

Clim. Past Discuss., author comment AC1  
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## Reply on RC1

Angelica Feurdean et al.

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Author comment on "Holocene wildfire regimes in western Siberia: interaction between peatland moisture conditions and the composition of plant functional types" by Angelica Feurdean et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-125-AC1>, 2021

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## Anonymous Referee #1

Referee comment on "Holocene wildfire regimes in forested peatlands in western Siberia: interaction between peatland moisture conditions and the composition of plant functional types" by Angelica Feurdean et al., Clim. Past Discuss., <https://doi.org/10.5194/cp-2021-125-RC1>, 2021

*This is a review for the manuscript "Holocene wildfire regimes in forested peatlands in western Siberia: interaction between peatland moisture conditions and the composition of plant functional types" by Angelica Feurdean et al. The study explores the interactions between peatland moisture, vegetation composition and fire regime from two peatland sites in Western Siberia. The study provides new and valuable insights of the relationship between the different proxies and the significance of the findings for ongoing and future climate change in boreal peatland ecosystems. Overall, I find the paper well-structured and the multiproxy data clearly presented with sound interpretations of the paleorecords. However, I would encourage the authors to make some of the fonts in figures larger to be print-friendly. I am not an expert for amoeba and thus my review comments are focussed primarily on the paleofire and vegetation records, which align best with my own research background.*

R: We thank the reviewer for valuable comments that helped to improve the current version of the paper and for encouraging words about our work.

## General comments

*Introduction: I think the authors mix fire (events) with fire regimes a few times and I suggest checking that the words are used correctly as fire regime is a defined term describing the general pattern in which fires naturally occur in a particular ecosystem over an extended period of time not an individual fire. See e.g., line 65 ff.*

R: Thank you for this observation. In the revised version, we have replaced the term fire regime whenever this was incorrectly used.

*Chronology: I would like to see more details on the chronology establishment in the main text, for example what material was dated and also why the authors included so many bulk dates instead of picking terrestrial macrofossils for dating purposes. The site description with local Betula growths implies that the record should contain enough datable terrestrial macrofossils for 14C purposes thus it's difficult to understand, why bulk material was dated instead.*

R: Although plant macrofossils are the preferred material over bulk for radiocarbon dating, bulk peat has been shown to yield reliable age information in Europe and western Siberia (Holmquist et al., 2016). However, larger differences were observed between dates on bulk peat and wood (see details in Holmquist et al., 2016). In revising this manuscript, we will include additional information on the plant material dated. As for the question of why so many bulk dates, no identifiable plant remains were found in the bottom part of both profiles. For the remaining parts of both profiles, we have alternated radiocarbon dating on plant macrofossils with those on bulk peat as the radiocarbon measurements on plant remains yielded slightly more inconsistent age, as opposed to those on bulk peat that were in consistent chronological order. We will discuss this slightly more in the revised version of the manuscript.

*Charcoal morphology: The authors state that they grouped charcoal in woody and nonwoody at both sites based on morphology criteria. However, they also state that the charcoal was classified based on length ratio for one site to achieve an additional classification of graminoids vs leaves/wood. It is unclear why they chose two methods to achieve the ratio for one site while only one method for the other site. This is especially surprising since the two classifications largely overlap for interpretation (graminae vs. woody/leafs and non-woody vs woody).*

R: Our standard method to determine charcoal types was via charcoal morphological classification which can identify a series of fuel types (see Fig S3A). At a later date, we additionally tested the length to width ratio method (L:W), which is a more simple way to determine fuel type. The reason that the L:W ratio method was only tested at UC site, is because we did not have peat material left at Rybnaya. In the process of charcoal morphological identification, we often break charcoal particles, thus measuring the length and width on samples already used for charcoal morphological identification would have likely introduced errors. We, therefore, refrained from testing the L:W ratio method at Rybnaya.

*Numerical analyses: I am a bit surprised to see how local indicators (watertable, macroscopic charcoal) and regional indicators (pollen) were combined for correlation calculations. Usually, microscopic charcoal >10um is used for regional reconstructions that would align better with the catchment of micron-size pollen. The authors should consider adding a justification why in this case such correlations across spatial scales are considered more useful than combining proxys with a similar catchment estimate? The difference in the geographic scale covered by the individual proxys seems also the most likely explanation why the authors couldn't find some of the statistical correlations they were expecting between watertable, fire and vegetation indicators*

R: We agree that the spatial scale of the water table and macrocharcoal is smaller than that of pollen. The macrocharcoal curve is strongly influenced by the smaller fraction (150-300 micron), which can have a more regional component (closer to pollen), although

the comparison of sub-recent charcoal values with satellite images indicates a localized fire picture recorded by our macrocharcoal record. In revising this paper, we will include the microcharcoal fraction in statistical analysis.

*Comparison between recent charcoal and satellite images: In my opinion the first paragraphs are presenting results rather than discussion of the results and should be moved to the results section. For the discussion of the relationship between charcoal based fire reconstructions and satellite-derived fire detection, I would like to see some references to other studies such as e.g. Adolf et al. (2018) or Daniau et al. (2017)*

R: We agree and have moved parts of this chapter at the Results under a new heading reading: "Fire-type identification from charcoal, satellite images, and forest statistics" and kept only a part of the text in the Discussions. References suggested are included in the revised paper.

*Betula pollen: Please clarify if you determined tree type Birch and shrub type birch (Betula nana) pollen (e.g. see Birks 1968). If Betula could not be identified, it may contain Betula nana as well, not only Betula trees, which should be considered for discussing the results.*

R: R: We have not attempted separation of pollen of *etula* tree from shrub type. However we have not noticed *Betula nana* locally at any of the two sites cored.

Specific comments

*Line 64-65 Reference needed: When these aerosols persist in the atmosphere, it leads to a medium-term increase in albedo and ultimately to regional cooling.*

R: This sentence links to the citation above: Rogers et al., 2015.

*Line 65ff: The sentence needs rewording, it currently reads : fire regimes ... are surface fires, see general comment above*

R: Thank you, we have reworded this sentence: " Fire types in the Siberian boreal forests are often litter-fuelled surface fires ...".

*Line 97: add spore analysis to pollen analysis*

R: Added.

*Line 99: I think it would be better to add "plant/vegetation, ...)" to "community" to clarify what community the authors refer to*

R: Adjusted to: " We have determined how peatland moisture has interacted with vegetation composition (with regard to plant functional types)..."

*Line 103: the term "charcoal site selection" seems misleading as the authors conduct a multiproxy paleoecological study not just charcoal analysis*

*Line 112: I believe the coring was conducted near the river not on the river*

R: Thank you, revised: "...on the river replaced with ...near the river".

*Line 114: Local peatland? Vegetation?*

R: The local vegetation replaced with local peatland vegetation.

*Line 115: What species are the dead tree trunks?*

R: *Pinus*, but we do not know which species. We have rephrased this sentence: "...and standing dead *Pinus* tree trunks at Ulukh-Chayakh Mire".

*Line 124: add unit (cal yr BP) to the surface age -69 and -67*

R: Added

*Line 129: fire history is not inferred from the peat sample but from the charcoal information in these samples. This needs to be rephrased.*

R: Agree and rephrased: "We inferred changes in local-scale fire regime based on charcoal particles identified in peat samples of 2 cm<sup>3</sup> extracted at 1 cm contiguous intervals".

*Line 137: Reference for charcoal catchment. Consider for example Adolf et al., 2018*

R: Added.

*Line 230ff: provide absolute values of change of understory vegetation values rather than "more", "predominantly"*

R: We will add these values in the revised manuscript.

*Line 240f: mention water table values as well for the second part of the paragraph rather than relative descriptions.*

R: Added. "At Ulukh-Chayakh, the water table was high (around 15 cm below surface) between 3.5 and 2.5 ka, 2 and 0.5 ka, and towards the present day. There, times of low water table (20-34 cm below surface) occurred between 6 and 3.5 ka, 2.5 and 2 ka, and around 0.5 ka."

*Line 244ff: the lack of correlation could be due to the different proxies reflecting local and regional spatial scales.*

R: We will run an additional analysis on microcharcoal fractions and discuss the influence of spatial scale on the statistical output.

*Line 265f: charcoal occurrence should be presented as concentrations of particles per volume or better as charcoal influx per year and a given area rather than "pieces".*

R: We agree and additionally add influx values.

*Line 289: add "for both sites"?*

R: Added.

*Line 320: determination of the detrital element Ti should be mentioned in the methods section*

R: The geochemical analysis section is now an individual chapter at the Methods.

*Line 401: Scenario should be replaced by period if considering how it is referred to in the following sentences.*

R: Agree and revised to: "At a Holocene timescale, we found two periods with contrasting moisture-vegetation-fire interactions".

*Line 411ff: references needed for future climate scenarios in Siberia*

R: IPCC, 2021.

*Appendix A1: font too small to read when printed in A4; Figure 1: fonts are very small in the maps, indicate latitudes and longitudes as well as direction of North for map b-d; Figure 6: labels for correlation: increase font size. All figures showing Holocene records: x-axis, I believe you show kilo-years here not years, adjust unit accordingly*

R: Agree and we will enlarge the fonts and kilo-years in the revised manuscript.

Technical comments

*Line 64: this leads =they lead*

R: Corrected to this.

*Line 97: Multiproxy analysis should be used in plural form here*

R: Thank you, replaced with: Multiproxy analyses.

*Line 99: "with regard to" instead of "in"*

R: Replaced with "...with regard to..."

*Line 122: add supplementary to file S1*

*Line 171f: using vs used, somehow the sentence structure is wrong*

R: Revised to: "This was done by averaging the Z-scores and smoothing them to 300 years with a locally weighted regression and a 95% confidence envelope on the same sites used for composite biomass burning reconstructions in the R palaeofire package".

*Line 354: change primally to primarily*

R: Corrected.

*Line 378: "from" or similar word missing*

R: Corrected to: "It should be acknowledged that each identified fire peak could have come from a few consecutive fire events that ...

References:

*Adolf, C., Wunderle, S., Colombaroli, D., Weber, H., Gobet, E., Heiri, O., ... & Tinner, W. (2018). The sedimentary and remote sensing reflection of biomass burning in Europe.*

*Global Ecology and Biogeography*, 27(2), 199-212.

Daniau, R. F., Arneth, A., Forrest, M., Hantson, S., & Kehrwald, N. (2017). *Historic global biomass burning emissions for CMIP6 (BB4CMIP) based on merging satellite observations with proxies and fire models (1750–2015)*.

Birks, H. J. (1968). *The identification of Betula nana pollen*. *New Phytologist*, 67(2), 309-314.

R: Reference added.

**Cited papers:**

Holmquist JR, Finkelstein SA, Garneau M, Massa C, Yu Z, MacDonald GM. A comparison of radiocarbon ages derived from bulk peat and selected plant macrofossils in basal peat cores from circum-arctic peatlands. *Quaternary Geochronology*. 2016; 31:53-61.