Zhang et al. “Holocene fire regimes around the Altai-Sayan Mountains and adjacent plains: interaction with climate and vegetation types”

## General comments

This compelling study traces a major Holocene divergence in fire regimes across the Altai-Sayan ecoregion, from early climate-mediated controls (temperature in alpine zones, moisture in plains) to anthropogenic dominance after ~2.0 cal kyr BP. A crucial insight is the role of biotic feedbacks, such as the spread of fire-resistant *Pinus sylvestris* in the western Sayan Mountains, in long-term fire reduction.

The human-era findings reveal a stark regional contrast: increased fire frequency due to agropastoralism in the west and north, versus decreased frequency in the Khangai Mountains due to grazing-induced fuel fragmentation. The manuscript convincingly frames modern Central Asian fire risk as an emergent outcome of long-term interactions among climate, vegetation, and humans.

The manuscript is strong and presents essential findings. To maximize its impact and readiness for publication, the following suggestions are offered to enhance clarity and rigor.

## Specific comments

### Abstract

P1.L17. “(i.e., west Siberian Plain)” to “(i.e., the west Siberian Plain)”

### Introduction

While the first two paragraphs are excellent, they lean heavily on modern ecology. The authors should ensure the transition to the Holocene feels central rather than an “add-on.”

P3.L52. “cascading infrastructure collapse” is highly technical and a socioeconomic point. So, the authors could clarify the connection between the collapse and permafrost thaw, or other fire-related impacts, to strengthen the point. For example, “cascading ecological and permafrost degradation”

P3.L59. Listing the components, “climatic drivers, ignition probability, and fuel complex properties,” is good. However, a brief, clear description of why the authors are focusing on paleorecords (i.e., their ability to resolve the long-term, non-linear relationships among these factors) would be a smooth lead-in to the Altay Sayan section.

P4.L76. “The Altai-Sayan region lies …” to “Within this crucial northern Eurasian context, the Altai-Sayan region lies …” for more directly linking it back to the core area discussed in the first paragraph.

The third paragraph is quite dense. It mixes modern satellite data, human management (fire prevention), and the limitations of instrumental records. The authors should break this into two paragraphs or use a clearer transition when moving from the region's ecological sensitivity to the limitations of current data.

P5.L111-112. “(~11750-0 cal. Yr BP)via the analysis of charcoal in Achit Nuur” to “(~11750-0 cal. Yr BP) using high-resolution macroscopic/microscopic charcoal analysis from Achit Nuur”, for example, to be specific about the proxy type if possible.

### Physiographic Settings

P6.L133. The explanation of the NAO and Siberian High is reasonable; however, to enhance this, the authors could consider a brief mention of how these systems changed during the Holocene Thermal Maximum or the Little Ice Age, as this bridges the gap between the modern setting and their results.

P6.L153. "… margins is" to "… margins are"

P7.L161-162. The authors cite Sun et al. (2013) regarding the core collection and dating methods. If this paper presents a reanalysis or a new proxy application of the same core, the authors should clearly state what is new in this study (e.g., the charcoal record) versus what was previously published (e.g., the pollen or lithology).

P7.L163. "mean size" to "mean grain size" (?)

P7.L165-167. The authors mentioned a 2100-year reservoir correction. This is a significant correction. The authors should briefly state whether this offset is assumed to be constant throughout the Holocene, thereby addressing readers' questions about "hard water" effects or changes in carbon cycling over time.

P7.L170. "This study just focused on the Holocene interval (i.e., the past ~11,750 cal. Yr BP)." is slightly abrupt. A smoother transition might link this temporal focus to the overall goal stated in 2.3 ("to investigate the spatial heterogeneities of fire regimes …")

P7.L173. "Total 24 sites including Achit Nuur (Table 1) were" to "A total of 24 sites, including Achit Nuur (Table 1), were"

P7.L174. "spatial heterogeneities of" to "spatial heterogeneity in"

P7.L174. "in the Altai-Sayan Mountains and adjacent plains" to "across the Altai-Sayan Mountains and adjacent plains"

P7.L180. "the Irtysh river valley" to "the Irtysh River valley"

P7.L187. "the Great Vasyugan Mire on the Western Siberia" to "the Great Vasyugan Mire in Western Siberia"

P7.L189. "of West Siberian plain" to "of the West Siberian plain"

P7.L190. "Ulukh-Chayakh Mire (…) located" to "Ulukh-Chayakh Mire (…) is located"

P8.L197-198. "on western piedmont" to "on the western piedmont"

P8.L208. "(Region D, …)" to "(Region E, …)"

P8.L209. "2150 m" to "2150 m a.s.l."

P8.L210. "in the central Altai part of Russian Altai" to "in the central part of Russian Altai"

P8.L218. "Lugovoe mire" to "Lugovoe Mire"

**Methods**

The methods are well-organized and outline distinct analytical procedures. However, to meet the journal's high standards for clarity, reproducibility, and technical accuracy, some sections need improvement. The primary concerns are inconsistent terminology, insufficient methodological detail, and formatting issues.

P9.L227. "characoal" to "charcoal"

P9.L227. The authors mention "standard pollen extraction method." In general, for charcoal, researchers distinguish between pollen-slide charcoal (microscopic) and macroscopic

charcoal (sieving). Since the authors used glycerin and a microscope, they are looking at microscopic charcoal. Thus, the authors should explicitly state the size fraction (e.g., >50 μm, 50-125 μm) or mention that these are "pollen-slide charcoal" counts to distinguish them from macroscopic charcoal (which is usually sieved).

P9.L230. “using particle counting method” to “using the particle counting method”; The authors should specify the magnification(s) used for identification and counting. They could also clarify the "particle counting method"—is this a standard point-count or area method? Finally, the authors should define the size categories counted (e.g., >50 μm, 50-125 μm), as this is critical for interpreting fire signals.

P9.L231. “moving pieces” to “microscope slides”, “wet mounts”, or “aqueous glycerin mounts” (?)

P9.L231-232. “using particle counting method” to “using the particle counting method”

P9.L232. “under Lycra microscope” to “under a Leica microscope”

P9.L233. “grains” to “particles” or “fragments”

P9.L235 and L238. The authors should use Word Equation Editor or LaTeX for their concentration and influx formulas

P9.236-237. “n is the number of additional lycopodium spores,” to “n is the number of additional lycopodium spores per mount,” (?)

P9.L237. “the statistical number” to “the total count”

P9.L238. Regarding the influx calculation, the authors’ definition of Influx (CHAR) is conceptually correct, but the phrasing “dividing by the sediment rate (yr/cm)” is awkward. It is more standard to say: “Calculated by multiplying the concentration by the sedimentation rate (cm/yr).” Moreover, I recommend adding a brief sentence about the chronology (or a reference to the section where the age-depth model is described), as the CHAR calculation depends entirely on the accuracy of that model. Thus, please mention how the authors handled chronological uncertainty. Did the authors account for age-depth model errors when calculating influx?

The GAM model specification is reasonable. To complete it, the authors should state how model convergence and adequacy were assessed (e.g., diagnostic plots of residuals, checks for curvature). The authors could either specify the basis dimension (k) used or state that it was checked for adequacy via gam.check().

P9.L245. The authors should briefly explain why these six taxa were selected (e.g., are they the dominant fire-sensitive species in your study area?).

P9.L247. Using a quasi-Poisson distribution is a good choice. However, it would be useful to explicitly state whether temporal autocorrelation (common in paleoecological time-series data) was accounted for and whether the GAM structure included it. Paleoecological time-series data often violate the assumption of independence because samples are ordered in time. Briefly mentioning whether you accounted for temporal autocorrelation (e.g., using a correlation structure or checking the autocorrelation function of the residuals) would make the methodology more robust.

P9.L250-251. The phrase “eliminating the need for additional assumptions regarding the probability distribution of the data” might be slightly overstated. While quasi-Poisson is flexible, it still assumes a specific mean-variance relationship. For example, the sentence could

be softened to "...corrects for overdispersion without requiring a specific parametric distribution for the data."

P10.L256. "three-step process" to "a three-step process"

The opening sentence in Section 3.3 mentions "standardize charcoal influx," but the first step (min-max transformation) is a normalization to a [0,1] range, which typically refers to Z-scoring. The three-step process is valid, but the introductory sentence could be more precise, for example, "To render charcoal influx records from different sites comparable, a three-step transformation procedure was applied to calculate comparable Z-scores …"

P10.L258. "Mini-max transformation" to "Min-max transformation" (?)

P10.L259. Before the equation, the authors could briefly describe the process, for example, "(1) Min-Max transformation: Raw influx values were rescaled to a 0-1 range to reduce the influence of varying magnitudes between sites:"

P10.L260. I think the definition of $C_i$ is incorrect if RoCs means "Rate of Change." Based on the section title and context, $C_i$ should be the charcoal influx (CHAR) value for the i-th sample. This must be corrected to avoid major confusion.

P10.L266. The definition of α is confusing because the following text "used to ensure that both $C_i'$ and $\lambda$ are zero" is mathematically incorrect. α (a small constant) is added to ensure all values are positive (> 0) before applying the Box-Cox transformation, as the power/log functions cannot handle zero values.

P10.L272. The justification for the 200-year interpolation window is clear and logical given the sample resolution, but the authors should clarify whether the "averaging" was performed using a specific method (e.g., a "composite curve" or "binning").

**Results and Discussions**

P11.L279-280. The authors should round precise charcoal values (e.g., 2643.46 and 509.99) to the nearest whole number to account for sampling uncertainty.

P11.L286-289. In Section 4.1, the authors cite p-values, but you can also report the Deviance Explained (%) or $R^2$ for the full model in the text to give readers a sense of the "strength" of vegetation control relative to climate.

P11.288-289. The authors use the GAM results effectively to support their claims. However, be careful with the phrase "significantly positively correlated with decreasing Larix." Statistically, if the variable is "decreasing," a positive correlation with that decrease should be a negative correlation with the abundance.

Regarding the reconstruction of fire history and vegetation at Achit Nuur in Region A, Section 4.1 provides a logical transition from charcoal data to GAM modeling, though the link between temporal fire peaks and taxa-specific trends warrants further clarification. Specifically, I recommend explicitly aligning the chronology of the late-Holocene fire increase with shifts toward flammable species, such as *Betula* and *P. sibirica*, to strengthen the mechanistic argument. The discussion could be enhanced by exploring whether *Larix* functions as a fire-avoidant species or contributes to surface fuel beds, and by highlighting that the positive link between charcoal and forest cover suggests a fuel-limited system rather than one solely influenced by climate.

There is some repetition between the subsections (4.2) and the synthesis (4.3). While some overlap is necessary for clarity, ensure that 4.3 focuses on the Big Picture (e.g., the shifting

influence of the Westerlies or the broad-scale migration of pastoralists) rather than re-stating individual lake results.

In Section 4.2.1, because the authors highlight a “pronounced doubling” of fire over the last 2,000 years, they should discuss whether this is purely climate-driven or marks the onset of anthropogenic burning (pastoralism). It may be worth adding a sentence in the discussion section to assess whether climate models for this period (e.g., the Medieval Climate Anomaly) support this surge.

To improve the study’s understanding of causal mechanisms and structure, the authors could categorize sites by their specific fuel strategies, particularly distinguishing between high-intensity, high-frequency forest-steppes (Achit Nuur and Tolbo) and litter-driven lowland environments (Alahake and Kuchuk). This categorization would better clarify the relationship between vegetation and fire. Furthermore, the discussion of charcoal doubling at Kuchuk Lake should be more explicitly linked to concurrent vegetation shifts, such as the expansion of Betula, within the same section to provide a clearer ecological context.

P12.L331. “are highly …” to “is highly …”

To enhance the manuscript's broader context in Section 4.2.2, I suggest that the authors emphasize the Younger Dryas/Early Holocene transition as the trigger for the charcoal pulse at 12–11 cal. kyr BP. This will help to better connect the findings with the journal's focus on climate studies.

Regarding ecological details in Section 4.2.3, emphasizing the contrast between fire-prone taiga (e.g., *Betula*) and fire-resistant taiga (*Abies*/*Pinus*) would provide valuable mechanistic insight. Technical refinements are also recommended, including the adoption of standard paleoecological terminology, such as 'pyrophytic' rather than 'fire-loving’.

In Section 4.2.4, the authors mention sharp rises at ~1.2 ka and ~0.5 ka BP but should briefly note whether these correlate with known climatic or cultural events, such as the Medieval Climate Anomaly or the expansion of nomadic empires (e.g., the Mongol Empire).

P16.L436. “This mechanism” to “This paraglacial mechanism”; The authors could use "paraglacial" to describe the period of landscape stabilization and the sediment/charcoal flush following deglaciation.

P17.L462-462. “Without glacial to acuumulate” to “Without glaciers to accumulate”

The explanation for the ~12–11 ka peak at Buibinskoye (transient warming drying sparse *Picea* litter) in Section 4.2.6 is plausible, though the authors should ensure the chronology is tight enough to distinguish just prior to forestation from the forestation itself.

To help the reader navigate the dense regional data in Section 4.3, the authors might consider organizing the summary section by main driving factors rather than by region alone:

- Climate-Driven Fuel Limitations (Early-to-Mid Holocene): In the early Holocene, fire regimes were primarily natural. In Region A, aridity limited biomass, thereby suppressing fire. In Regions E and F, fire tracked temperature-driven fluctuations in forest cover and productivity.
- The "Anthropogenic Override" (Late Holocene, <2.0 ka BP): Across most regions (A, B, C, D), a rapid rise in charcoal indicates that human activities began to override climate controls. Pastoral burning and metallurgy provided ignition sources that increased fire frequency beyond natural variability.

- The Grazing Fragmentation Effect: Region G is a unique contrast. Although humid conditions should have increased fuel, intensive grazing fragmented surface vegetation, removing the continuity of fuels required for fire spread.

P23.L663. “We presents” to “We present” or “This study presents”

**Conclusions**

In Section 5, the authors mention Region D in two different contexts (low influx vs. gradual decline). Please ensure the distinction between the drivers (productivity vs. temperature) is sharp.