Response to Dr. Wetterich

GENERAL REMARKS

The manuscript by Xia et al. entitled "Long-term ecosystem dynamics of an ice-poor permafrost peatland in eastern Eurasia: paleoecological insights into climate sensitivity" addresses late-Holocene to recent permafrost peatland development and ecosystem dynamics based on active-layer sampling and analyses of peat properties including density, contents of organic matter, carbon (C) and nitrogen (N), C/N ratios of the peats as well as plant macrofossil compositions. Further analytical work dealt with carbon ($\delta^{13}C$) and oxygen ($\delta^{18}O$) stable isotope compositions of *Sphagnum* cellulose samples to infer local changes in moisture conditions. The chronology of the peat cores is based on radiocarbon dating that was further employed to model peat accumulation and carbon sequestration. As the study was undertaken in an ice-poor peatland, which are generally poorly studied, the present work is certainly of interest for understanding hydrology-vegetation feedback mechanisms in permafrost ecosystems and clearly fits into the scope of Biogeosciences. The data are novel and especially the applied combination of analyses allows for deducing substantial conclusions on the functioning of ice-poor peatlands and their role as carbon sink under climate change. The conclusions are clearly based on and supported by the novel data of the present study. Needed references to previous studies are indicated and appropriate.

Response: We thank Dr. Wetterich for the positive evaluation of the manuscript.

The overall presentation of the study is very good. The text is well structured, concisely written and easy to follow. The figures and tables are clear and informative. The methods are valid and mostly sufficiently described in the Material and methods section. Thus, the applied methods are sufficiently described to make the results traceable. Only the CN analyses and the radiocarbon dating require some more information on lab procedures, analytical errors etc., which might be included either providing relevant references or a more detailed description.

Response: Thanks. We have added further information to this manuscript in this revision: CN analysis at L163-166 and radiocarbon analysis at L173-178.

A very minor flaw relates to some potentially misleading understanding in terminology such as "circumpolar thermokarst landscapes" or "longterm".

Response: We have paid attention to and evaluated these two terms in this revision. See responses below for details.

Please, find my minor remarks below referring to line (ln) numbers of the submitted ms.

Response: Thank you for thoroughly reading and reviewing the entire manuscript.

MINOR REMARKS

Title

ln1-2: I find the term "longterm" misleading as it implies longer timescales than the late Holocene captured by the present study. Please, consider rewriting the title and elsewhere in the manuscript.

Response: We have revised the title by removing the "long-term" in this revision. For the rest of the manuscript, we evaluated case by case and made changes if the word "long-term" may cause misleading to general readers. Although, as you noted, the timescale we cover is relatively short compared to the full Holocene, "long-term" here actually refers to the entire period from peat initiation to the present. This usage is common in peatland studies; for example, the term "long-term apparent C accumulation rate" is often used to describe the observed rate of carbon accumulation over the complete peatland accumulation history. Also, "long-term" is a general term that can point to a paleo-based study, which differs from laboratory or observation-based studies. So, we still keep the use of this term in some places.

Abstract

ln13-15: As permafrost of the southern hemisphere is not relevant for the present study, consider using the more common term "circumarctic permafrost" here and elsewhere in the manuscript. I'm further unsure what do you mean by "circumpolar thermokarst landscape"; firstly because landscape captures in my understanding much smaller areas with comparable inventory, which is surely not the case on circumarctic scale stretching from the Arctic Ocean across lowlands and mountains, tundra and boral taiga, etc. Secondly, noting "thermokarst" probably indicates features of permafrost degradation—thermokarst is one of them—in areas of continuous distribution of ice-rich permafrost. As I understand, you refer to the Olefeldt et al. (2016), but in context of your abstract you might just write the permafrost in your study region is ice-poor.

Olefeldt et al. (2016). https://doi.org/10.1038/ncomms13043

Response: We thank you for this thoughtful comment. First, we have changed "circumpolar" to "circum-Arctic" in this revision. Second, we agree with your criticism regarding our previous use of "circumpolar thermokarst landscape". Thus, in this revision we have replaced it with "distribution of thermokarst landscape", a term also used by Olefeldt et al. (2016). Third, we consider thermokarst to be a term of broader interest and a strong indicator of ice-rich permafrost environments undergoing degradation, which can be observed both in simple photographs and in satellite imagery. Because the thermokarst map produced by Olefeldt et al. (2016) represents an influential product in this field, we hope to use it as context to highlight the specificity of our study site in the abstract: it is not associated with present-day thermokarst due to ice-poor conditions.

ln16-17: It would be useful to restrict periods to one term throughout the manuscript. In ln376-380 you use "neoglacial period" and "late Holocene cooling" for the Little Ice Age (LIA). I further suggest defining the period by dates either from your study or from the regional literature as the timing and the expression of the LIA differed spatially. See e.g. Neukom et al. (2019).

Neukom et al. (2019). https://doi.org/10.1038/s41586-019-1401-2

Response: Thanks for this suggestion. In this revision, we have deleted "neoglacial". Regarding the LIA, in the earlier draft we wrote in section 4.1 that "The timing is within the Ming-Qing cold period in China (1321–1920) that broadly aligns with the Little Ice Age (LIA)" and citing a highly-cited regional literature, thus we do recognize the regional feature of LIA. In this revision, we wrote down the time frame of the late Holocene as "4200 cal. yr BP to present" when the term "late Holocene" was first introduced in the main text (L374). Also, we additionally mentioned that "Ming-Qing cold period in China" can be seen as the regional expression of the Little Ice Age (L402).

ln18: „…leading to rapid surficial peat accumulation."

Response: Thanks. After thinking, we prefer "having rapid surficial…", because it describes a result. No change was made.

Introduction

ln76: As you state in ln108 that your study site is within the zone of continuous permafrost distribution, but in ln122probably belonging to the zone of discontinuous permafrost distribution in general terms based on the IPA map, please be concise to avoid confusion. It might be useful to refer to the updated permafrost map by Obu et al. (2019).

Obu et al. (2019). https://doi.org/10.1016/j.earscirev.2019.04.023

Response: Thanks for this suggestion. There are different kinds of classifications of permafrost zone for our study region: it is "discontinuous" according to IPA, "predominantly continuous" according to China's cryosphere monograph publications, and "sporadic permafrost" according to Obu et al. (2019). In this revision, we describe our site as "predominantly continuous" as it matches actual observations and is consistent with previous literature (Miao et al., 2012, https://doi.org/10.5194/bg-9-4455-2012; also published on Biogeosciences). After this revision, we call our site in a more general tongue as "a boreal ice-poor peatland in permafrost-affected northern Northeast China" in Introduction (L109) and then we clearly mention in section 2.1 that we prefer "predominantly continuous" while there are also other different kinds of classifications (L123-126).

ln114-115: "… allow us to draw a comprehensive picture …"

Response: Revised.

ln114-115: Delete "any".

Response: Revised.

ln114-115: "spatial heterogeneity" of surface features and/or ground properties"

Response: Revised.

Materials and methods

ln119-124: Add information on ground temperatures, active-layer thickness (ALT) and if available further permafrost characteristics of the study region in this paragraph.

Response: In this revision, we have provided such information in this section with relevant references cited (L127-130). It reads as: "The permafrost coverage is 65–75% and the mean active layer thickness is about 1 m (Ran et al., 2012; Wen et al., 2021). The mean annual ground temperature is about –2 °C at 1.5–2.5 m depth based on the nearest borehole measurements to the study site (Li et al., 2022). The soil is dominated by ice-poor frozen soil and ice soil (Fan et al., 2023; Zhang et al., 2023), with ground ice mainly found in 30 m below the ground surface (Zhang et al., 2024)."

ln125: "The Tuqiang peatland developed on a …"

Response: Revised.

ln136-137: Polygon ponds? As the ice content of the frozen deposits is generally low?

Response: This sentence means that there is no permafrost-related outstanding surface terrain feature at this site, including ponds. The "ponds" mean thermokarst ponds that can be formed from degradation of palsa or peat plateau (see the review paper by Olefeldt et al., 2021). No change was made.

ln140: Do you know when the annual maximum ALT occurs in the study region?
Response: October. This information has been added with reference cited (L139).

ln143 and ln148-151: As you sampled the unfrozen active layer, why did you freeze your samples altering the core lengths?
Response: We froze the cores to consolidate them, making it easier to cut the cores into 1-cm slices with the electric chainsaw. In our opinion, freezing them in a refrigerator does not significantly alter the core length. Rather, it is the transportation process from the field to the lab that often causes changes in core length, as some soft peat material may become compressed or loosened. We have added this explanation in this revision (L156-159). It reads as: "Due to complications during transportation from the field to the laboratory, some core sections had experienced compression or expansion by a few centimeters compared to the original core lengths measured in the field. Assuming that compression or expansion was uniform across the cores, sample depths were corrected if necessary to match the field-measured core lengths (Table 1)."

ln155-156: Please, see my general remark and add some more information on lab procedures, analytical errors etc., which might be included either providing relevant references or a more detailed description.
Response: We have added more information about C/N ratio analysis in this revision, including lab procedures and uncertainties (L163-166). It reads as: "Bulk peat concentrations of C and nitrogen (N) of cores were analyzed at 2 cm intervals for peat sections. Additional subsamples were oven-dried, and 1.5–2 mg of the dried material was ground using a ball mill and weighed into tin capsules. Measurements were carried out with an elemental analyzer (EA3000, EuroVector, Italy). The analytical precision (1σ) was about 0.4% for C and 0.2% for N concentrations."

ln163-165: Same here: add some more information on lab equipment and the lab procedures, which might be included either providing relevant references or a more detailed description. Please, add also information on the calibration of the radiocarbon dates.
Response: Following your comment, we have provided more such information about ^{14}C measurements in this revision (L173-178). It reads as: "Three dating samples were analyzed at the newly established 0.2 MV MICADAS AMS system housed at Northeast Normal University—authors' institution (Synal et al., 2007). For these runs, samples were pretreated using the acid–base–acid method and then converted to graphite using an automatic graphitization equipment. Graphitized materials were pressed into cathodes, which were loaded into the MICADAS ion source for 14C measurements, together with standards and blanks to ensure data accuracy. The analytical precision (1σ) of 14C/12C was typically better than 2%." However, we have added the calibration of ^{14}C dates in the following section 2.3 (L203-204). It reads as: "The results of pre-bomb (before 1950) and post-bomb (after 1950) 14C dates were calibrated into calendar ages based on IntCal20 and Bomb21 NH1 curves, respectively (Reimer et al., 2020; Hua et al., 2022)."

ln170: "…from 20 inspections."
Response: Revised.

ln173-174: Move "at 1 cm or 2 cm intervals" to the end of this sentence.

Response: Revised.

ln196:

Response: Perhaps you forgot to input your comment for line 196. We have checked it out and added "per unit area" for parameter *m* in this revision (now L216).

Results

Figure 2: Move the legend to empty space where it doesn't overlap with plotted data. The TQ22-C2 seem suitable.

Response: Revised.

Figure 6: As in Fig. 6b the y-axis for cellulose $\delta^{13}C$ is shown twice, please don't extent the arrow "wetter-drier" over both axes but plot the arrow instead twice; one to each axis. Otherwise, you indicate wetter to drier conditions from –26 to –32 to –26 to –32‰.

Response: Revised.

Discussion

ln379: See comment on ln16-17.

Response: We have addressed this issue. See our previous response.

ln380: "… lateral peatland expansion …"

Response: Revised.

ln481-484: In order to characterize the quality of freeze-locked OM, there are plenty of studies from ice-rich ancient permafrost deposits such as by Haugk et al. (2022) and references therein. Haugk et al. (2022). https://doi.org/10.5194/bg-19-2079-2022

Response: Thanks for this suggestion. We read this paper in details and found this work interesting. We also used C/N ratio to characterize the OM quality. Their values are much lower than the previously reported "permafrost peat" C/N values determined in a community synthesis paper (Treat et al., 2016; https://doi.org/10.1002/2015JG003061), leading us to consider that the lack of freeze protection leads to poor decomposability of OM upon thaw. In this revision, we have made some edits to reference this literature (L499-502). It reads as: "Therefore, the present-day stronger C sink is likely related to the persistent surficial peat accumulation from *Sphagnum* litter, which must greatly outweigh any deep C loss caused by deeper active layers and aerobic conditions. Additionally, the generally low C/N ratios of basal and lower peat sections indicate the lack of freeze protection and poor decomposability (Treat et al., 2016; Haugk et al., 2022)."

Synthesis and conclusions

No comments.

Appendix

No comments.

References

Not checked.

Response: We thank Dr. Wetterich for a detailed review of this manuscript.

Response to Reviewer #2

General comments

The manuscript addresses an important topic concerning the ecosystem dynamics of an ice-poor permafrost peatland in eastern Eurasia over several centuries. The manuscript is well written and well organized; however, there are some errors or inconsistencies. I'm not entirely sure whether the authors of the manuscript are trying to convey that the peatland has experienced wetting or drying in recent decades. In the discussion — perhaps due to my misinterpretation — two opposing theories seem to be presented. The interpretative value of brown mosses was also overlooked. Detailed comments are presented below.

Response: We thank you for the positive evaluation of the manuscript. We understand that there are still places that we need to improve the clarity. In this revision, we addressed your main raised concern by further clarifying our interpretations of wet and dry conditions and also corrected errors/inconsistencies you pointed out. See our responses below for details.

Introduction

1. In **Figure 1b**, are relative elevations [m] or absolute elevations [m a.s.l.] shown? If absolute elevations are presented, the correct label should read: „Elevation (×100 m a.s.l.)".

Response: It is the absolute elevation. This has been corrected in figure caption in this revision.

2. The description of **Figure 1e** is not very informative. What does "typical surface vegetation" mean? What types of plant communities or species are shown in the figure? Possible solution: The detailed description is provided in the manuscript text, so the figure caption could include: 'Description of the taxa shown is given in the site description.

Response: Thank you. We have edited this figure caption following your suggestion (L103-105).

Study site and sampling

1. **Line 124**: Why are only precipitation values given specifically for the May to September period, while the mean annual temperature is reported without such a seasonal breakdown? Please provide an explanation or consider including temperature data for the same May–September period. I understand this seasonal focus may relate to the timing of field sampling or the growing season, so clarifying this would improve the clarity of the presentation.

Response: Great point. The reason we gave a value of May–September precipitation is because this is the period with temperature exceeding zero (growing seasons). This has been newly added to the text in this revision (L121-123). It reads as: "From nearby Mohe weather station (1959–2022), the mean annual temperature is –4.1 °C with above-zero temperatures occurring from May to September. The mean annual precipitation is 443 mm, of which 365 mm falls between May and September."

2. **Lines 128-130**: In the vegetation description, outdated species names were used, and synonyms were applied. According to current classifications: *Rhododendron tomentosum* (syn. *Ledum palustre*), *Alnus hirsuta* (syn. *Alnus sibirica*), *Calamagrostis angustifolia* (syn. *Deyeuxia angustifolia*). Please, improve accordingly throughout the text and in the figures.

Response: Thanks! We have corrected these names in this revision in both text and all figures.

3. **Table 1**. What was the measurement precision for core length, total peat section length, and *Sphagnum* peat section depth? Currently, the precision is inconsistent — some values are reported to the nearest 1 cm, while others are reported to the nearest 0.1 cm. Please standardize the precision (e.g., 49.0, 34.0, 43.0 cm).

Response: Core lengths measured in the field were recorded as integers. The cores were also cut into 1-cm slices in the laboratory for subsampling, so all depth values should ideally be integers. However, as noted in the earlier version of the manuscript, "due to complications during transportation from the field to the laboratory, some core sections experienced compression or expansion by a few centimeters compared to the original core lengths measured in the field. Assuming that compression or expansion was uniform across the cores, sample depths were corrected, if necessary, to match the field-measured core lengths" (L156-159). This correction resulted in some depth values being reported as decimals in the table. In this revision, we have retained the use of integer depth values and added an annotate to the table explaining the cause of some reported decimals.

4. **Table 1**. It would be helpful to standardize the columns "total peat section length" and "*Sphagnum* peat section depth." It is preferable to rename "total peat section length" to "peat section depth" and present values as ranges, for example, 0–49.0, 0–34.0, 0–43.0 cm. This way, the table itself would clearly indicate that the gap in the *Sphagnum* peat deposition in core TQ22-C6 was due to a change in peat type rather than the presence of a sand or clay layer.

Response: Thanks! We have edited the table following your suggestion. See updated Table 1.

5. **Table 1**: As above, what is the cause of the peat discontinuity observed in core TQ22-C6? Please, explain. I found the response to this comment only in Figure 2 (page 10).

Response: We have added an annotate to this table explaining that there is a discontinuity of *Sphagnum* peat for this core due to changes in macrofossil compositions. See updated Table 1.

Laboratory analysis

1. Almost the entire section lacks citations. It is necessary to reference the authors of the protocols applied in the laboratory and microscopic analyses.

Response: In this revision, we have cited references to support the descriptions of all laboratory methods.

2. Note that the latest classifications distinguish the following subgenera: *Rigida*, *Sphagnum*, *Acutifolia* (with sections *Squarrosa*, *Polyclada*, *Insulosa*, *Acutifolia*), *Subsecunda*, and *Cuspidata*. This classification is adopted by the British Bryological Society, as well as by the latest textbooks and field guides, for example, Laine et al. (2018): *Sphagnum* Mosses – The Stars of European Mires, University of Helsinki. Please, improve accordingly throughout the text and in the figures.

Response: Thanks for this note. After studying the new classification, we have changed "section" to "subgenus" (subg.) in the text and figures throughout the paper.

3. **Line 166:** *"Plant macrofossils from cores were analyzed at 1 cm or 2 cm intervals."* Please be precise. Which cores and peat depths were analyzed at 1 cm resolution and which at 2 cm

resolution? This information is currently too general and does not explain why a lower resolution was chosen in some sections.

Response: Thanks for this suggestion. In this revision, we have modified the text to clarify that we analyzed macrofossils at 1-cm intervals for most depths but at 2-cm intervals for some depths in lower sections of cores in which *Sphagnum* moss macrofossils are scarce (L179-180).

4. **Line 174:** As above.

Response: In this revision, we have modified the text to clarify that *Sphagnum* macrofossil carbon and oxygen isotope compositions were analyzed at 1-cm intervals for cores C1, C2, C3, C5, but at 2-cm intervals for cores C4 and C6 (L189-190).

Discussion

1. I don't quite understand the logical flow of these arguments in the discussion. Initially, the authors indicate a transition from wet conditions to dry conditions**:**

- **Lines 425-429:** *For our site, the macrofossil data show long-term stability of wet minerotrophic fen environment dominated by herbaceous vegetation since >2000 years ago, until recent decades when all cores document a consistent, abrupt transition in vegetation composition toward a present-day dominance of dry-adapted Sphagnum mosses (S. sect. Sphagnum and S. sect. Acutifolia) and an increased abundance of shrubs, both indicating a dry ombrotrophic (rain-fed) bog environment (Figs. 2 and A1–A6).*

In the next part of the text, however, the authors write:

- **Lines 510-512:** *Therefore, the shift from dry to wet conditions over the last several decades documented in our records appears to be an expected outcome for peatlands under warming-driven permafrost degradation.*

I kindly ask the authors to be more precise in their considerations.

Response: Thanks for this criticism. We apologize for the lack of clarity in the earlier version of the manuscript and greatly appreciate the opportunity for clarification here. We also received similar comments from our colleagues on the same issue. First of all, we interpret that the transition in vegetation structure from herbaceous plants to *Sphagnum* mosses (the fen-bog transition) documented in the macrofossil records of multiple cores indicates warming-driven ecosystem shift and drier conditions during the recent decades compared to the past. The ecosystem condition has become much drier compared to conditions over the previous several centuries or millennia. After *Sphagnum* mosses became the dominant vegetation, we interpret that the progressively higher $\delta^{13}C$ values of *Sphagnum* mosses toward the present indicate ongoing trends toward wetter conditions that result from either gradual subsidence or *Sphagnum*'s role in enhancing water-holding capacity of peat, *despite the overall dry condition*. In other word, we think that this ongoing wetting occurred within a broader context that the ecosystem at present should be still drier than the wet fen. The story we want to make is that "ongoing wetting against long-term drying" implies peatlands' resilience in mitigate their drying through negative feedback. We believe that our conceptual model (Figure 8) provides a good summary and visualization of above interpretations. In this revision, we have tried to better clarify our interpretations by adding some supporting text to avoid misunderstanding from readers or editing some sentences. In particular, we now use "recent" and "ongoing" to distinguish these two findings: the FBT and drier conditions are a "recent" phenomenon against the long-term peatland history resulting from warming and permafrost degradation, while the subsequent shifts to wetter conditions are an “ongoing” phenomenon and has implications for assessing future trajectories.

2. The results and discussion sections entirely overlook the potential indicative value of brown mosses such as *Polytrichum* and *Aulacomnium palustre*. This is a significant omission, especially considering that *Aulacomnium palustre* is widely recognized as a characteristic species of raised bogs and transitional peatlands. Its presence in the macrofossil diagrams should be accompanied by corresponding interpretative discussion, as it may provide important insights into environmental conditions and peatland development trajectories.

Response: We agree that our original manuscript did not sufficiently address the indicative value of non-*Sphagnum* mosses. In response, we have added relevant text to discuss these data in the revised version. To summarize our revisions: our PCA analysis shows that non-*Sphagnum* mosses load in the opposite direction to shrubs along the PC2 axis. We interpret this as a possible indication of competitive interactions, where the dominance of one group may suppress the other (L307-309; “The second component explains 12.1% of variance and separates non-*Sphagnum* mosses and Ericaceae, likely related to microhabitat conditions or competition for resources.”). However, we acknowledge that this observation contributes little to explaining broader shifts in ecosystem structure. In our view, species such as *Polytrichum spp.* and *Aulacomnium palustre* are indicative of relatively drier conditions. Their presence in *Sphagnum* peat—but absence in sedge peat—supports the interpretation of a stratigraphic transition from wet fen to dry bog (L447; “… and an increased abundance of shrubs as well as dry-adapted *Polytrichum* spp. and *Aulacomnium palustre* (Faber et al., 2016), all indicating a dry ombrotrophic (rain-fed) bog environment (Figs. 2 and A1–A6).”). We also notice that the core TQ22-C4, which spans a shorter temporal window, shows a notable peak in *A. palustre* abundance (>20%) in the middle of transition from sedge peat to *Sphagnum* peat, possibly reflecting a short-lived period of transitional peatland conditions (L451-452; “For newly formed peat at core TQ22-C3, a fast sedge-*Polytrichum*/*Aulacomnium*-*Sphagnum* succession can be distinguished (Fig. A3).”). These points have been added to the manuscript in this revision.