Author response to RC3

Dear referee,

we would like to thank you for your time spent on reviewing our manuscript and value your honestly voiced concerns. Please find below your original review comments in black and our author responses in green:

This is a carefully prepared and well written manuscript. The topic is certainly important: fire records are needed from Siberia. Despite the detailed analyses, this is nevertheless a difficult site to interpret. I am not convinced by the results or their current interpretation for the following main reasons, which have been articulated in detail by the other reviewers.

Thank you for your assessment! We hope that our responses and improvements following your individual remarks below will clarify the results and provide an interpretation that better captures what our data can and cannot tell.

1) The chronology is very difficult. The offset of 1000 yr in the bulk sediment series may be approximately right, given there is carbonate bedrock in the vicinity. However, the mixed-up macrofossil dates suggest that material of different ages becomes incorporated into the sediment matrix, so why not the charcoal?

In contrast to the bulk sediment used for radiocarbon age dating, most of the matrix of which is thought to be either autochthonous or transported to the lake via erosional events or it's inflow stream before being reworked and finally accumulating on the lake bottom, charcoal particles follow an additional route of spreading through the environment via the air, lifted by the convection from fires themselves (primary input). We would expect the secondary input of charcoal particles from "old" fires (i.e. up to few tens of years ago) to be limited by fairly dense vegetation along the lake shore and the low angle of surrounding slopes. However, even such a lagged charcoal input would only apply to fires within the catchment area, whereas the suspected source area goes well beyond the catchment's boundary. Additionally, such secondary input would only lead to a "smoothing" of the charcoal chronology by increasing overall charcoal amounts in samples. Most dated macrofossils, on the other hand, are small structures from ground-dwelling plants that likely deposited after time-lagged secondary input only. For these reasons, the majority of charcoal particles are thought to be deposited soon after fires took place, instead of being a mere remnant of permafrost thaw like the other macrofossils potentially are.
As pointed out in another review, the charcoal could thus itself provide an opportunity to constrain the age offset throughout the sediment core. However, within the present study this is not feasible due to three main reasons: (i) small amounts of charcoal in present samples; (ii) scarce and valuable sediment material from an effortful helicopter expedition, preventing us from obtaining more material for that purpose; (iii) rigorous preparation steps of small charcoal particles as outlined by Bird (2013). Despite these difficulties, we will consider this promising approach for future studies.

2) The FRI's seem extraordinarily short. An average of 43, in a ~2000 yr series that has a quiescent period of ~600 yr is high, and when broken down into zones/phases, estimated FRI levels of 14 yr do not sound at all realistic. Nothing as short as this is reported from the region.

We agree that the mean FRI of phase 2 seems extraordinarily short at just 14 yrs. However, it is important to note how our charcoal record differs from reconstructions using other archives/proxies. We have not emphasized this enough on this in our current version, but the revised manuscript will feature a clear description of three main reasons behind the short FRIs and differences to other

studies: (i) the large lake size allows to incorporate charcoal from a large source area, thus capturing more fires than more locally constrained studies at small lakes or with tree ring chronologies. This leads to (ii): we can interpret only fire episodes instead of individual fires, because of the large source area and a lower-intensity surface fire regime (i.e. a single peak of the charcoal record is not an individual fire, but rather a phase of increase fire activity). (iii) Our statistical approach considers adjacent peaks above a threshold as individual fire episodes, where conventionally, only the highest peak is considered. We assume that the ability of a single fire to create multiple adjacent outstanding peaks is limited at this study site due to quick recovery of dense vegetation on low angle slopes, limiting secondary input, and sediment mixing processes likely not exceeding the sampling resolution of the charcoal record.
Furthermore, most existing studies utilize tree ring chronologies which might only record fires that did not kill a tree population at the same site, whereas the charcoal record includes also such higher-intensity fires across a larger region. Also, most of these studies (e.g. Kharuk et al., 2008, 2011) seem to be located further north, while fires are most frequent in central and southern Yakutia (see Ivanova 1996). It does seem like few degrees in latitude can have a striking impact on fire frequency, especially when combined with differences in stand-specific moisture conditions. For example, a mean FRI of 15 yrs was reported by Takahashi (2006) near Yakutsk, and similarly low values are found in Ivanova (1996) in central Yakutia, with the longest FRIs not exceeding 40–50 yrs on moist sites and only half of that on dry sites.

3) The relevance of calculating FRI's for different types of charcoal morphology is not explained and no convincing implications of doing this, or the results, are presented. Thus despite excellent detailed methodology, it would be difficult to draw much that is useful from this study. This study would be better presented as simpler types of time series and together with the pollen (as suggested by the other reviewers); it is far preferable to treat the data appropriately than to develop complex analyses that could well provide a misleading picture of events.

Applying our statistical approach not only to the sum of charcoal particles, but also to specific sub-categories of size classes or morphotypes, aims at evaluating how these different categories contribute to the overall charcoal record and differ from each other. The results are included in Fig. 5c, d of the manuscript, showing the peak frequencies and background components for the various size classes and morphotype groups, with more detailed individual diagrams available in the Supplement. We totally agree that the purpose of this has not been sufficiently stated, and we will add this information to the updated manuscript in L238: "This was done in order to assess in detail their individual contribution to the sum of all particles, the way they capture a fire signal to see if different charcoal groups represent different types of fires, potential relationships between charcoal particle size and source area, and whether certain charcoal types represent varying fuel types over time."
One relevant result of applying the statistics to the size classes individually is briefly stated in L322, which will be expanded as follows: "The three charcoal morphotype groups show a similar temporal pattern for their background and peak component distributions (Fig. 6c, d), mostly mirroring the decreased variability in the second half of the record as described for the sum of all particles. However, when assessed individually, large particles have a generally lower variability than the other size classes, whereas the variability of irregular morphotypes is higher than that of elongated or angular particles (see Supplement)." Since charcoal morphotypes are not yet well constrained in their interpretations, we feel that their inclusion within this high-resolution record and the separate application of our statistics is justified and might be of interest to future studies looking for comparisons.
In general, our analyses aim at incorporating uncertainties, which is the prime reason for including the robust CHAR approach, and we try to communicate those as best as we can. Thus, we agree that

the usage of certain terms (i.e. “FRI”, “fire frequency”) can be misleading here without proper definition, especially when put in comparison to studies using other proxies. For that reason, we will include a new paragraph at the beginning of the discussion to provide definitions of the peak component as “fire episodes”, the expected charcoal source area and how, with these two key points of information, the term “FRI” is to be understood in the present study. In the end, this is, to the best of our knowledge, the first study working on a high-resolution macroscopic charcoal record from eastern Siberia, while also developing new statistical approaches to better capture uncertainties. Therefore, from our point of view, the present study provides a strongly needed foundation upon which further research will be able improve.

Other comments

L80 useful to mention any estimates of FRI here in description of region, seeing this is a fire study.

We agree and will add FRI estimates from studies set in Yakutia to the description of the study region in L110: “Wildfires most frequently occur in the central to southern regions of Yakutia, with varying stand-specific mean fire return intervals not exceeding 20–50 yrs at around 60°N, 120°E (Ivanova 1996) and 15 yrs near Yakutsk (Takahashi, 2006). Longer estimates, increasing with higher latitude, were found by Ponomarev et al. (2016) in central Siberia with 80 yrs at 62°N, 200 yrs at 66°N and 300 yrs at 71°N, with similar ranges reported by Kharuk et al. (2016).”

L187 need to clarify a bit more about the samples that had no char analysis; was it just the 11?

Unfortunately, 11 samples (all within the top 40 cm of the sediment core) were used for a destructive type of analysis test before morphotype classification could be applied. However, total charcoal concentrations and size class distributions of these samples were obtained beforehand. This will be clarified in the revised manuscript: “At that time, 11 samples distributed within the top 40 cm of the sediment core had already been used for other purposes and thus lack information on morphotype classification (total charcoal concentrations and size classes are available for all samples).”

L206 well established

Thank you!

L250 Between lines 250 and 263 there is repetition, this part needs re-writing. In general, the discussion of the radiocarbon dates is long and over-complicated. It would help the reader to place a statement about how mixed the radiocarbon dates are right up front and state the whole problem much more directly.

We think that this discussion is a necessary part of this paper as the chronology is the largest source of uncertainty, and it also serves as a case study on a prominent issue with paleoenvironmental studies set in permafrost regions. We believe issues of that kind should be thoroughly discussed, and not put as a side note. Many studies depend on such chronologies whilst not providing in detail the assumptions or potential issues behind them (see Lacourse and Gajewski, 2020), which by doing so might motivate our community to eventually find more effective solutions. We agree that the readability of this part could be improved, so we will try to streamline this discussion in the updated manuscript. To better introduce the reader, a sentence briefly summarizing the age dating outcome will be added in L252: “Bulk sediment ^{14}C ages indicate a rather linear chronology, with only the deepest two samples returning similar ages. In contrast to this, the macrofossils ^{14}C ages do not indicate a clear chronological pattern and are not in good agreement with the bulk sediment samples“. In L261, the sentence part “which shows a recent surface age” will be deleted because of repetition.

L266 sentence beginning "(II) Macrofossil 14C ages...." Not quite sure what this means

The plant macrofossil dated at ~10.000 ^{14}C BP in a surrounding sediment matrix that is likely only at ~100 yrs BP shows directly how the presence of "old carbon" can, when included in a bulk sediment sample, artificially lead to an older-than-expected age result. This sentence should emphasize that there is direct evidence for the presence of old carbon with the mixed macrofossil ages, apart from the age offset between surface $^{210}Pb/^{137}Cs$ and ^{14}C results. It will be rephrased to: "Macrofossil ^{14}C ages older than the surrounding sediment matrix provide direct evidence for the potential influence of old carbon on bulk sediment samples at various depths (e.g. macrofossil age of 9902 ± 97 ^{14}C yrs BP in sediment that dates back to only c. 100 yrs BP according to the parallel core's Pb/Cs age)."

L345 The relevance of the PCA is hard to see; more explanation in caption would help. Given the lack of impact of the morphology data (and Fig 4 only mentioned in results once), this could be omitted.

The PCA indicates that there is no clear clustering of charcoal morphotype or size classes with increasing core depth. Additionally, it visualizes correlations between the charcoal classes, showing how irregular type M particles are more closely associated with small particles. We think that these are relevant bits of information, as they also add to the discussion of weak morphotype correlations in L445-455. To further highlight the relevance of the PCA, we will add to L330: "Furthermore, the PCA indicates that there are rather weak grouping patterns of morphotype or size-class distributions in samples of increasing core depth and age, potentially reflecting a stable vegetation composition around the lake." Additionally, the PCA caption in L345 will be expanded with: "[…], with colored dots representing potential grouping patterns of charcoal assemblages with increasing age."

L400. "In general, FRIs increase with latitude due to lower incoming solar radiation, shorter fire seasons, and lower flammability of moist biomass (Kharuk,2016; Kharuk et al., 2011), which likely contributes to a relatively short mean FRI at Lake Khamra." Explain further? This site has a short FRI therefore...it is further south than other sites? The argument is not clear.

We argue that at Lake Khamra, located further South compared to many of the other cited studies, it is in that sense not unexpected to see shorter FRIs (in addition to differences in archive and proxy used, as noted above). To make this better understandable, we will re-phrase this sentence: "In general, fire frequency tends to increase with decreasing latitude due to higher solar radiation, longer fire seasons, and higher flammability of dry biomass (Ivanova, 1996; Kharuk, 2016; Kharuk et al., 2011), which likely contributes to a relatively short mean FRI at Lake Khamra when compared to studies set further north."

L 419 argument is a bit hard to follow in sentence beginning "However, the present "

In essence, the original argument by Enache and Cumming (2007) is that fragile morphotypes in lake sediment originate preliminary from primary input through the air, as they would likely be destroyed during longer distances of secondary input via surface runoff. When the catchment to lake-area ratio is large, the distance of potential surface runoff increases and would therefore "filter" secondary input of fragile morphotypes. However, even though Lake Khamra has a large catchment to lake-area ratio, the charcoal record is dominated by fragile particles. This is why we suspect other factors, like the type of biomass burning, to be mainly responsible for the morphotype distribution we see in the sediment. To improve the wording of this reasoning, the paragraph from L415-L421 is rephrased to: "Enache and Cumming (2007) explain how a large catchment to lake-area ratio might favour secondary deposition of compact/stable morphotypes, while fragile morphotypes are more prone to fragmentation during surface runoff and thus rather represent primary input through the air. However, the catchment to lake-area ratio at Lake Khamra (23:1), as well as the share of fragile charcoal particles (types F, M, and S alone make up >80%), are both comparably large. This might indicate that morphotype distribution within the record is not controlled by potential filtering effects of secondary charcoal transport, but rather by the type of biomass burning. This is also implied by the predominantly primary charcoal input through the air due to the densely vegetated surrounding slopes, and mirrors the stable vegetation composition seen in the pollen record."

L459 there is low peak frequency during much of what might be thought of as the MO and higher one toward the end of the LIA, so there is not a very good fit to climate – this is over-interpreted, especially given the caveats provided and the difficult chronology

It is correct that the general timeframe of the MO/MCA (c. 950 – 1250 CE according to Mann et al. 2009) does not match the inferred fire activity from the present charcoal record. Even though this is acknowledged in L459, stating the underlying chronology as an additional issue, we agree that this part should be rephrased to better represent its foundation in our data. On the contrary, low CHAR during phase 3, in a time before industrialization and rapid population growth and with the stable vegetation composition, would lead us to suspect a cooler and/or wetter climate as the main driver. Seeing then how various other studies find evidence for the LIA during that time frame, we think it is not unlikely that this is captured by our record as well. We suggest the following revision of this paragraph:
"Although it has been demonstrated that the timing and extent of supposedly ubiquitous warmer or cooler climatic phases are in fact heterogeneous (Guiot et al., 2010), evidence for their occurrence in Siberia is seen in proxy studies (Churakova Sidorova et al., 2020; Feurdean et al., 2019; Kharuk et al., 2010; Osborn and Briffa, 2006), albeit less pronounced when it comes to vegetation response in the West Siberian Lowland (Philben et al., 2014). Neukom et al. (2019) show how such climatic periods arising from averaged reconstructions at many individual study sites are not spatially or temporally coherent on the global scale, and conclude that environmental reconstructions "should not be forced to fit into global narratives or epochs". This might be especially true for studies of a single site, using chronologies that have ^{14}C reservoir effects. However, the low fire activity in the latter half of phase 3 (900–1750 CE) strikingly coincides with the Little Ice Age (LIA), when in many regions of the Northern Hemisphere a cooler climate prevailed from c. 1400 to 1700 CE. In contrast to this, high fire activity during phase 2 not matching the proposed timing of the warmer Medieval Climate Anomaly (MCA, c. 950 to 1250 CE) demonstrates the limitations of such comparisons based solely upon the ^{14}C-dated segment of the charcoal record (estimates of LIA and MCA durations from Mann et al., 2009)."

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