

From: zhdl@ms.xjb.ac.cn

Authors replies to the review comments

Climate of the Past (EGUSPHERE-2025-1991R1)

Dear Prof. Natalia Piotrowska :

We are very grateful to you and the reviewer for her/his comments which helped us to improve the revised manuscript.

In this revised manuscript, we have made revisions according to your valuable suggestions. Thank you very much for your attention and consideration again.

Sincerely yours

Dr. Dongliang Zhang

Jan. 16. 2026

Replies to Reviewer 1 (thank you ! From Dongliang Zhang):

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

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.

Reply: Thank you very much for your positive suggestion.

Specific comments

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

Reply: Done.

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.”

Reply: Done. We modified this part.

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”

Reply: Done. We changed the sentence: This ecological transformation triggers critical climate feedback mechanisms through [carbon pool transformation, cascading ecological and permafrost degradation \(Ivanova et al., 2019; Jones et al., 2020\)](#).

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.

Reply: We deleted this paragraph.

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.

Reply: Done.

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.

Reply: Done.

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.

Reply: Thank you. We changed it to “using high-resolution microscopic charcoal analysis from Achit Nuur”.

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.

Reply: Thank you for your question. We here discussed the drivers of modern climate in the studied region, thus we don't want to add these systems change during the Holocene Thermal Maximum or the Little Ice Age.

P6.L153. “... margins is” to “... margins are”

Reply: Done.

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).

Reply: We added the sentence: It should be pointed out that we analyses the charcoal data in this study and the pollen and lithology were previously published (Sun et al., 2013).

P7.L163. “mean size” to “mean grain size” (?)

Reply: Done.

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.

Reply: Thank you. We added the sentence: A 2100-year reservoir correction was applied to all radiocarbon ages prior to calibration due to old carbon-influenced 2099 ¹⁴C BP on the surface sediment **and this correction is assumed to be constant throughout the sequence (Sun et al., 2013).**

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 ...”)

Reply: Thank you for the suggestion. We have revised this sentence to better link the temporal scope with the research goal mentioned in Section 2.3.

This study just focused on the Holocene interval (i.e., the past ~11,750 cal. yr BP) **to investigate the spatial heterogeneities of fire regimes in the Altai-Sayan Mountains and adjacent plains.**

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

Reply: Done.

P7.L174. “spatial heterogeneities of” to “spatial heterogeneity in”

Reply: Done.

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

Reply: Done.

P7.L180. “the Irtysh river valley” to “the Irtysh River valley”

Reply: Done.

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

Reply: Done.

P7.L189. “of West Siberian plain” to “of the West Siberian plain”

Reply: Done.

P7.L190. “Ulukh-Chayakh Mire (...) located” to “Ulukh-Chayakh Mire (...) is located”

Reply: Done.

P8.L197- 198. “on western piedmont” to “on the western piedmont”

Reply: Done.

P8.L208. “(Region D, ...)” to “(Region E, ...)”

Reply: Done.

P8.L209. “2150 m” to “2150 m a.s.l.”

Reply: Done.

P8.L210. “in the central Altai part of Russian Altai” to “in the central part of Russian Altai”

Reply: Done.

P8.L218. “Lugovoe mire” to “Lugovoe Mire”

Reply: Done.

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”

Reply: Done.

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., $>50\ \mu\text{m}$, $50\text{-}125\ \mu\text{m}$) or mention that these are "pollen-slide charcoal" counts to distinguish them from macroscopic charcoal (which is usually sieved).

Reply: Thank you for this critical technical point. We have clarified in the revised manuscript that we analyzed microscopic charcoal from pollen slides.

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., $>50\ \mu\text{m}$, $50\text{-}125\ \mu\text{m}$), as this is critical for interpreting fire signals.

Reply: We appreciate these suggestions. We have corrected the grammar as requested. Furthermore, we have added the necessary details to the methodology section: we used a magnification of $400\times$ for identification and counting. We also clarified that we

employed a standard particle count method where individual charcoal particles were tallied. We are sorry that we can not answer the final question because we just counted and did not measure the size of charcoal.

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

Reply: Done.

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

Reply: Done.

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

Reply: Done.

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

Reply: Done.

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

Reply: Done.

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

Reply: Done.

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

Reply: Done.

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?

Reply: We thank the reviewer for pointing out the phrasing issue and the importance of chronological control for CHAR calculations. We have corrected the text to state that CHAR was calculated by multiplying the charcoal concentration by the sedimentation rate (cm/yr). Regarding chronological uncertainty, our CHAR calculation is based on the median sedimentation rates derived from the age-depth model established using R software. While we did not propagate the age-model error into the influx calculation directly, the robustness of our chronology is supported by sufficient dating control points in the Holocene interval, which ensures the reliability of the CHAR trends.

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()`.

Reply: We have added details on model diagnostics to the methodology. Specifically, we assessed model convergence and adequacy using the `gam.check()` function in R. We confirmed that the basis dimensions (k) were sufficient and inspected diagnostic plots of residuals to ensure the model structure was appropriate.

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?).

Reply: We selected these six taxa because they represent the dominant arboreal species in the study area and are key components of regional forest ecosystems.

In this study, we investigated the associations between charcoal influx and two types of predictors: (1) individual taxa, including *Abies*, *Betula*, *Larix*, *Picea*, *P. sibirica* and *P. sylvestris*, because they represent the dominant arboreal species in the study area and are key components of the regional forest ecosystems;

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.

Reply: We appreciate this suggestion regarding time-series properties. To address this, we examined the autocorrelation function (ACF) of the model residuals. The diagnostic plots indicated no significant temporal autocorrelation remained after fitting the GAM structure, supporting the validity of our approach without adding a specific correlation structure.

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.”

Reply: Thank you for your question. We agree that the original phrasing was slightly overstated. We have adopted your suggestion and revised the sentence to read: ‘...corrects for overdispersion without requiring a specific parametric distribution for the data’.

P10.L256. “three-step process” to “a three-step process”

Reply: Done.

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 ...”

Reply: We agree with the reviewer’s suggestion regarding terminology. We have revised the opening sentence to be more precise: ‘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” (?)

Reply: Corrected.

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:”

Reply: Thank you. We have added the descriptive text before the equation as suggested to improve readability.

(1) Min-max transformation: Raw influx values were rescaled to a 0-1 range to reduce the influence of varying magnitudes between sites.

(2) Box-Cox transformation for homogenization of variance: This transformation was applied to homogenize within-record variance and improve the normality of the data distribution, satisfying the assumptions for subsequent statistical analyses.

(3) Z-score calculation: The data were converted into Z-scores (standardized anomalies with a mean of 0 and unit variance) to facilitate direct comparison and the synthesis of charcoal records across different 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.

Reply: Yes, you are right. We modified them.

P10.L266. The definition of α is confusing because the following text “used to ensure that both! and λ 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.

Reply: We verify that the original explanation was confusing. We have rewritten this sentence to correctly state that α is a small constant added to ensure all data values are positive (>0) prior to the Box-Cox transformation, as the function 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").

Reply: We have clarified in the text that the averaging was performed using a binning method to generate the composite curve over 200-year intervals. We rewrittern the sentence: Considering the ~200-year sample resolution at most sites, the transformed Z-scores were linearly interpolated to 200-year time steps. Subsequently, the interpolated data were averaged using a binning method to construct composite curves that characterize fire regimes across different regions.

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.

Reply: We agree with the reviewer’s point regarding significant figures and sampling uncertainty. We have rounded all reported charcoal values to the nearest whole

numbers throughout the manuscript.

P11.L286-289. In Section 4.1, the authors cite p-values, but you can also report the Deviance Explained (%) or R2 for the full model in the text to give readers a sense of the “strength” of vegetation control relative to climate.

Reply: Thank you for this suggestion. To better illustrate the predictive strength of our model, we have added the Deviance Explained to the revised text in Section 4.1.

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.

Reply: We appreciate the correction regarding the statistical phrasing. We have revised the sentence to clearly state that charcoal influx was negatively correlated with *Larix* abundance, avoiding the confusion caused by mixing correlation direction with the trend of change.

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.

Reply: We have strengthened the discussion in Section 4.1 to explicitly link the timing of fire peaks with specific vegetation changes. We added text clarifying how the temporal rise in charcoal influx coincides with the observed shifts in key taxa.

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.

Reply: Thank you. We have revised the text to explicitly align the late-Holocene fire increase with the expansion of flammable taxa (*Betula* and *P. sibirica*). We expanded the discussion on the ecological role of *Larix*, exploring its potential function in fuel accumulation versus fire avoidance. We explicitly highlighted that the positive correlation between charcoal and forest cover points to a fuel-limited system, providing a stronger ecological interpretation beyond climatic drivers alone.

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.

Reply: Thank you. We have streamlined Section 4.3 to reduce redundancy. The revised section now focuses on synthesizing the regional ‘Big Picture,’ specifically highlighting broad-scale drivers such as the shifting influence of the Westerlies and major pastoralist migrations, rather than reiterating individual site 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.

Reply: Thank you for your question. We added the related descriptions in the part 4.3 and attributed this pronounced doubling to anthropogenic disturbances under the wetting climate.

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 Nur 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.

Reply: Thank you for your question. We have revised this part. However, as taxonomic identification of the charcoal was not conducted for individual sequences, we did not engage in a detailed discussion regarding the fuel sources for fires in the different sequences.

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

Reply: Done.

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.

Reply: Thank you. We have revised Section 4.2.2 to explicitly identify the Younger Dryas/Early Holocene transition as the climatic trigger for the charcoal pulse at 12 – 11 cal kyr BP, aligning our findings with broad-scale climatic events.

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' .

Reply: Thank you. We have refined the ecological discussion in Section 4.2.3 to contrast fire-prone vs. fire-resistant taiga taxa. We also replaced informal terms like ‘fire-loving’ with standard terminology such as ‘pyrophytic’ or ‘fire-adapted’.

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).

Reply: Thank you for your question. We checked and found no direct cultural events around the Uzunkol Lake and Dzhangyskol Lake. We will continue to check the other references written in Russia. Thank you again.

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.

Reply: Thank you. Corrected. We use the term ‘paraglacial mechanism’ to describe these post-glacial processes.

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

Reply: Thank you. We corrected it. Regarding the Buibinskoye peak, we have reviewed the chronology. We clarified in the text that while the chronology supports the ‘drying of sparse litter’ hypothesis, we interpret the timing carefully considering the age-model uncertainties during the transition period.

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.

Reply: Thank you. We rewritten this part in the revised manuscript.

Broadly, Holocene fire regimes in the Altai-Sayan Mountains and adjacent plains reflect a shift from climate-limited systems to human-modified systems. Before ~2 cal kyr BP, fire activity was largely regulated by moisture (limiting fuel in steppe regions A & G) or temperature (limiting fuel in alpine/forest regions E & F). Since ~2 cal kyr BP, a divergence occurs: in regions with expanding agriculture/pastoralism (A, B, C, D), anthropogenic ignition sources amplified fire activity beyond natural baselines. Conversely, in heavily grazed areas (Region G), pastoral pressure fragmented fuels, suppressing fires. This synthesis highlights the fundamental transition of the Altai-Sayan fire regime from biophysical control to anthropogenic dominance in the late Holocene.

P23.L663. "We presents" to "We present" or "This study presents"

Reply: Done.

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.

Reply: Thank you. We rewritten this part in the revised manuscript.

This study presents a long-term fire record from western Mongolia and systematically evaluates the spatiotemporal variations in charcoal influx and its coupling with vegetation across the Altai-Sayan Mountains and adjacent plains. Our

synthesis reveals distinct regional drivers of fire regimes rooted in climate-fuel feedbacks and, more recently, anthropogenic forcing:

Prior to ~2000 cal. yr BP: Fire activity was primarily regulated by biophysical constraints on fuel availability. In the steppe zone (Region A), low charcoal influx was driven by aridity, which limited vegetation productivity and fuel continuity. In the Central Altai forests (Regions D and E) and Western Sayan (Region F), fire trends generally followed temperature-regulated forest dynamics. Specifically, the early-to-mid Holocene decline in charcoal influx (Regions D, E, and F) reflected shifts in forest composition and cover. Notably, in Region F, this decline was mechanistically linked to the expansion of fire-resistant *P. sylvestris*, which reduced ecosystem flammability by displacing more combustible taxa.

A synchronized surge since ~2000 cal. yr BP in charcoal influx occurred across Regions A, B, C and D. This widespread intensification was driven by the synergistic effects of regional climatic changes and intensified human activities (e.g., agricultural expansion and pastoral burning), which overrode natural fuel limitations. Conversely, Region G exhibited a marked decline in charcoal influx despite favorable climatic conditions. This anomaly is attributed to landscape fragmentation caused by intensive grazing, where livestock pressure reduced surface fuels and suppressed fire spread.

Our findings underscore that fire regimes in the Altai-Sayan ecoregion are determined not just by climate, but by the specific flammability traits of dominant vegetation (e.g., pyrophytic *Betula/Larix* vs. fire-resistant *P. sylvestris*) and land-use history. Understanding these long-term fire-vegetation-human interactions provides critical baselines for predicting future wildfire risks and implementing sustainable forest management strategies in a warming world.