Author response to RC1 by Philip Higuera

Dear Philip Higuera,

thank you for your review of our manuscript and the time and effort put into it! We welcome your constructive feedback, which we value and without doubt improves this manuscript. Please find below your original comments in black and our author responses in green:

****General comments****

The paper presents well-developed datasets from what I imagine was a hard-earned lake-sediment record in a region lacking long-term fire history information. The text is well written. The graphics are clear and well-developed. The new "robust" charcoal analysis approach is refreshing. The community needs fire history information from this part of the boreal forest, and this is well motivated in the introduction.

Thank you, we appreciate your assessment!

Based on the comments below, this record does not seem well-suited for peak analysis and interpretation of peaks as individual fire events. Given lake size, a surface-fire component in the fire regime, and chronological uncertainty from old-carbon effects, interpreting total charcoal (concentration and/or accumulation rates) and a smoothed derivation may be more justified. The spatial integration of this record, given the large lake size, could be an advantage to help more reasonably compare general trends in charcoal accumulation (as a proxy for regional biomass burning) to regional climate, vegetation, human history.

We mostly agree with this general statement, which is why we chose to use such smoothed derivations of the charcoal record in the comparisons to climate/vegetation/human history (background component, which results from a LOESS applied to charcoal influx, and smoothed "peak frequency"). However, your comment made clear to us that some of our data description and its interpretation do not match the broader level of detail as provided by the lake archive and its chronology in its current state. We will therefore revise passages with the goal of capturing all factors that differentiate this record from smaller lakes or strictly local and well-constrained proxies such as tree ring studies. Please refer to our responses on your individual remarks below for more detail on the applied revisions. An important suggested addition will be a new paragraph at the beginning of the discussion describing clearly how our terms (e.g., "fire episode", "FRI") are defined, what signals they capture based on our archive and the lake's size, and which uncertainties need to be stressed when interpreting the data (see our response to [2] (i)).

My two main concerns are described below:

[1] Chronology:

I appreciate the many challenges of developing chronologies from boreal lakes, and the authors are upfront about these challenges. Nonetheless, some important limitations of the chronology remain and seem to not be transferred through to the interpretation of the proxies. Most concerning is the assumption that a single old carbon offset applied to the entire core. The same approach was used in Vyse et al. (2020), but without further citation or justification. How robust is this assumption; does it also assume the rate of permafrost thaw is non-varying over time? Any additional information supporting these assumptions would help potentially quell these concerns. In line 370 in the current paper, the authors note "...any changes in the magnitude of this reservoir-like effect are impossible to quantify." But, couldn't that assumption be tested by dating the charcoal that is assumed to be deposited at the same time as the sediment? The macrofossil dates likely reflect materials with a long terrestrial residence, but the charcoal pieces – to be interpreted as they are – should reflect relatively instantaneous deposition. A similar approach (based on non-charred terrestrial macrofossils) has been used to quantify variability in the age offset over time in a tundra lake: Gaglioti et al. 2014 (https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1002/2014JG002688).

Regarding the assumption of a stable old carbon effect over time:

As an integral part of a paleoenvironmental study that needs to derive accumulation rates from an age-depth model, we understand that the assumption of a constant old carbon related age-offset raises concern. While at the sediment surface we can estimate the magnitude of the offset, we do not have this possibility below c. 25 cm depth. Unfortunately, as you stated, the dated plant macrofossils, as a potential way to quantify bulk sediment offsets through time, seem to be part of the problem at this lake, as they might derive also from older permanently frozen deposits that have thawed.
If we would ignore the (important) ^{210}Pb/^{137}Cs ages, a possible and often used approach would be to only adjust the surface ^{14}C age to the year of core extraction and then connect that to the other, non-adjusted ^{14}C ages (e.g. Biskaborn et al., 2012, 2013). This approach of assuming a recent ^{14}C surface age is also used in studies that do not have any ^{14}C surface age estimates (e.g. Katamura et al., 2009; Klemm et al., 2016). Lacourse and Gajewski (2020) found that about two thirds of 80 recently published age-depth models manually assign a surface age without reporting the details of that decision, thus may not be able to see any potential carbon offset issues. In this case, however, such an approach not only assumes that any input of old carbon only happened during recent times (which we know from deeper, mixed macrofossil ages that it did not), but it would also create a strong and unlikely shift in sedimentation rate between the top two ^{14}C ages. The Lake Khamra sediment appearance does not offer any evidence for such a change, its homogenous composition rather being a reason for suspecting a linear sedimentation rate. This is underlined by reports of stable lake conditions and thermokarst processes in central Yakutia during the late Holocene (at least up to the rapid warming during past decades, e.g. Ulrich et al., 2019; Pestryakova et al., 2012).
In summary, we recognize that assuming a constant age offset due to old carbon input throughout the record has its limitations. However, the evidence from ^{210}Pb/^{137}Cs, bulk and macrofossil ^{14}C dates and sediment appearance neither hint at a highly varying age offset, nor at a highly variable sedimentation rate. That being said, we fully agree that we have to carry this underlying and known uncertainties through to a proper interpretation of our results, together with the robust CHAR analysis that explicitly considers known age and analytical uncertainties. We will additionally follow recommendations on best practices for age-depth model reporting by Lacourse and Gajewski (2020), by adding the lab numbers to the table of ^{14}C dated samples and clearly stating that MICADAS uses AMS ^{14}C dating.

Regarding age dating of charcoal particles:

Dating charcoal particles, which are suspected to derive mostly from primary input, is a very reasonable idea! However, after careful consideration, we think that within this study it is not feasible for the following reasons: (i) scarce sediment material – the sediment core provided valuable material from a helicopter-based expedition in Yakutia, where liners of 6 cm diameter had been used. Even with the combined charcoal and pollen extraction, most of recovered sediment now has either already been used or is required by other analyses from colleagues, making it difficult to obtain new and potentially larger sediment samples for the purpose of extracting higher amounts of charcoal for age dating. (ii) limited amount of charcoal in the existing samples – even samples with the highest number of charcoal particles are suspected to barely provide enough material for a reliable dating process on their own, seeing how 6 out of our 15 macrofossil samples were also too small (<= 10 µg C). We would thus have to combine/destroy multiple charcoal samples across the record, potentially also losing the precision needed to effectively constrain the age offset. Unfortunately, together with the rigorous preparation steps for radiocarbon dating of very small charcoal particles as recommended by Bird (2013), these factors rule out a test of this method within the scope of this present study. Despite this, we do think that it would definitely be worth a try as we now increasingly start to use 9 cm liners, yielding more sediment material to work with, which is why we will include that option in future studies and refer to the inspiring study by Gaglioti et al. (2014) you kindly provided.

The chronology issue is important given that (i) CHAR calculations are a function of sediment accumulation rates, and (ii) the record is interpreted at fairly fine temporal scales - e.g., Phase 2 is only 300 yr long, and there are interpretations of the LIA and MCA. Interpreting fire history at these scales is already pushing the limits of 14C-based chronologies, and the added uncertainty of dating bulk sediment with known old carbon contributions seems additionally constraining.

The assumption of a stable age offset, the main concern voiced about the chronology above, would only change the absolute timing of all samples deeper than the second ^{14}C age, but less affect the distribution of CHAR values relative to each other. This is because with or without the offset, we have no reason to assume abrupt and strong shifts in the sedimentation rate based on the sedimentology. For the upper samples, on the other hand, we provide a well-constrained ^{210}Pb/^{137}Cs chronology and do have evidence for the age offset, including an estimate of its magnitude. The four phases are therefore not dependent on the age information.
However, we agree that any comparison to temporally constrained events (like the LIA) must be adequately justified. We will revise the discussion of climate forcing (4.2.2) to be clearer about the uncertainties in any such comparison, e.g. by stating: "[…] high fire activity during phase 2 not matching the proposed timing of the warmer Medieval Climate Anomaly […] demonstrates the limitations of such comparisons based solely upon the ^{14}C-dated segment of the charcoal record […]." However, other studies on similar timescales (e.g. Churakova Sidorova et al., 2020; Feurdean et al., 2019) have reported that such climatic phases left behind a visible impact on various proxy reconstructions in Siberia. With a marked decrease in fire activity probably being climate-related, it seems likely that low CHAR in phase 3 could indeed correspond to a colder climate as reported for the LIA. We think this assumption is justified also as the reliable, near-surface ^{210}Pb/^{137}Cs ages allow to constrain the LIA time frame better than older climatic periods.

[2] Charcoal peak analysis and interpretation:

(i) Lake size: The rationale and tools developed for peak analysis (e.g., decomposition approach in general, and as reflected in CharAnalsyis) assume a small lake surface area, and that charcoal primarily comes from airborne deposition. For example, most lakes used for peak analysis are < 10 ha (e.g., Alaskan lakes summarized by Hoecker et al. 2020). A lake with 4.6 km2 (460 ha) surface area is quite different, and this distinction is key to point out and carry though the interpretation of the record. The large lake size could be an advantage – integrating more area than a small lake – but it does not lend itself then to interpreting individual peaks in the record.

For example, interpreting intervals between peaks is significantly different for a lake this size vs. a lake < 10 ha, since the large lake integrates a much larger area. At a minimum, it's confusing to compare mean FRIs from a lake with such a large surface area to mean FRI estimates from tree rings (summarized over a small area), small lakes, or modern fire history records (e.g. summarized as fire rotation periods).

You are absolutely right, we did not pay enough attention to a discussion of the influence of lake size and the subsequent meaning of our descriptive terminology. Hence, we have now revised the discussion part of the manuscript and include the definitions of terms used, as well as summarize the archive's benefits and downsides at the beginning, before comparisons to any other studies are made. Now, charcoal peaks and their meaning as fire episodes are clearly put into context of the lake size, the surface fire regime, and an estimate of charcoal source area, which in turn enables to reader to see how the FRIs of this study may differ from others. The newly added paragraph reads as follows:

"We use the term "fire episode" instead of "fire event" when referring to identified peaks in the record. This should highlight that multiple fires could have contributed to any peak in the charcoal record. Consequentially, the FRIs of this study should be regarded rather as "fire episode return intervals", marking the time span between periods of increased fire occurrence within the charcoal source area. This is because the relatively large water surface area of Lake Khamra likely captures charcoal from a larger source area when compared to smaller lakes. A larger source area of charcoal is directly related with an increased number of fires that were able to contribute charcoal to the present record. The gentle and densely vegetated slopes framing the catchment limit secondary charcoal input (Whitlock and Larsen, 2001), thus emphasizing a direct fire signal with predominantly primary input through the air. However, a higher number of captured fires from a larger source area also means that the comparability of FRIs reconstructed in this study to those obtained from smaller lakes or tree ring chronologies may be limited, since those usually convey direct fire impact and are more locally constrained (Remy et al., 2018).
Although it has been shown that larger charcoal particles originate generally within a few hundred metres of a lake archive (Clark et al., 1998; Higuera et al., 2007; Ohlson and Tryterud, 2000), they have also been observed to travel further depending on vegetation and fire conditions (Peters and Higuera, 2007; Pisaric, 2002; Tinner et al., 2006; Woodward and Haines, 2020). As wildfires in the Siberian boreal forest are predominantly considered low-intensity surface fires (de Groot et al., 2013, Rogers et al., 2015), the potential of the resulting convection to transport large charcoal particles is probably limited compared to high-intensity crown fires. We therefore assume a charcoal source area between few hundred metres directly around the lake for low-intensity fires (Conedera et al., 2009) and increasing distance of up to several kilometres for more intense fires producing stronger convection, resulting in a total source area estimate of up to c. 100 km². Even though some extreme fires may well surpass this estimate and, occasionally, small fires within might fail to contribute sufficient amounts of charcoal, identified fire episodes in the charcoal record should still be biased towards fires closer to the lake, especially when they consist of predominantly large charcoal particles (Conedera et al., 2009; Whitlock and Larsen, 2001). The uncertainty regarding any source area estimate highlights the need for further spatial calibration studies. Also, it remains unknown whether the charcoal record might be dominated more by close-proximity low-intensity fires, or by high-intensity fires from a larger distance. An estimation of fire intensity from charcoal particles (e.g. measuring charcoal reflectance, Hudspith et al., 2015; or the charcoal's oxygen to carbon ratio, Sumon Reza et al., 2020) could potentially clarify the respective contributions and thus help with better constraining the source area."

Among some other minor changes regarding a clearer communication of our results, we will also rephrase the misleading statement on FRIs as "fire occurrences": "Interpreting the classic peak component as temporally restricted increases in fire activity, 50 such fire episodes […] were identified." Also, we added to the conclusion: "The large lake size may be an important factor behind these shorter FRIs, since it is associated with a large charcoal source area of multiple kilometres from the lake and thus capturing more fires compared to more locally constrained studies."

(ii) Peak analysis and consecutive samples above a threshold: The peak analysis presented here appears to consider all samples above the threshold – even in adjacent samples – as peaks and thus fire events. This is quite different from “classic” CharAnalysis, as implied in the methods, and this is unlike examples I am familiar with from the literature (e.g., CharAnalysis or predecessors CHAPS and Charster). Typically, it is recognized that a single event can create a charcoal peak that spans multiple samples (and the first or maximum value is used as the peak date); other approaches would benefit from explicitly describing the framework and rationale used, and provide any empirical support. The results are challenging to accept: e.g. adjacent samples above the threshold are interpreted as distinct fire events, such that a mean FRI of 14 yr is inferred for Phase 2 (line 304). Is there any modern calibration work that supports this type of interpretation (i.e., that consecutive samples above a threshold indeed reflect different fires)?

You are correct, our approach here considers all peaks reaching above the threshold as “fire episodes”. We do not assume that one peak necessarily equals one individual fire, as it integrates multiple years over a fairly large source area. We rather think of fire episodes as periods of increased fire activity, based on one or more fires. From remote sensing observations we know that fires burned within Lake Khamra’s catchment in 2006/7 and 2014 (Fig. 1b of the manuscript), just 8 years apart. Considering the median sample resolution of 6 years, such fires could well have been responsible for two consecutive peaks in the charcoal record. After fires, we expect the dense surface vegetation of the lake catchment to quickly recover (i.e., < 6 years), and together with the sedge belt and wetland around the lake acting as an efficient barrier for secondary input via surface runoff; both aspects have been observed in the field (see expedition reports from this region in Kruse et al., 2019; Fuchs et al., 2021). Additionally, any remaining charcoal that is deposited after a fire would have to spread across the large lake basin, probably leading to a smoothing rather than a second peak. This leaves charcoal fixation and sediment mixing processes as a remaining option for the distribution of one fire’s charcoal across multiple samples, however, the charcoal record shows many individual peaks that are clearly distinct from their adjacent samples. This indicates that charcoal redistribution effects during sedimentation did not exceed the sampling resolution. Based on this rationale we decided to include every peak above threshold. To make this clear to the reader and provide this reasoning for any future discussion, we will include this reasoning briefly in the methods section. However, we will also feature a minimum estimate of fire episodes (and thus a maximum estimate of FRI) by considering only one of directly adjacent fire episodes that simultaneously have a SNI >3.

The methods paragraph will be revised as follows: “All peak component values exceeding the threshold were subsequently identified as signal (representing fire episodes) and marked when they overlapped with periods of SNI >3, indicating whether they are clearly distinct from surrounding noise. While usually in instances of multiple consecutive samples above threshold only the highest peak is recognized, we included all of them as fire episodes in this case. Recent fires within the lake’s catchment that were just 8 years apart, as well as a predominantly primary input of charcoal and quick recovery of the filtering wetland vegetation around the large lake, lead to suspect that the ability of a single fire to create high charcoal counts in multiple samples is limited at this site. However, an absolute minimum estimate of the number of fire episodes was obtained by considering only one fire episode from a given peak in CHAR, and only those that are clearly separated from noise (i.e. they also overlapped with phases of SNI >3).”

Consequently, a maximum estimate of a mean FRI will be mentioned in the results and discussion sections next to the standard estimate of mean FRI at 43 yrs: “Even if this mean FRI would strongly overestimate the number of included fire episodes, we would not expect the true mean FRI to exceed the maximum estimate of 95 yrs.”

(iii) Peak analysis in a surface-fire regime: More broadly, a surface-fire regime is not expected to create distinct peaks in CHAR (as noted in the text). Peak analysis is generally considered most suited for high-severity fire regimes. Thus, it’s not surprising that the SNI is at or below 3 for nearly 1/2 of the record; the large lake size likely also contributes to the low SNI values. Interpreting peaks in CHAR from a low-severity fire regime, with a record with SNI < 3, should recognize that many low-intensity surface fires are likely missed. But again...all of this in in the context of small lakes – the larger lake adds more “noise” to this rationale, and calls into question the value/meaning of return intervals in the first place.

We will include this important relationship between fire proximity and fire intensity in our revised discussion of charcoal source area: “Even though some extreme fires may well surpass this estimate and, occasionally, small fires within might fail to contribute sufficient amounts of charcoal, identified fire episodes in the charcoal record should still be biased towards fires closer to the lake, especially when they consist of predominantly large charcoal particles (Conedera et al., 2009; Whitlock and Larsen, 2001).”
So, even though the fire that burned approximately half of Lake Khamra’s 107 km² catchment and beyond in 2014 was likely of lower intensity than usual forest fires elsewhere (MODIS-based mean fire radiative power of c. 76 MW, compared to generally higher values in Canada after Rogers et al., 2015), we would expect it to leave a distinct mark in the sediment archive. Unfortunately, the relationship between proximity and intensity is difficult to further assess for the fire episodes identified in the charcoal record due to the temporal mismatch of recent satellite observations and the sediment archive’s coverage, as well as the difficulty of differentiating low- from high-intensity reconstructed fires in the sedimentary record so far. However, we agree that the predominant fire regime is indeed an additional reason for increased noise, and thus the generally low SNI, and as such we will include it in the updated manuscript in the discussion: “The mostly rather low SNI, which is below 3 for c. one third of the record, might result from the lower intensity surface fire regime found around Lake Khamra. Such fires probably create less distinct peaks than the high intensity crown fires of other regions, especially considering the large lake size.”

****Specific comments****

L 44: Consider Kelly et al. 2016 (Nature Geoscience 6:79-82) as a useful reference for boreal forest carbon balance changing with changing fire regimes.

This is a great study to include here. We will add it as reference to L44 in the revised manuscript.

L 165: Nice way to save sediment here, with the dual pollen-charcoal subsampling.

Thank you! Since sediment material from expeditions is quite valuable, maybe this might benefit some other research by potentially enabling more proxies to be analyzed from the same sediment core.

L 189-190: Nice way to help account for some counting uncertainty.

Thank you, we will try to carry that through to future studies and see where we can improve on capturing this uncertainty.

L 206: This is slightly misleading, as there appears to be important differences between what was done here and what is implemented in CharAnalayis. For example:

(i) CharAnalsyis does not identify adjacent samples above the threshold as peaks, as is done here; and (ii) it appears that the Gaussian mixture model used here may be different from the one used in CharAnalysis, if not the actual algorithm, then in the way it’s applied. See notes on Fig. 1, below. Overall, it would be more accurate to say something like: “First, we used a set of analyses to decompose the charcoal records into peak and background signals, similar to well-established approaches applied in CharAnalysis." Upon reading the original text...it really sounds like the same methods of CharAnalysis were translated into R (which I wish were true!).

You are correct, this is indeed misleading. We are, of course, clarifying this part in the revised manuscript. Similar to your suggestion, L206 will read: "First, we applied a set of analyses referred to as "classic CHAR" (R script by Dietze et al., 2019), similar to the charcoal record decomposition in the well-established "CharAnalysis" (Higuera et al., 2009)." Furthermore, the repository and R script names associated with this manuscript will be changed to "CharcoalFireReconstruction".

L 116: A better paper to describe how a Gaussian mixture model is used to identify a threshold would be Gavin et al. 2006 (Ecology 87: 1722-1732 – first to use Gaussian mixture model) or Higuera et al. 2011. To my knowledge this method was not established yet in 2003.

You are right, Whitlock and Anderson (2003) describe the purpose of the peak component's threshold and not the specific method, so we will instead add "(Gavin et al., 2006; Higuera et al., 2011)".

L 218: This trade-off between "Longer window widths..." that yield a higher SNI values and "a strong averaging of the record" is in part what motivates the use of local thresholds (e.g., in CharAnalysis). Local thresholds also reduced the impacts from any changes in CHAR due to change in sediment accumulation rate.

We justified the use of a global threshold with the relatively uniform sedimentation rate as implied by the age-depth-model (with or without adjustment of the age offset), as well as the very homogenous vegetation composition that is thought to provide steady fuel types during the comparably short time covered by the record.
Nevertheless, we tested whether the application of a tentative local threshold would result in improved SNI or a different interpretation of reconstructed fire history (see Fig. 1 below). To be comparable to the global threshold used in the manuscript, we calculated it in a similar manner, using the same window width that was applied to the background LOESS. Briefly, the Gaussian mixture model was applied to the positive peaks of the peak component in a moving window. The local threshold was then obtained by applying a default LOESS to the resulting values to minimize the impact of outliers. Expectedly, this local threshold version identified less fire episodes during phase 2, and more during phase 3. However, the difference is not so large as to change key points of our discussion. Also, this threshold variant does not seem to improve the SNI. Of course, other methods of smoothing and calculating a local threshold will likely yield slightly different results as well. With the improved definitions on the terms used in our discussion and more appropriate comparisons to other studies mainly based on the general distribution of peak and background CHAR, we view our interpretation as not being majorly affected by the choice of threshold method. For that reason, we suggest keeping the current and approach of a global threshold in the revised manuscript, but adding Fig. 1 below to the Appendix for the reader to compare. The local threshold test will be briefly described in 2.4 Statistical methods. The R script now also contains the option to apply this tentative local threshold (see https://github.com/rglueckler/CharcoalFireReconstructionR/tree/revised).

L 221: This rationale justifying why peaks are interpreted when the SNI is consistently < 3 is not very convincing.

We will remove the sentence starting in L221 in the updated manuscript. Instead, to more clearly describe our rationale for application of the SNI, we will add to L218: "[…] and marked when they overlapped with periods of SNI >3, indicating whether they are clearly distinct from surrounding noise." The SNI will furthermore now be used to provide a minimum estimate of fire episodes, as noted above.

L 225: Unlike in Dietze et al. (2019), the differences between this “robust” approach and the “classic” approach applied is more challenging to make sense of in this record. E.g., the “robust CHAR peak” in panel (e) is hard to reconcile with “classic” results, particularly in Phase 2.

We applied the robust approach to account for the age and counting uncertainties, inherent to any reconstruction, with the resulting trends being a conservative estimate of the changes in past fire regimes. The relatively large age uncertainties, as well as a high counting uncertainty of 20% when compared to Dietze et al. (2019), lead to a high degree of smoothing from resampled distributions. Therefore, we would not expect robust CHAR to mirror the peaks found in classic CHAR, but rather only those phases that are prominent enough to stand out even with these added uncertainties. However, we have revised the script behind the calculation of robust CHAR to, among some other minor changes, optionally include a sample aggregation step used in Dietze et al. (2019). This has so far not been implemented in this manuscript, and allows for a combination of multiple samples to better scale our high-resolution record to the large added uncertainties. With this revised script (available at https://github.com/rglueckler/CharcoalFireReconstructionR/tree/revised), we re-calculated robust CHAR and will include this new version in the updated manuscript and the diagrams in its Supplement. It does not change the general trends observed in the current version, but it improves the fit between classic and robust CHAR, especially in phase 4 (see comparison in Fig. 2 below).

L 301, 204: Are these mean FRIs of 31 and 14 years because multiple peaks in a row are interpreted as fire events? I keep double checking this...but this must be the case. I don’t understand how consecutive samples above the threshold are interpreted as separate/independent fire events. This needs some empirical (and/or theoretical) support.

Yes, consecutive fire episodes are part of the FRIs. The definition of fire episodes is important here, which we have not sufficiently explained in the current manuscript, as well as our rationale for including adjacent identified peaks in the FRIs. As we have laid out above, the revised manuscript provides descriptions of both aspects, while also featuring a maximum FRI estimate, based on the reduced number of fire episodes when adjacent ones from the same CHAR peak distribution are ignored (and only considering those with SNI >3).

L 334 – Figure 3: The threshold identified (Fig. 1a) seems very low – e.g., there are many negative samples (anomalies) that would exceed the threshold, were it inverted to be negative, particularly in Phase 1-2. Conceptually, samples exceeding the same threshold value below 0 suggest something is off in the parameters, as there is no interpretation for negative departures beyond the threshold. This type of record, even though short, is the type that motivates local thresholds, as there are changes in both the background and variability in CHAR over the different phases. *But again...this is usually in the context of smaller lakes.

Negative samples of the peak component visualization (Fig. 3a in the manuscript) are residuals from the subtraction of the background component from the CHAR timeseries. This means that those samples did not contribute any, or only some few, charcoal particles to the record. For this reason, the threshold is only based on the positive peaks within the peak component, as only they contain the information about fire episodes we are looking for. Negative samples that might exceed an inverted threshold are more frequent in phases 1 and 2 because of the higher variability in CHAR in these phases, resulting in more samples with few charcoal particles in phases of a higher background component. We tested whether capturing this difference in background component and variability in our peak detection made an important difference by testing a tentative local threshold as laid out for a similar remark above (also see results in Fig. 1 below).

L 344: Why not plot this based on age, instead of depth? All other analyses are presented by age – it seems odd to have this plotted by depth.

We agree, samples within the PCA will be plotted according to their age instead of depth in the revised manuscript.

L 368-371: As noted above, this seems like a major constraint of the chronology, and thus interpretation. It's good that it's pointed out here, but it's then hard to reconcile this with interpreting changes with the LIA or phases that are 300 yr long.

As described above, the phases are not set regarding chronological information or some compared climate data, but rather depend on the charcoal distribution in the sediment core. However, we understand that we can, among other improvements, better communicate the general chronological uncertainty in the discussion. Especially in 4.2.2, we will emphasize the limitation of comparing our record to climatic phases, e.g. by stating: "[…] high fire activity during phase 2 not matching the proposed timing of the warmer Medieval Climate Anomaly […] demonstrates the limitations of such comparisons based solely upon the ^{14}C-dated segment of the charcoal record […]".

L 385: Would we expect one site to necessarily reflect regional or global patterns in fire activity, at these smaller time scales? If so, it would be worth including the potential mechanisms for such synchronous fire activity.

With a multitude of factors influencing fire activity on a regional level when considering shorter timescales, we would not necessarily expect that. Nevertheless, it seems that an increase in fires in the 18th to 19th centuries, followed by a decrease within the 20th century, is recorded at multiple sites across studied regions (examples are listed as references in this part of the manuscript). Since there are so many other differing factors between these various study sites, synchronizing mechanisms could include wide-spread climatic events (such as the LIA) that might have influenced fires regardless of the ecosystem they occurred in (for example, a likely longer snow season during the LIA could have reduced the length of the fire season). With the timeframe from 18th century to present days, population growth and cultural transformations during the industrialization could be a further candidate for such a mechanism, as described in Marlon et al. (2008) for a similar timescale to our record. Since this is a better reference to include here when compared to Marlon et al. (2013), which looks at the whole Holocene, we will update L387: "A global charcoal record synthesis for the last two millennia by Marlon et al. (2008) indicates decreasing biomass burning from c. 0 CE towards the industrial era, where, after a maximum around 1850 CE, it decreases with the onset of the 20th century. A potential explanation for this similar trend during the most recent centuries, observed across many study sites set in different regions of the world, could be the onset of industrialization with an accompanying change in land use and subsequent fire management. However, charcoal records from Siberia are underrepresented […]".

L 405: Yes – shorter intervals between peaks in small charcoal, compared to peak in large charcoal – make sense based on a larger source area for smaller charcoal. It's key to tell readers what spatial scale, approximately, you think this record integrates, prior to this point in the text. The spatial scale reflected is key to interpreting the FRI values described above.

We totally agree, and will include this information directly in a new paragraph at the beginning of the discussion as described above: "We therefore assume a charcoal source area between few hundred metres directly around the lake for low-intensity fires (Conedera et al., 2009) and increasing distance of up to several kilometres for more intense fires producing stronger convection, resulting in a total source area estimate of up to c. 100 km². Even though some extreme fires may well surpass this estimate and, occasionally, small fires within might fail to contribute sufficient amounts of charcoal, identified fire episodes in the charcoal record should still be biased towards fires closer to the lake, especially when they consist of predominantly large charcoal particles (Conedera et al., 2009; Whitlock and Larsen, 2001).".

L 407-410: This comparison conflates a bit the difference between "just dispersal" vs, "enough charcoal to create a peak that is distinct from background charcoal." Large pieces can travel far...and distinct peaks can still be strongly biased towards "local" fires.

We agree, it is the distinct peaks that are commonly interpreted to represent local fires, but not necessarily the individual particles a peak is made of. We illustrated the different size classes' travel distance to constrain a rough estimate of source area. This part will be rephrased and featured in the source area discussion as noted above: "Although it has been shown that larger charcoal particles originate generally within a few hundred metres of a lake archive (Clark et al., 1998; Higuera et al., 2007; Ohlson and Tryterud, 2000), they have also been observed to travel further depending on vegetation and fire conditions (Peters and Higuera, 2007; Pisaric, 2002; Tinner et al., 2006; Woodward and Haines, 2020). As wildfires in the Siberian boreal forest are predominantly considered low-intensity surface fires (de Groot et al., 2013, Rogers et al., 2015), the potential of the resulting convection to transport large charcoal particles is probably limited compared to high-intensity crown fires. […] Even though some extreme fires may well surpass this estimate and, occasionally, small fires within might fail to contribute sufficient amounts of charcoal, identified fire episodes in the charcoal record should still be biased towards fires closer to the lake, especially when they consist of predominantly large charcoal particles (Conedera et al., 2009; Whitlock and Larsen, 2001)."

L 412-414: This assumption of the spatial scale reflected by the charcoal records would be much more useful if it came before presenting the FRI information – the meaning of FRI (and mean FRI) is contingent upon the spatial scale reflected or integrated across. Given the circumference of the lake, what does this translate to in terms of km^2? That's the key piece of information.

Thank you for pointing out how we could clarify the estimated charcoal source area. We agree that this is essential for all of the following discussion, which is why we will add an estimate in square km in its beginning as described above: "We therefore assume a charcoal source area between few hundred metres directly around the lake for low-intensity fires (Conedera et al., 2009) and increasing distance of up to several kilometres for more intense fires producing stronger convection, resulting in a total source area estimate of up to c. 100 km²."

L 441: Could this "contrast" between the current study and others, to some extent, reflect the differences in temporal scale? The current study is "only" 2000 yr long, whereas several of the studies cited span much longer time periods. Mechanisms for vegetation change vary over these different scales.

We agree, this is a good point and likely to have an effect on such comparisons! We will acknowledge that by adding a sentence in L442 in the revised manuscript: "A reason for this might be that studies on longer timescales capture long-term concurrent trends in both fire and vegetation that are not observable in the last c. 2000 yrs alone (e.g. glacial to interglacial changes in vegetation distribution and temperature; see Marlon et al., 2013)."

L 443-444: And...the very large spatial footprint integrated by the pollen record in this lake is key here. A clear pollen signal would require persistent vegetation change over a large area.

Correct. This is what we wanted to imply in L450 stating the pollen source area, but we feel that this was not done sufficiently. Thus, we will expand this aspect starting in L449: "In addition to differences in proxy source area and taphonomy between macroscopic charcoal and pollen grains, a variety of factors likely obscures traces of potential fire impacts: surface fires in the deciduous forests in central Yakutia mostly result in the elimination of only a share of the affected tree population depending on fire intensity (Matveev and Usoltzev, 1996). This might not be enough to leave behind a clear mark in the pollen record, which also covers a source area that is probably way larger than the area affected by fire. Herbs or shrubs, on the other hand, may recover too quickly for changes to be detected in our record with a median temporal sampling resolution of 6 yrs. Any potential mixing processes and the residence time of pollen grains before settling in the lake sediment may further diminish visibility of a fire impact […]"

L 467-468: Doesn't this also directly apply to comparing the fire history record reconstructed here (i.e., one site, with a chronology subject to 14C reservoir effects) to any proxy with a well-constrained chronology?

From our point of view, such comparisons might hint towards the general viability of our reconstruction, although the constraints you mention need to be kept in mind. For instance, phase 3 with generally lower CHAR fits the timing of the LIA as indicated by other studies from Siberia and is also near the well-constrained top part of the chronology. For the lower half of the record, on the other hand, it is stated that comparability might be hampered by the assumptions behind our chronology (L485), thereby acknowledging that the problem might well lie within the present record instead of some other forcing yet to be recognized. We would also like to include the limiting factor of only having one site for now, by revising the last sentence of this paragraph (L487): "Yet, an impact of colder Arctic temperatures on the reconstructed low fire activity in phase 2 at Lake Khamra seems probable. Since we can currently rely only on this one record, more palaeoclimatic reconstructions and high-resolution charcoal records from the region could greatly improve the validity of such inferred links between climate and the fire regime."

L 543: Some citations would help identify the other studies noted here.

You are right, and this was mainly resembling global synthesis studies (e.g. Marlon et al., 2013), however, due to differences in temporal resolution and our underlying uncertainties we decided to drop the first part of this sentence. In the updated manuscript, L543 will read: "Recent levels of charcoal accumulation at Lake Khamra are not unprecedented within the last two millennia."

Figure 1: Classic CHAR comparing the **alternative local threshold (red)** to the global threshold (orange). Vertical dashed lines mark the different phases of the fire regime. (a) Classic CHAR peak component (dark grey bars = signal, light grey bars = noise, colored lines = threshold versions). (b) SNI after Kelly et al. (2011) (black horizontal line = SNI cutoff value of 3). (c) Classic CHAR sum (black line = interpolated CHAR, blue line = LOESS representing the CHAR background component, red/orange marks = fire episodes for local and global threshold versions, respectively, with color = fire episodes with SNI >3, in grey = fire episodes with SNI <3).

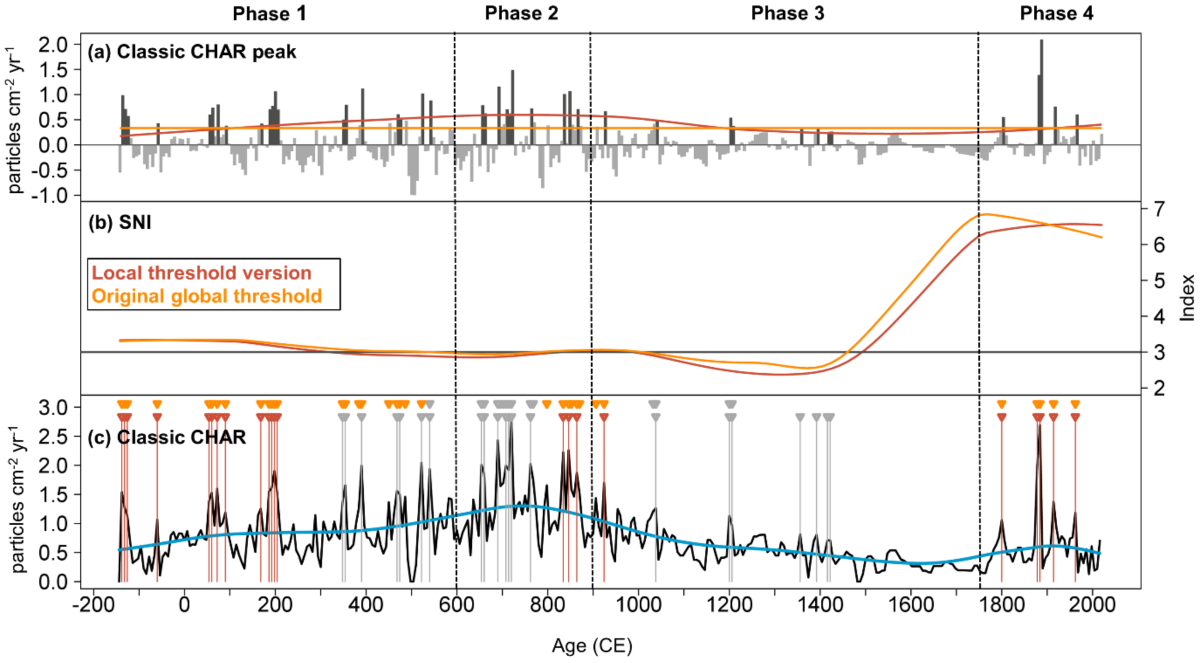
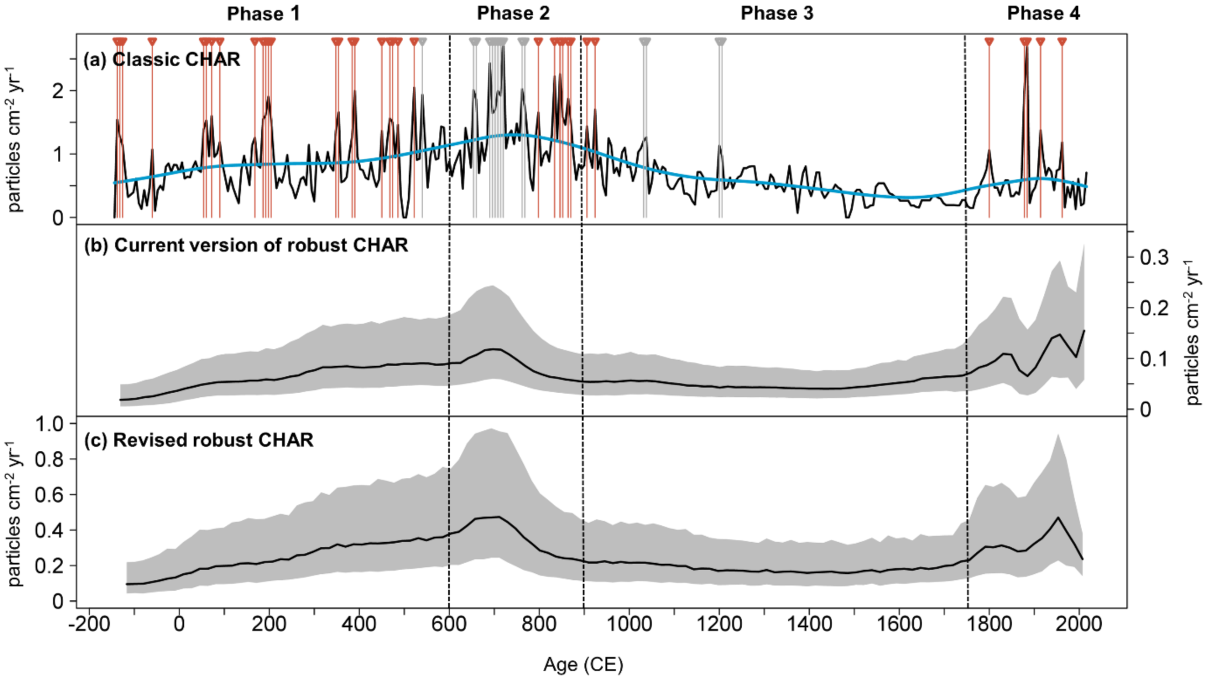


Figure 2: Comparison of **revised robust CHAR (including aggregation of three consecutive samples)** with its current version. Vertical dashed lines mark the different phases of the fire regime. (a) Classic CHAR peak component (dark grey bars = signal, light grey bars = noise, dashed horizontal line = threshold). (b) Current version of robust CHAR. (c) Revised robust CHAR. For (b) and (c): black line = median, grey area = interquartile range.



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