Response to Reviewers

A high-resolution perspective on climate drivers of lake stratification and phototrophic community dynamics in Late Glacial Central Europe

Zahajská et al.

We thank the editor and all three reviewers for their careful, constructive evaluation of our manuscript. Below we provide a point-by-point response to each comment (reviewer comments in italics, our responses in plain text), followed by a consolidated list of all changes made in the revised manuscript.

Reviewer 2 – Prof. Brian Cumming

We thank the reviewer for this insightful comment. We completely agree that a rigid "successional" framework may imply an autogenic progression toward equilibrium that does not capture the highly dynamic, climate-forced nature of this system. Accordingly, we have thoroughly revised the manuscript to replace this framework with environment- and climate-driven designations. In Section 3.2, the groups are now explicitly defined as: (1) the Pleniglacial assemblage, (2) the transitional Bølling assemblage, and (3) the stratified Allerød assemblage. We have also updated the heading of Section 4.2 to "The climate-driven primary producer community shifts" and reframed the text to focus on allogenic responses to temperature, wind shielding, and mixing regimes rather than ecological succession.

Comments by line, some of which reinforce the general comments above.

Comment (Line 18): *Typo — change 'phosphorous' to 'phosphorus'.*

Response: We have corrected this typographical error throughout the manuscript.

Comment (Lines 74–78): *Questions asked — not sure you are answering these questions in your paper. The first question I would keep as how phototrophic communities change over periods of rapid climate change over the D-O Event 1 from 14,690 to 11,700 cal BP, involving both warming and cooling events. You are inferring stratification, which is not the question. Question 2 is more of a discussion point and not a central question. Question 3 — not sure you can assess hysteresis and this isn't discussed much in the paper. Hysteresis is not adequately defined in the paper.*

Response: We appreciate this constructive critique. We have streamlined and restructured our central research questions in the Introduction to align tightly with our datasets and final interpretations. We removed the overemphasis on stratification in the first question, omitted the separate discussion point, and addressed the conflation regarding "hysteresis". The text now presents two focused questions: (1) How do phototrophic communities change in response to periods of rapid climate shifts (both warming and cooling) during Dansgaard-Oeschger Event 1 (14,690–11,700 yr cal BP)? (2) How do internal biogeochemical mechanisms (such as iron-mediated nutrient retention) and catchment vegetation modulate the resilience of the lake and ecosystem reversibility when these climatic drivers shift?

Comment (Line 79): *'Identify' and 'hypothesize' are two similar but different concepts. I would not use 'identify'.*

Response: We agree with this distinction. The text has been modified to: "...we hypothesize the specific roles of catchment vegetation and iron availability in regulating lake resilience."

Comment (Line 86): *Change to 5.8 ha (simpler).*

Response: To balance maximum data interoperability with reader scannability, we have included both units in the site description: "...with a small surface area of 58,000 m^2 (5.8 ha) relative to its maximum depth of 20 m."

Comment (Line 87): *Your site description needs to be better. You state that Holzmaar is predisposed to meromixis, but you never state that it is or is not meromictic. This is important. More information is needed on this lake. Is it permanently meromictic? How does the mixolimnion mix (Lewis classification)? The thermocline and the chemocline are two separate things, and normally don't co-occur. Do they in Holzmaar? This leads to great uncertainty later in your discussion.*

Response: We thank the reviewer for pointing out these ambiguities. We have expanded the site description to provide a clearer limnological context. Specifically, we clarify that Holzmaar is currently dimictic rather than meromictic. We also explicitly note that in this specific system, the thermocline and chemocline are often located in close proximity to one another, at approximately 6 m water depth.

Comment (Line 101): *Don't know what is meant by 'parallel' — sediment cores taken within 'x' meters in the same location?*

Response: We agree that this wording was unclear and have replaced "parallel" with "adjacent" to better describe the coring locations. We also added a map with the coring locations as Figure 1.

Comment (Line 146): *Not sure you need 'absolute' before pigment. Maybe be more explicit on why hyperspectral and HPLC — to allow for inexpensive higher resolution analysis.*

Response: We have removed the word "absolute" as suggested. Furthermore, we have explicitly clarified our methodological rationale, noting that combining hyperspectral imaging with HPLC enables highly detailed, continuous-resolution analysis in a more time- and cost-effective manner.

Comment (Line 185): *'Neutral pH' — distilled water is not neutral (pH ≈5.6).*

Response: We appreciate the reviewer catching this detail. The text has been adjusted to: "An aliquot of each sample was oxidised with 30% H_2O_2 and heated in a water bath at approximately 80°C for 2–5 minutes to eliminate organic matter, following standard methods (Battarbee, 1986). Permanent slides were mounted using Naphrax mounting medium."

Comment (Line 185): *Reference for your modified evaporation methods. What evaporation method (Battarbee?) and how modified. If you have concentrations of diatoms, I would prefer this over the relative abundances presented so that you can relate to your Si from XRF.*

Response: We have added the proper reference for the evaporation method used: Battarbee, R.W., 1986. Diatom analysis. In: Berglund, B.E. (Ed.), Handbook of Holocene Palaeoecology and Palaeohydrology. J. Wiley & Sons Ltd., New York, pp. 527–570. We have also added the diatom concentrations to Figure 1 (now Fig. 2) alongside the Si/Ti ratio, but we retain the Si/Ti ratio for further analysis, as its resolution is much higher than that of the diatom concentrations.

Comment (Line 190): *Subscript for HNO3.*

Response: We have corrected the formatting.

Comment (Line 220): *Only a selection of variables... was used for further RDA... please describe how you selected the variables you used.*

Response: We have expanded Section 2.8 ("Predictor variable selection and quality control") to explicitly describe our selection workflow. We performed a formal Variance Inflation Factor (VIF) assessment with a strict exclusion threshold of VIF > 10 to remove collinear variables (which excluded 8 redundant parameters, including annual/seasonal insolation curves and several pollen series), leaving a robust subset of 10 non-redundant environmental drivers for the final Redundancy Analysis (RDA).

Comment (Line 230): *State the 'common' resolution?*

Response: We have added the specific information regarding the common resolution. The XRF–HSI data were aligned at a 1 mm resolution, while the lower-resolution variables and external datasets were aligned at a 100-year resolution.

Comment (Lines 236–237): *I agree completely with the reduction of dimensionality, but how did you do this (see above)? Did you adjust your permutation test for multiple comparisons? Fig. 4 seems to have a lot of forward-selected variables. Did you assess the VIFs?*

Response: As described in our updated Section 2.8, we shifted to a strict VIF-based filtering protocol to resolve collinearity prior to running the ordination model, removing 8 highly multicollinear variables. For quality control in our permutation testing, we now explicitly state that significance was evaluated using 999 unrestricted permutations across the entire model and individual axes, with p-value thresholds systematically adjusted using a conservative Bonferroni correction (α) to avoid Type I error inflation. The final RDA biplot has been updated accordingly and is now presented as Figure 5.

Comment (Section 3.2): *Primary producer communities — I would not characterize these as pioneers, secondary colonizers and diatoms + PS. This has been done in the literature in Arctic systems, but high abundances of the benthic diatoms you list in Fig. 3 occur in any lake depending on the depth sampled. These taxa are likely 'low-light' specialists (see Kingsbury et al. 2012, doi:10.1111/j.1365-2427.2012.02781.x; Gushulak et al. 2020, https://doi.org/10.1007/s10933-020-00146-w).*

Response: We are very grateful for this recommendation and the provided literature. We agree that describing these shifts as a classic autogenic succession was flawed. In Section 4.2, we have reinterpreted the Pleniglacial community through the lens of light limitation and landscape instability. Guided by Kingsbury et al. (2012) and Gushulak and Cumming (2020), we now discuss these small fragilarioid diatoms and cryptophytes (alloxanthin) as specialised low-light and turbidity-tolerant assemblages that held a clear competitive edge during a harsh, well-mixed phase with prolonged ice cover, matching the ecological traits highlighted by the reviewer.

Comment (Line 257): *Agree that you have a pattern in Fig. 2, but all pigments infer very low production and high erosion.*

Response: We have clarified this interpretation in Section 4.2. The Pleniglacial pigment profile is now explicitly contextualised as a low-biomass assemblage operating under severe nutritional and climatic strain, strictly co-occurring with high detrital input (Ti) driven by intense regional wind activity and an open, unstable landscape.

Comment (Fig. 2): *If you keep it, it needs to be better labelled. I would much prefer seeing a plot of your pigments in terms of concentrations.*

Response: We opted for a heatmap because centring and scaling the data effectively removes the sediment-composition effect, allowing us to better visualise the relative shifts between communities. We hope the reviewer agrees that this is valuable for interpretation. However, we understand the desire to see absolute values, so we have added a supplementary figure presenting the absolute pigment concentrations.

Comment (Line 263): *'Silicifiers' is not a commonly used term — do you mean diatoms and chrysophytes, or just diatoms? What do you mean by 'red-blooming algae' — dinoflagellates or Rhodophyta? Likely best to specify the pigment so as not to confuse the reader.*

Response: We have eliminated these vague categories to prevent confusion. The text has been modified to specify the precise biomarker names alongside their concrete producer classifications based on modern chemotaxonomic literature (e.g., reclassifying "red-blooming algae" to astaxanthin-producing chlorophytes or dinoflagellates). These details have also been carefully cross-checked and updated in the summary reference table (Table A1).

Comment (Line 264): *I didn't read Vossel et al. (2015), but you should explain how they concluded that P. ocellata thrives in oligotrophic waters with low phosphorus concentrations and a stratified water column. Vossel et al., from the title, seems like a taxonomic paper and not an ecological paper. In your Fig. 3, P. ocellata has similar %abundances during the B-A when S. minutulus and Chl a are high, so in your core, your interpretation is contradictory.*

Response: The reviewer is entirely correct; our original interpretation of *Pantocsekiella ocellata* was too restrictive and created a clear contradiction with our productivity proxies (*S. minutulus* and high chlorophyll *a*) during the Bølling-Allerød. We have completely revised this section. While Vossel et al. (2015) is primarily a taxonomic paper, they note that open-water morphotypes of the *P. ocellata* complex exhibit wide nutrient tolerances and are prevalent in deep, open water-column settings. Because cyclotelloids have a clear morphological advantage for staying suspended, their co-occurrence with heavy *S. minutulus* diatoms and photic-zone euxinia markers (okenone) provides key evidence for a structured meromictic mixing regime. This indicates that active seasonal mixing occurred within an open upper mixolimnion, while a stagnant deep monimolimnion preserved the anoxic bacterial plate.

Comment (Lines 268–269): *The lake level likely increased, so this is not succession.*

Response: We agree. This text has been completely rewritten to frame the structural transition in terms of expanding pelagic niches, rising water levels, and changing lake morphometry, rather than autogenic ecological succession.

Comment (Line 277): *Agree that production was low in the Pleniglacial. Please state why you believe that nitrogen was limiting.*

Response: We have added our explicit biogeochemical reasoning to Section 4.2: the inference of nitrogen limitation is supported by the high relative abundance of generalist cyanobacteria (highly efficient in nitrogen-poor waters, or capable of nitrogen fixation) occurring alongside the terrestrial expansion of *Juniperus* in the catchment, a prominent pioneer taxon known to target young, nitrogen-deficient soils.

Comment (Line 278): *If the driver is an increase in lake level, why would you refer to these taxa as late-successional? The environment is changing; succession needs stability.*

Response: We agree completely. The term "late-successional" has been deleted and replaced with a process-based descriptor: "the stratified Allerød assemblage" (Sections 3.2 and 4.2).

Comment (Line 302): *Would like a little more discussion on vivianite. Biogeochemists I have discussed vivianite with in the past believe that vivianite is post-depositional, requiring anoxia and ferrous iron. Wouldn't sequestering of phosphorus occur simply by the formation of meromixis?*

Response: This is an excellent point. Vivianite is indeed a post-depositional mineral requiring anoxia and ferrous iron, and this is precisely the mechanism by which phosphorus was trapped in Holzmaar. While P is initially sequestered due to meromixis, we emphasise that it remains permanently sequestered in the sediment due to its reaction with iron. In systems where iron is not readily available, P can be released back into the water column under anoxic conditions via reductive dissolution of the Fe-(oxy)hydroxides it adsorbs onto. We have expanded our discussion to clarify these distinct processes.

Comment (Fig. 3): *You state 'selected sedimentary pigments'. Why were these ones selected (higher concentrations?); do you have concentrations of diatoms — do the concentrations correspond to your spikes in Si from Fig. 1? At least in North America, P. ocellata is a planktonic taxon. I would not agree that it is benthic; it is a round, rapheless centric.*

Response: We have updated this figure (now Figure 4) and its caption to explicitly state that the pigments selected serve as diagnostic biomarkers representing the major primary producer groups and major structural shifts. We have added absolute diatom concentration profiles to Fig. 1 (now Fig. 2), which show excellent stratigraphic agreement with our high-resolution ratio. Finally, we thank the reviewer for pointing out the ecological misclassification of *P. ocellata*; we have corrected its classification from benthic to planktonic and updated its colour coding in the figures.

Comment (Line 324): *You represent your pollen as relative abundances. Pine can be transported a very long distance, so don't over-interpret high AP values.*

Response: We thank the reviewer for this important caveat regarding pine pollen transport. We are mindful of this and have ensured that our interpretations of the AP values remain conservative.

Comment (Line 326): *Totally disagree — this is not like ecological succession.*

Response: We agree and have removed the succession analogy entirely, replacing it with an interpretation driven by physical mixing and landscape thresholds.

Comment (Line 332): *Yes, you have small amounts of many taxa including benthic diatoms. To me this infers low light and an unstable landscape. What is a 'generalist cyanobacteria'?*

Response: We have rewritten this section to centre on a low-light, highly turbid environment and an unstable, eroding landscape. We also clarified that "generalist cyanobacteria" refers to our bulk echinenone pigment signal, which indicates the presence of general cyanobacteria prior to the development of specialised nitrogen-fixing communities, and have rephrased this part for clarity.

Comment (Line 342): *Agree with your rising lake levels and expansion of macrophytes and a more diverse benthic flora in the early Bølling, and then even higher water levels. In your HPLC data, could you look at the ratio of chlorophyll a/pheophytin a as a proxy? I would hypothesize that if the lake got deeper this ratio would be lower and stable (more photodegradation on the way to the sediments), and when shallow and turbid the ratio would be high and potentially variable due to quick burial. Just a thought if you have this data.*

Response: This is an excellent methodological suggestion. We calculated the chlorophyll a / pheophytin a ratio and included it in our supplementary material. However, we have added a note of caution in our text: because these pigments are highly sensitive to post-coring oxidation and light exposure during core handling and extraction, the downcore ratio can easily incorporate modern degradation artefacts. Thus, while it aligns conceptually with our water-depth interpretations, we treat it with appropriate analytical caution.

Comment (Line 346): *The trend towards thermal stratification cannot be inferred from okenone. Okenone is normally associated with meromixis. Thermal stratification likely occurs in the upper water layer (mixolimnion), which could be categorized based on the Lewis Lake classification system (i.e., with respect to mixing patterns).*

Response: We agree completely with this critical limnological distinction. We have adjusted our phrasing in Section 4.2 to specify stable chemical stratification and meromixis rather than thermal stratification, noting that okenone strictly marks the position of the chemocline reaching the photic zone. We do note, however, that in modern Holzmaar the thermocline and chemocline often coincide at a depth of 6–8 m.

Comment (Line 354): *I would not use the term 'succession'.*

Response: The term has been removed here and throughout the manuscript.

Comment (Line 361): *I don't see a paradox. Anaerobic bacteria and S. minutulus occupy different regions of the lake and could easily vary seasonally, even if the lake was chemically stratified. You do not need to imply ecological plasticity. Do you have any data on seasonality? Does this lake also thermally stratify in the mixolimnion?*

Response: We appreciate this comment, which helped clear up an over-complicated argument. We have removed all mention of an ecological paradox or specialised plasticity. The text has been heavily rewritten to describe this co-occurrence as a direct consequence of a structured, compartmentalized water column (seasonal or spatial separation) within a meromictic lake.

Comment (Line 369): *Sounds like you have some data on seasonality and distribution of diatoms. Please describe the data collected. However, upon examination of the references, any evidence is sparse. If you have any evidence that S. minutulus exists above the bacterial plate, please let the reader know — this appears to be highly speculative but is written definitively.*

Your statement "...S. minutulus to thrive in the lower epilimnion immediately above the bacterial plate..." makes no sense: the epilimnion is up in the mixolimnion, and the bacterial plate is by the chemocline. The deep chlorophyll maximum normally occurs in the metalimnion in the mixolimnion; the chemocline is normally deeper in the lake. Spanbauer's statement is correct, but she is referring to the lower epilimnion, where diatoms get stuck due to thermal density changes. This whole section is not written well; coming in at the end of the paragraph with new data on seasonal separation should have come earlier.

Response: We apologise for the flawed limnological terminology in our previous draft, which we have carefully corrected in Section 4.2. Our direct evidence for seasonal vs. spatial coexistence comes from our high-resolution hyperspectral core scans (Figure 6, formerly Figure 5). The micro-stratigraphy of the varves shows a dual control: distinct, unmixed seasonal laminae (strict spring diatom blooms interbedded with summer bacterial plates) occur alongside micro-layers showing simultaneous deposition. We have restructured the paragraph to clarify that during summer stratification of the upper mixolimnion, *S. minutulus* successfully occupied a shaded, light-limited niche, forming a Deep Chlorophyll Maximum (DCM) within the metalimnion, positioned vertically distinct from and immediately above the dense, self-shading purple sulphur bacterial plate at the deeper chemocline.

Comment (Lines 392–394): *I would insert that the expansion of birch may have acted as a reinforcing factor.*

Response: We agree and have adjusted Section 4.3 to state that the expansion of *Betula* acted as a reinforcing factor: "...the stabilisation of catchment soils by *Betula* and the associated export of organic matter acted as a reinforcing factor for lake stratification." (originally-submitted lines 463–464).

Comment (Line 403): *Yes, DOC may be important, but without data you are getting complex without a lot of support.*

Response: We agree that without direct proxy data for DOC, this becomes overly speculative. We have removed this hypothesis from the discussion.

Comment (Lines 412–415): *Yes, may be a factor, but cooling/aridity is likely the most important factor. Did regional conditions show drops in lake levels? The drop in production is huge. When you say stable lake stratification, do you mean the breakdown of meromixis, or both? On Fig. 3, were diatoms simply not present, but analyses attempted? Did lakes freeze over permanently? Do other records show such large changes?*

Response: We agree completely with the reviewer that regional cooling and increased aridity were the decisive drivers behind this major transition. We have adjusted the discussion in Sections 4.2 and 4.3 to explicitly focus on these climatic drivers: (1) Lake-level changes — to our knowledge, there are no documented or conclusive indications of distinct Late Glacial lake-level fluctuations from the Eifel region; we therefore avoid attributing the productivity drop to changes in water depth and focus primarily on thermal and density-driven mechanisms. (2) Permanent ice cover vs. seasonal cover — given Holzmaar's latitudinal position (≈50°N), a permanent, perennial ice cover is highly unlikely and is physically argued against by the continuous deposition of sediments preserved throughout both the Pleniglacial and the Younger Dryas; an extended seasonal or severe winter ice cover certainly could have occurred. (3) Stratification vs. meromixis breakdown — when discussing the termination of this stable state, we specifically refer to the complete collapse of the stable meromictic density gradient (and its associated monimolimnetic anoxia) back into a holomictic mixing regime; the drop in temperatures and enhanced wind-driven wave action increased surface water density, triggering deep convective mixing that completely re-oxygenated the deep basin and dismantled the metalimnetic chemocline. This structural shift and the subsequent ecological reversal are discussed in Section 4.2 and visually traced by the multivariate trajectory returning to the pioneer-community space in Figure A5. (4) Further diatom data — diatom analysis was not conducted in the Younger Dryas because of a sedimentary hiatus, and the diatom analysis resolution was 4 cm; a higher-resolution diatom analysis at the end of anoxia would be useful, though we don't have these data. We refer to the Holocene diatom stratigraphy from García et al. (2022) to describe the community after the hiatus.

Comment (Line 469): *Internal phosphorus loading and hysteresis are not the same; needs clarification.*

Response: We appreciate this correction. We have uncoupled these terms in our Conclusions (Section 5). We now clearly define internal phosphorus loading as the specific biogeochemical feedback loop that recycles sediment nutrients, whereas hysteresis is properly described as the resulting macro-level ecosystem state (i.e., a delayed recovery after the primary climate driver is removed).

Reviewer 1 – Prof. Wojciech Tylmann

We sincerely thank the reviewer for the highly positive assessment of our manuscript and for recognising the value of our multi-proxy approach and statistical analyses. We appreciate the careful reading of our work and the constructive suggestions provided, which have helped us improve the clarity and accuracy of the final manuscript.

Detailed comments:

Comment (Lines 40–41): *This statement may be misleading, as winter mixing is often inhibited by ice cover. Furthermore, autumn is not strictly a stratified period but rather a transitional season during which mixing begins to intensify. I suggest rephrasing to: "...is disrupted when surface cooling and wind stress induce deep convective mixing in late autumn/winter or spring."*

Response: We thank the reviewer for this precise limnological clarification. We completely agree that winter mixing is often inhibited by ice and that autumn is transitional. We have adopted the suggested phrasing to ensure our description of the mixing dynamics is accurate.

Comment (Lines 83–91): *Although the lake is well known, a more detailed description, possibly accompanied by a schematic map, would improve clarity for a broader readership.*

Response: We appreciate this suggestion. To make the manuscript more accessible to a broader readership, we have expanded the site description and added both a regional map and a detailed bathymetric map of Holzmaar as Figure 1.

Comment (Lines 93–99): *Not sure if this information may be more appropriately placed in the Introduction section.*

Response: We agree with the reviewer that this overview of the established Late Glacial trajectory of Holzmaar serves a much stronger thematic purpose in the Introduction than in the Methods section. We have removed this text block from the site/methods section and integrated it into the final paragraph of the Introduction (Section 1). By weaving these details into our summary of previous work (Zolitschka, 1998; García et al., 2022), the manuscript now clearly outlines the known broad-scale vegetation and trophic shifts before explicitly targeting the unresolved high-resolution biogeochemical gaps that our study addresses. We feel this has greatly improved the text's structural flow and readability.

Comment (Line 148): *Consider revising to "0.5 to 1.5 g".*

Response: We have corrected this to read "0.5 to 1.5 g" as suggested.

Comment (Line 185): *The "modified evaporation tray method" requires either a reference or a brief description of the modification.*

Response: We apologise for the omission. We have added the appropriate reference (Battarbee, 1986) to clarify the modified evaporation tray method used: Battarbee, R.W., 1986. Diatom analysis. In: Berglund, B.E. (Ed.), Handbook of Holocene Palaeoecology and Palaeohydrology. J. Wiley & Sons Ltd., New York, pp. 527–570.

Comment (Line 245): *Replace "three" with "III".*

Response: We have replaced the word "three" with the Roman numeral "III" to maintain consistency with our cluster naming convention.

Comment (Lines 251–252): *I have concerns regarding the interpretation of PC2; in fact I do not understand why you think it "...represents the division between minerogenic (negative) vs. organic matter and redox sensitive (positive) elements in the sediment record". Please carefully verify the loadings, as the current interpretation appears inconsistent with Fig. A4. See also my comment on the figure caption below.*

Response: We are very grateful to the reviewer for catching this discrepancy. The description of the PC2 axis was indeed swapped in the text compared to Figure A4, as well as in the figure caption of Fig. A4. We have corrected the text according to the figures; everything should be correct now.

Comment (Fig. A4 caption): *The caption appears inconsistent with the figure. It does not seem that Mn, S, Cu, and Zn have positive loadings on PC2. Instead, the figure indicates that Mn, Cu, and Zn exhibit strong negative loadings, while S shows only a minor positive loading.*

Response: Similar to our response above, the PC2 axis description in the caption was mistakenly swapped. We have revised the caption for Figure A4 to accurately reflect the directional loadings (i.e., Mn, Cu, and Zn exhibit strong negative loadings, and S shows a minor positive loading).

Community comments — Dr. Zander

Dear Dr. Zander,

Thank you very much for your kind words and for taking the time to review our manuscript. We deeply appreciate your insightful comments, which have allowed us to refine the integration of the regional GDGT-based temperature reconstruction and strengthen our paleolimnological interpretations.

Please find our detailed responses to your points below:

Comment (Auel Maar age model): *Suggestion to remove the 3 youngest data points from Auel Maar (ages 13128, 13340, and 13875 b2k), which were excluded from the final reconstruction in Zander et al. (2024).*

Response: We thank you for pointing this out. We have completely removed these three data points from both our figures and our statistical analysis. To ensure complete transparency, we have explicitly noted this quality-control filtering step in the Methods section, under Section 2.8 ("Predictor variable selection and quality control").

Comment (Figure axis/label): *Note in the figure that GDGTs represent Temperatures of Months Above Freezing (TMAF).*

Response: We have updated the text accordingly. Due to the insertion of a new site/bathymetric map early in the manuscript, the multi-proxy summary diagram is now Figure 7 in the revised version. We have corrected the y-axis label to explicitly state T_{GDGT} (TMAF, °C) and updated the figure caption to ensure maximum clarity for the reader regarding the nature of the temperature proxy.

Comment (Molecular evidence): *Incorporating high %GDGT-0 (methanogenesis) and the presence of brGDGT-IIIa'' to confirm deep-water anoxia as early as 14.2 ka BP.*

Response: This is an excellent addition to our framework, and we are grateful for the suggestion. We have integrated these specific molecular indicators into our discussion in Section 4.2. Mentioning the presence of brGDGT-IIIa'' provides robust, independent geochemical support for our sedimentary pigment evidence (such as the early green sulphur bacteria marker).

Thank you once again for your constructive guidance, which has significantly upgraded the geochemical accuracy of our paper.

Sincerely,
Zahajská et al.

List of All Relevant Changes Made in the Manuscript

Manuscript location	Summary of change	Raised by
Manuscript-wide	Corrected "phosphorous" to "phosphorus" throughout the text.	Reviewer 2
Introduction	Streamlined the two central research questions to focus tightly on phototrophic community change and biogeochemical/ecosystem resilience; removed the separate 'discussion-point' question and the unsupported use of 'hysteresis'.	Reviewer 2
Introduction	Changed "identify" to "hypothesize" when describing the roles of catchment vegetation and iron availability.	Reviewer 2
Introduction	Integrated the former Methods paragraph on Holzmaar's established Late Glacial vegetation/trophic trajectory into the final paragraph of the Introduction, citing Zolitschka (1998) and García et al. (2022).	Reviewer 1
Introduction	Rephrased the seasonal-mixing description to: "...is disrupted when surface cooling and wind stress induce deep convective mixing in late autumn/winter or spring."	Reviewer 1
Site description (Methods)	Clarified that Holzmaar is currently dimictic (not meromictic) and noted that the thermocline and chemocline are typically in close proximity (≈6 m depth).	Reviewer 2
Site description (Methods)	Added surface area in hectares (5.8 ha) alongside the m² value.	Reviewer 2
Site description (Methods)	Replaced "parallel" with "adjacent" to describe coring locations; added a new Figure 1 with a regional map and a detailed bathymetric map of Holzmaar showing coring locations.	Reviewers 1 & 2

Manuscript location	Summary of change	Raised by
Methods — pigments	Removed "absolute" before "pigment"; clarified the rationale for combining hyperspectral imaging with HPLC (cost- and time-effective higher-resolution analysis).	Reviewer 2
Methods — diatoms	Corrected "neutral pH" description of distilled water; specified the H_2O_2 oxidation protocol and temperature/time.	Reviewer 2
Methods — diatoms	Added Battarbee (1986) reference for the modified evaporation tray method; added diatom concentration profiles to Fig. 1 (now Fig. 2) alongside the Si/Ti ratio.	Reviewers 1 & 2
Methods	Corrected sediment mass range to "0.5 to 1.5 g".	Reviewer 1
Methods	Corrected HNO_3 subscript formatting.	Reviewer 2
Methods	Replaced "three" with Roman numeral "III" for consistency with cluster naming.	Reviewer 1
Methods — Section 2.8	Expanded "Predictor variable selection and quality control": added a formal VIF assessment (threshold > 10), removing 8 collinear variables and retaining 10 for the final RDA; added the QC step removing the 3 youngest Auel Maar age-model data points; described permutation testing (999 unrestricted permutations, Bonferroni-corrected p-values); specified common data resolution (XRF–HSI at 1 mm; other variables at 100-year resolution).	Reviewer 2 & Dr. Zander
Results — Section 3.2	Replaced the "successional" framework with three climate-driven assemblages: the Pleniglacial assemblage, the transitional Bølling assemblage, and the stratified Allerød assemblage.	Reviewer 2
Results	Corrected the PC2 interpretation text (previously swapped relative to Fig. A4) and revised the Fig. A4 caption to correctly describe loading directions (Mn, Cu, Zn negative; S minor positive).	Reviewer 1
Results	Updated the RDA biplot (now Figure 5) following the VIF-based variable reduction.	Reviewer 2
Results	Replaced vague terms ("silicifiers", "red-blooming algae") with precise biomarker/producer names; cross-checked and updated Table A1 accordingly.	Reviewer 2
Results	Corrected classification of *P. ocellata* from benthic to planktonic and updated its colour coding in the figures; updated the caption of Fig. 3 (now Fig. 4) to explain the pigment-selection rationale.	Reviewer 2
Results / Supplement	Added a supplementary figure presenting absolute pigment concentrations (complementing the centred/scaled heatmap in Fig. 2).	Reviewer 2
Discussion — Section 4.2	Heading changed to "The climate-driven primary producer community shifts"; removed "succession" terminology throughout and reframed community changes as allogenic responses to temperature, wind shielding and mixing regime.	Reviewer 2
Discussion — Section 4.2	Reinterpreted the Pleniglacial community through a light-limitation/turbidity-tolerance framework, citing Kingsbury et al. (2012) and Gushulak and Cumming (2020).	Reviewer 2
Discussion — Section 4.2	Added explicit biogeochemical reasoning for inferred nitrogen limitation (generalist cyanobacteria co-occurring with *Juniperus* expansion).	Reviewer 2

Manuscript location	Summary of change	Raised by
Discussion — Section 4.2	Revised the ecological interpretation of *P. ocellata* (structured meromictic mixing regime) citing Vossel et al. (2015); removed the contradictory oligotrophic/stratified claim.	Reviewer 2
Discussion — Section 4.2	Replaced "late-successional" with the process-based descriptor "stratified Allerød assemblage".	Reviewer 2
Discussion — Section 4.2	Expanded the discussion of vivianite formation and the phosphorus-sequestration mechanism (post-depositional, anoxia- and ferrous-iron-dependent).	Reviewer 2
Discussion — Section 4.2	Corrected the okenone-based interpretation to specify chemical stratification/meromixis rather than thermal stratification; noted that the thermocline and chemocline coincide at 6–8 m in modern Holzmaar.	Reviewer 2
Discussion — Section 4.2	Removed "ecological paradox"/"plasticity" language; reframed the *S. minutulus*–bacterial-plate co-occurrence as a consequence of a compartmentalized (seasonally/spatially separated) water column.	Reviewer 2
Discussion — Section 4.2	Corrected limnological terminology (epilimnion/metalimnion/chemocline) describing the *S. minutulus* Deep Chlorophyll Maximum; added new supporting evidence from hyperspectral core scans (Figure 6) showing both discrete seasonal laminae and simultaneous-deposition micro-layers.	Reviewer 2
Discussion — Section 4.2	Integrated brGDGT-IIIa" and high %GDGT-0 molecular evidence supporting deep-water anoxia onset as early as 14.2 ka BP, cross-validated against the sedimentary pigment (green sulphur bacteria) evidence.	Dr. Zander
Discussion — Section 4.3	Added statement that *Betula* expansion and associated organic matter export acted as a reinforcing factor for lake stratification.	Reviewer 2
Discussion	Added a conservative caveat on pollen AP values regarding long-distance transport of pine.	Reviewer 2
Discussion	Removed the succession analogy in the discussion of pioneer/low-light taxa; reframed around physical mixing and landscape thresholds.	Reviewer 2
Discussion / Supplement	Calculated the chlorophyll a / pheophytin a ratio and added it to the supplementary material, with a caveat on post-coring degradation artefacts.	Reviewer 2
Discussion	Removed the speculative DOC hypothesis (no supporting proxy data).	Reviewer 2
Discussion — Sections 4.2/4.3	Expanded the discussion of cooling/aridity as the primary Younger Dryas driver, addressing lake-level data availability, ice-cover plausibility, the stratification/meromixis-breakdown mechanism, and diatom-data resolution limitations (citing García et al., 2022, for the post-hiatus Holocene diatom record).	Reviewer 2
Conclusions — Section 5	Uncoupled "internal phosphorus loading" and "hysteresis", now separately and explicitly defined.	Reviewer 2
Figure 7 (formerly the multi-proxy summary figure)	Corrected y-axis label to T_{GDGT} (TMAF, °C) and updated the caption to clarify that the GDGT-based proxy represents Temperature of Months Above Freezing.	Dr. Zander