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Interactive comment on "Wildfire history of the boreal forest of southwestern Yakutia (Siberia) over the last two millennia documented by a lake-sedimentary charcoal record" *by* Ramesh Glückler et al.

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****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.

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.

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).

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.

[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).

(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" CharAnalsyis, 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 peaks that span 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 result 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)?

(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 $\frac{1}{2}$ 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 $\approx<$ 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.

****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.

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

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

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.

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.

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

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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.

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?

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

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