canuel, 2011; Lami et al., 2000)."

L439: I suggest the following: "(…) show that environmental changes on the Swiss Plateau responded very closely to large-scale climatic shifts (…)".

➔ **Implemented**

L441: try to be more specific about how you have done that.

➔ As is written, we contextualize our findings by discussing them along chronozones as defined by Ammann et al. (2013).

➔ **Now the text read as follows:**

"To place our findings in context with the large-scale Late-Glacial paleoclimate evolution, we discuss the development of Amsoldingersee along chronozones as defined by Ammann et al. (2013).

L458: I don't see anywhere a curve with sedimentation rates. This "sharp" decrease is not visible in the age model presented in Fig. 2. Actually, you have no date before 16.2 ka BP, and there is just the assumption that the lake formed around 19 ka. The sedimentation rate you infer is quite uncertain, even if it makes sense. I would be more cautious here.

➔ **We agree and adjusted the text accordingly:**
"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."

L462-466: your interpretation makes a lot of sense, but you don't have the smoking gun. A stronger argument would be if you could show that the rounded grains in your sediment are Ti, K, Si and Zr-rich and the angular ones are from carbonate rocks.

➔ Indeed, more in-depth observation, using SEM, would be required to support this argument. We analysed the grains under light and cross-polarized microscope, but the changes amongst the grains were non-quantifiable and, hence, not discussed to keep the paper somewhat short.

L469: How this correlation has been calculated to cope with their different resolutions? Also, it is not easy to tell if the Ti curve in Fig 9g is the low-resolution one or the opposite. More contrasted colours would help.

1. Using linear regression
2. It is the low-resolution one; the contrast is enhanced.

L481: I'm not sure this statement is true and useful.

➔ **Removed the statement.** "This is quite particular for a lake sediment record."

L489: in my view, turbid waters are not compatible with low clastic sedimentation rates, or maybe there is something I don't understand here.

➔ Not necessarily; today, in Alpine lakes during summer, clay/fine silt may mostly remain in suspension and limit Secchi depth (and color the water). Clastic varve thickness may be <<1 mm yr^{-1} mostly consisting of the winter clay cap (formed during ice cover), the summer layer may be very thin. Accordingly, we replaced 'turbid water' with 'suspended solids'.

"During H1, starting at 16.2 ka cal. BP (; Fig. 7), anoxygenic phototrophic bacteria (APB) and some cyanobacteria inhabited the lake, indicating hypolimnetic anoxia during the very early phase of the lake development. APB have an advantage over other photosynthetic organisms in light-limited conditions (Karr et al., 2003); in Amsoldingersee, light could have been limited because of suspended solids in deglacial windy environments and/or prolonged winter ice cover"

L497: this fluctuation is barely visible. Is it a question of scale?

➔ Yes, we agree. We specified and now refer in the text to Fig. 9b, f and g.

L514: I do not see the 80% in this Bølling/Allerød interval. It starts near 0% and ends around 60%.

➔ In figure 9 we plotted only pollen % of individual species with the purpose of showing the vegetation changes. We added the total arboreal pollen % in the Fig. S4 and added reference to Fig. S4.

L537: "Drought" is quite a strong word. In this small closed lake, a drought would have a more spectacular impact. Maybe you should simply say that the climate was drier and that it produced a lowering of the lake level.

➔ **Changed sentence to:** "The end of the YD was associated with a persistently drier climate leading to lake-level lowering (Lotter & Boucherle, 1984)."

L541: one should expect high rates of changes between phases, but not during phases, right?

➔ Yes, we edited the text for clarity:

"Overall, algal community changes are synchronous with lithotype transitions and phases with high RoC (Fig. 7), which match the transitions of the chrono- and biostratigraphic zones reflecting large-scale climatic variations in the North Atlantic domain (Ammann et al., 2013)."

L568: Fig. 9g does not show Si.

➔ **Yes, it shows Ti;** We removed Si

L569: this idea of Si-fertilization has already been presented earlier. Try to avoid repetition

➔ **Rewriten to have less repetition:** "Further, after the substantial Si-fertilization by LST (13.05-12.90 ka BP; Fig. 7) we observe an isolated response of silicifyers, while green algae (lutein and Chl *a* - related pigments) and cyanobacteria remained invariant."

L571: what do you mean here? How the LST could Si-fertilize the lake before the eruption even occurred?

➔ **We edited the two sentences for better clarity:**

“Blooms of diatoxanthin (diatoms) and alloxanthin (cryptophytes) persisted during the YD, suggesting that Si fertilisation persisted throughout the YD. It is unusual to find signs of aerial silica fertilisation related to the tephra (Züllig, 1986) or dust in lake sediment records because these typically contain substantial siliciclastic fractions (Koffman et al., 2021).”