

**Response to reviews for Manuscript No. egusphere-2024-1707**  
***Reconstruction of Holocene and Last Interglacial vegetation dynamics and wildfire***  
***activity in southern Siberia,***  
**by Margerum and colleagues, submitted for publication in Climate of the Past.**

Dear Editor,

We kindly thank you and the reviewers for your support, helpful comments, and generous remarks relating to our paper.

We have modified the manuscript according to the list of suggestions provided by all three reviewers and included here the list of recommendations and our detailed responses to each of these. We have given our responses in blue.

We also provide a list of additional minor changes that we believe improve the clarity of our manuscript.

We hope the following improvements satisfy your expectations, and hope that the revised version is satisfactory for publication in your journal.

Yours sincerely,

Jade Margerum (on behalf of all co-authors)  
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## Reviewer #1: Comments & Responses

Major Comments:

1) Interpretation of Lev differences in absolute values between LIG and Holocene. In general, I am okay with how this has been interpreted as it is backed up by LOP. That said I would prefer if before diving into the climate interpretation the authors discuss the fact that there is a growth hiatus between the two intervals in the sample and what this might mean for Lev transport and incorporation into the speleothem archive? Specifically, presumably the above cave freezing must change flow paths changing Lev transport through the karst, is this something we should be worried about? At a minimum I think some discussion of this is warranted at the beginning of the discussion. I also think that doing so helps justify that normalized Lev record (Figure 5 D) that is discussed in section 4.2. That said, the only way to verify the trends would be to replicate the datasets in other stalagmites from the same cave (which is out of scope for now).

**Response:** We agree that the incorporation of Levoglucosan and any associated limitations have not been discussed and that this information would be beneficial. We added a section wherein we discuss Lev transport in karst environments and the relevance of speleothem minerology. We also expanded on the hiatus between the two intervals (Holocene and LIG). Please see between lines 255 – 270.

2) Comparison between Holocene and LIG and the role of seasonality. Section 4.3 is well written and reading it over a few times one might possibly get the gist. However, given that this is part of the important take home of the paper, that the seasonal differences in

hydroclimate led to differences in vegetation and fire, a conceptual schematic diagram might be helpful. That authors might even consider that this could enhance their discussion at the end of section 4.3 (last paragraph) when referring to what to expect in the future.

Response: We agree that a schematic in this section would be beneficial and have made one to complement section 4.3. Please see line 470.

3) Data tables and d18O data. A few comments here:

1. a) The data tables in the “Asset\_1” excel document need significant cleaning up for useability. I wanted to plot some of the data myself but found this quite difficult.

Response: We checked the data tables and re-organised and carefully labelled the data to allow easier access and usage.

2. b) Specifically denoting what is raw data and what is interpolated between the aragonite and calcite scales is important.

Response: Aragonite and calcite scales are not interpolated (rather interpreted in areas with mixed mineralogy) – all information is derived from the Raman mapping, and aragonite/calcite mineralogy needed for corrections are from the specific points milled for stable isotopes. The final isotope results (corrected for mineralogy) are used throughout the paper.

3. c) The authors mention the d18O dataset but do not plot it or interpret it. It seems to me that this is not best practice in the community. While the data are likely very complex due to the mineralogical changes, some of the interpretation of the d13C (and the organic proxies) would likely be enhanced by looking at the d18O data in detail. More on this below in my minor comments. The authors might also consider looking at the ( $^{234}\text{U}/^{238}\text{U}$ ) initial values from the two time periods.

Response: It is correct that the  $\delta^{13}\text{C}$  and  $\delta^{18}\text{O}$  results were collected simultaneously during isotope ratio mass spectrometry. Although we have these results, we abstain from publishing them at this time because we are still investigating several potential influencing processes that contribute to the final  $\delta^{18}\text{O}$  signal in the stalagmite in this cave. We plan to provide the results in a separate manuscript and deem that it provides little additional information for interpreting the  $\delta^{13}\text{C}$  signal. We use independently analysed LOP results to help disentangle the influencing factors that govern the  $\delta^{13}\text{C}$  signature in our stalagmite. We feel the presented interpretation is robust without the need to include  $\delta^{18}\text{O}$  and  $^{234}\text{U}/^{238}\text{U}$ . We removed reference to the  $\delta^{18}\text{O}$  dataset.

- d) Dates were produced in Vaks et al. 2013/2020 (based on section 2.2) – the “dates input” tab is very minimal and does not include assumptions regarding Th corrections, half lives etc. While I realize this is already previously published, those studies included many samples, it would be helpful for understanding the fidelity of this work to include those measurements and details here. Related to this, looking at the data tables in both Vaks et al. papers in comparison to “dates input” I do not see an age of -58 yrs BP, was this just assumed for the age model? My impression was this was a broken stalagmite. Or, based on Figure 2 was this age not used?

Response: The U-series dating table includes dates derived from the U/Th dating taken from Vaks et al. 2013/2020 and simplified for input into the COPRA age modelling code. This was only an input, and if the same information is input into COPRA similar results should be produced. The age -58 ka BP was a dummy age used to bracket the topmost part of the stalagmite. All the relevant Vaks et al. (2013, 2020) datasets have been copied into the assets table.

#### Minor/Specific Line-by-Line Comments:

26-27 and 34: I suggest you specify that this is the last interglacial maximum, MIS 5 is much longer itself. Where did 124.1 to 118.8 ka come from? This does not appear to be the MIS5e interval. Note on line 185 you use 115 to 130 ka.

Response: We rephrased the sentence in the abstract to be clearer when specifying the time intervals. Both other instances on lines 34 and 185 mention that our record covers part of the last interglacial only. Please see line 28.

29: Please specify if this is the organic carbon  $\delta^{13}\text{C}$  or the carbonate  $\delta^{13}\text{C}$

Response: We agree and have added carbonate to make this clearer. Please see line 30.

30-31: Sentence is not very informative, please revise.

Response: We agree and have reworded the sentence to emphasise the controls of the proxies. Please see line 32.

39: due to? Insolation configuration?

Response: We have added the reasoning in the revised manuscript. Please see line 39.

43-46: I would split this into two sentences, very disjointed opening sentence.

Response: We split the sentence into two as suggested, the second sentence starts “these changes collectively affect...”. Please see lines 43 – 46.

53: remove “competing” as the sentence structure is not amenable to such a statement

Response: Thank you, we have removed this word. Please see line 57 (This section was moved from line 50-55 in the prior manuscript to lines 54 – 58 in the new manuscript).

62-63: Can you be specific about what the mechanism was that linked wildfire activity to climate change?

Response: We added more details: “Elevated summer temperatures” to be more specific. Please see line 61 – 62.

83: expand to “as a compound embedded in secondary cave carbonates” or similar ◇  
relatedly do we know how it is incorporated in the carbonates?

Response: We agree and have expanded as suggested. Please see line 83.

104-107: What is the soil overlying the cave and karst? Is it seasonal permafrost? I see the Figure on the next page, need to link to this explicitly.

Response: The study site is characterised by isolated patches of permafrost. The soil and subsoil remain frozen in the later autumn, winter, and spring seasons, with a thaw period between June and mid-September. The epikarst above Botovskaya cave seems to remain at least partly unfrozen because we observe active drips even in February when surface air temperatures reach minima well below  $-40^{\circ}\text{C}$ . The soils above Botovskaya cave show strong similarities to the burozems described by Golubtsov et al. (2023) and the Umbric

Albeluvisols/ Podzoluvisols/ Luvisols defined by Granina et al. (2019). The soils are between 15 and 45 cm deep and can be defined as grey-brown residual carbonate soils with added humus (Kozlova et al. 2021). We added more information of the soil and permafrost to the manuscript. Please see lines 109 – 113.

140-142: This is the first time that the previous work is mentioned. It would have been helpful to summarize the findings (and analyses) from those samples in the introduction briefly or do that here.

Response: We agree and have added more information on previous studies. Please see lines 144 – 147.

186-187: See above comment on the timing of LIG, I think in the abstract and intro you should use the full time period but yes, in results/discussion the interval you have measurements from.

Response: We agree that the timing of the LIG needed clarification in the abstract, both other instances do specify that they are only periods within the LIG. After 187 we specify that for simplicity, the LIG will be used for the period of 118.8-124.1 ka BP. Please see lines 191 -194.

Figure 2: Can the Raman maps be blown up and stacked together in a cleaner way? Perhaps in a supplemental figure. As the figure is presented now it is very hard to see the details of the Raman maps.

Response: We agree that for the reader these images are quite small. We have produced a large figure in the appendix with the scans stacked and details of mineralogy much easier to see. Please see Appendix C; Fig C – also included in Assets\_1.

200: 0.0002 years? Is this correct? Are you propagating just the drilling resolution and not the age uncertainty here?

Response: We calculated the age resolution of each Levoglucosan sample by using the modelled age and the depth (from top) datasets to see what timespans each sample incorporated. For the Holocene which had multiple U/Th dates and varying sizes of milled sample (despite similar weights), there was greater age variability between each sample. For the LIG, we have only two ages and all milled samples were approximately the same size, resulting in a very low standard deviation. The 0.0002 should be in ka. We added the calculation and dataset for the age resolution and sampling measurements to the asset table. Each LIG sample encompasses almost a century, but the temporal difference from one sample to the next is very small because we milled large but continuous aliquots. We have also reworded the manuscript. Please see line 210.

229-230: I still think it should be plotted. Additionally, it may be useful for interpreting the  $\delta^{13}\text{C}$  record's variations. For example, is there much covariation between  $\delta^{18}\text{O}$  and  $\delta^{13}\text{C}$  in this sample related to in-case or prior calcite precipitation processes?

Response: Please see our comment above. We abstain from showing the  $\delta^{18}\text{O}$  because we are planning to present the data and our interpretation in a separate study. The two isotope ratios are not significantly correlated as one would expect if prior calcite and/or aragonite processes (PCP/PAP) were involved. This suggests that other forcings are at play which are beyond the scope of the present study. We decided to remove all  $\delta^{18}\text{O}$  data and reworded the title of this section to 3.5 Carbon stable isotopes (line 237).

350: See major comment above about Lev incorporation differences between LIG and Holocene.

Response: We agreed with the major comment and included a paragraph in the discussion. Please see lines 255 – 270.

361: Given that this is the only fire record that can be compared to I was expecting it's inclusion into a stack plot. Figure 5 is already pretty busy but perhaps another figure could be added with the Lev record, S/V and C/V vs. some of the datasets from Barhoumi et al. (2021)?

Response: We agree and will add this data to Fig 5. Please see line 355.

371-374: Is there any evidence in the archaeological record for controlled management?

Response: We have found no evidence on forest management or related wildfires. We have added a sentence summarising this in the discussion. Please see lines 407 – 409.

388-390: Sentence grammar needs to be fixed.

Response: We changed this sentence to make it clearer. Please see lines 434 – 435.

409: reference needed for Scandinavian glaciation

Response: We added a reference. Please see line 456.

## Reviewer 2#: Comments and Responses

### General comments:

Margerum et al. present a novel and timely reconstruction of wildfire activity and environmental conditions in southern Siberia for the Holocene and the last interglacial, based on proxy analyses of a speleothem archive. They find differences in wildfire activity between the two periods and discuss potential drivers, including mostly climate and vegetation, but also human activity. The study is a highly welcome contribution to the understanding of long-term wildfire activity in a region that, despite its ecological significance and recent extreme fire seasons, remains under-represented in the global distribution of paleo-fire studies. That is especially true considering the use of a stalagmite archive in combination with biomarkers as fire and vegetation proxies, a combination which adds to the diversity of approaches regarding scarce paleo-fire studies in Siberia. The manuscript is well written, features concise and very well-constructed figures, presents relevant findings, discussed in the context of previous research, and fits well to the scope of Climate of the Past. I recommend the acceptance of this manuscript after minor revisions, mainly related to a clarification of the implications of the hiatus in the speleothem archive, minor potential additions to the discussion section, and corrections/clarifications on some smaller, more technical aspects.

### Specific comments:

The growth hiatus of the studied stalagmite, separating the last interglacial from the Holocene, is described well, and a convincing explanation for it is presented with the presence of permafrost above the cave. However, the difference of Levoglucosan (Lev) concentration levels between the two periods made me wonder whether such an outcome

could be caused by systematic or taphonomic differences behind Lev enrichment within the stalagmite rather than such a fundamentally different level of wildfire activity. Other proxies not showing such a clear difference between the two periods may be an argument for the interpretation of Lev as actual fire signal. However, both climatic and environmental differences between the last interglacial and the Holocene are inferred, as well as differences in the aragonite and calcite sections and a higher growth rate in the last interglacial, and I wonder if these differences may have also affected Lev enrichment? The inclusion of normalized Lev in Fig. 5D is very welcome in that regard. While reading the manuscript, I felt that a brief discussion of the comparability of pre- and post-hiatus proxy signals would be highly helpful to judge the more in-depth following discussion of all the findings.

Response: This comment is similar to another major comment made by Reviewer 1#. We agree that more information relating to Levoglucosan in the speleothem matrix should be discussed and have added more detail in the beginning of the discussion (4.1). Please see lines 255 – 270.

When discussing the findings of past vegetation composition (e.g., forests more established and denser in the second half of the Holocene, changing prevalence of hard- and softwood species over much of the Holocene; L340f.), I would have expected to see some more comparisons to previous studies on past vegetation. Studies from the Baikal region covering the Holocene are already referenced in the manuscript (e.g., Demske et al., 2005; Tarasov et al., 2009; Barhoumi et al., 2021 and references therein) and discussed briefly in the final paragraph of section 4.1 (which is very well reasoned), however, a closer comparison of vegetation-related results to these previous studies seems to be lacking. I think an inclusion of such a brief comparison (for example in discussion section 4.2) could help highlight your new findings in contrast to previous studies!

I'd also recommend to include in the discussion section a brief evaluation of Lev as indicator of mostly low-intensity, smoldering fires (Dietze et al., 2020 and references therein) and how that may affect the interpretation of reconstructed wildfire activity.

It may furthermore be great to include a few more references throughout the discussion, where and if possible (for example in L348, with thunderstorms suggested as main source of rainfall, or where similar statements are made).

Response: We agree that more comparison to previous studies on vegetation could be made in this section, and that describing the already cited studies in more depth would be beneficial. We also agree that the description of Lev as an indicator can be further explained here, specifically that identification of signals will more than likely only be smoldering fires as the detection of levoglucosan drops after 350 degrees Celsius (please see lines 255 – 270). We agree that more references for some of the statement sentences can be added.

#### **Technical comments:**

(Very) small remarks throughout the text: Recommend to double-check standardizing the use of Oxford comma, the referencing style of studies with two authors (“and” or “&”, e.g., L. 68), writing of “paleo” or “palaeo” (e.g., L71), the use of long-dashes for ranges (e.g., L81), writing of geographic directions (e.g., L100, L115), use of spaces before units (e.g., before “°C”, L105, L117)

Response: We agree there were some inconsistencies that we missed. All referencing styles have been homogenised, all palaeos have been changed to British English, dashes have



been corrected, geographic directions have been written in full, and spaces between units has been used.

L73: Would suggest to remove “surface” in “lake surface sediments” – this may be applicable to the two studies by Glückler et al., but Dietze et al. (2020) used samples from a 318 m long sediment core

Response: We agree and have deleted surface. Please see line 73.

L75: First reference may not be needed here?

Response: We agree and have deleted the reference. Please see lines 74 – 75.

L89: If I understood correctly, then this part of the sentence can be misleading – would it be correct to switch it around to read “[...] the S/V and C/V ratios delineate different vegetation types such as hardwoods vs. softwoods and woody vs. non-woody, respectively [...]”?

Response: We agree and have swapped the terms. Please see lines 89 – 90.

L101: Not sure if a comma is missing in the reference or if it is referencing style – if a comma is added than I recommend to double-check this also throughout the manuscript

Response: Thanks for noticing, the comma was missing, and we have added it back. Please see line 102.

L104: Also not sure if journal style, but to me it seems that “Fig. A1” could be removed as there is only one figure per appendix

Response: We have replaced all Fig. (letter) 1 with just (letter).

L120: “345 mm” seems sufficient as it is described as “annual precipitation” (the same accounts for L407)

Response: We have deleted the mm/a for both instances. We moved this sentence to the middle of the paragraph. Please see lines 119 – 120.

L144: Suggest to write the institute name fully in English (i.e., “Helmholtz Centre”)

Response: We changed the name into English. Please see line 149.

L149: Maybe rephrase, not sure if one can mill dates?

Response: We agree and have rephrased the sentence to be clearer. Please see line 154.

L156: Stalagmite ID was previously written with an underscore, maybe adapt

Response: We agree and have checked this throughout, all align now to SB\_pk7497-1.

L185: According to the age model, the age-dated range seems to include rather the intervals of 0.5 – 9.9 ka BP and 118.8 – 122.7 ka BP, with years before and after each interval being extrapolated by the age model. Since this extrapolation includes comparably few years it may be okay to use, but I’d recommend to clarify the way the age-dated vs. total covered/interpreted ranges of data are reported.

Response: We agree and changed the phrasing of this paragraph to be more specific and expand on the extrapolation of ages. Please see the additional sentence added at lines 198 – 199.

L201: Dot missing at the end of the sentence?

Response: We agree this was missing and have added the dot. Please see line 211.

L221: Suggest to move the first bracket indicating a range to after “the LIG”

Response: We agree and have moved the bracket. Please see lines 228 – 230.

L239: If I am not mistaking, it should here rather read “Low C/V ratios [...] while high C/V ratios [...]”?

Response: We agree and have changed the sentence according to the comment. Please see lines 249 – 250.

Figure 4: Very nice plot with a great separation between Holocene and last interglacial conditions! I only stumbled a little over two “Late LIG” and “Mid LIG” values plotting in seemingly opposite boxes (121.4 and 121 ka BP) – maybe re-phrase the caption accordingly, e.g. by adding something like “Regions generally characterized by [...]” to the corresponding description of the shaded boxes?

Response: Thank you! Good idea, we changed the caption. Please see line 284.

L275: Here I was wondering how a “regular” fire recurrence interval of 80–120 years was determined and how it is regular compared to which baseline. However, looking into Shvetsov et al. (2019), if considering the correct part of their study that was referred to, I think that the authors don’t mention such a fire return interval at all: “[...] the deciduous successional stage would take 80–120 years before coniferous species return, in the event of absence of repeated disturbances [...]”. I therefore suggest to modify this sentence to better reflect the content of the cited study.

Response: We agree and rephrased this part to not mislead or misinform. Please see line 300 – 302.

L276: I think this date technically lies outside the age-dated period and thus relies on extrapolation. Not sure if that is common in speleothem archives, and as mentioned before, it is only comparably few years difference, but it may be useful to briefly clarify this (e.g., in the methods)

Response: We agree this needs to be clarified. We have a good understanding of the very low and stable growth rates of the sample speleothem and there is no petrographic evidence of any drastic changes in the growth rate; therefore, we are confident that our extrapolation is reliable. We added a short section to the radiometric dating section to expand on the extrapolation.

L287: Remove additional bracket

Response: We agree and have removed the additional bracket. Please see like 313.



L304: Maybe include a reference for the statement of dry lightning being the only natural ignition source for wildfires in Siberia, or clarify it in another way (e.g., “most common” instead of “only” – other ignition sources may be a bit exotic but certainly exist)

Response: We agree that, although relatively rare, other ignition sources do exist, and changed the wording to ‘most common’. Please see line 332

L369: Great finding! Considering our other paleo-fire studies further north, this generally decreasing trend of wildfire activity over the Holocene may apply not only to the wider Baikal region, but even broader regions of eastern Siberia. Very interesting to see!

Response: Thanks, great to see.

L385f.: Not sure if it is implied here that it is usually not possible to reconstruct fire regime attributes from various archives, but if that’s the case I would like to differ – it depends of course on the archive at hand, but, from my perspective of lake sediment studies, reconstructions of fire frequency, intensity, severity, and, with some more effort, burned area, are possible (e.g., Hennebelle et al., 2020; Hudspith et al., 2015). That being said, I do appreciate the concise definition given here of fire pattern vs. fire regime, which enables a clear understanding of the reasoning for focusing on factors defined as fire pattern.

Response: Thanks for your comment, this sentence was not aimed to discredit other research or methods; rather, our point was that it is challenging to recover information from archives and that our study is limited to reconstructing fire pattern. We rephrased the final sentence to the following ‘Given the challenges in extracting these properties from speleothem archives, we have chosen to limit our reconstruction to fire pattern’. Please see lines 431 – 432.

L434: “[...] most important natural drivers of Siberian wildfires” – I suggest to rather mention “southern Siberian” or “wildfires in the Baikal region”, as findings may likely not be directly applicable to other regions of vast Siberia such as the deciduous east, or tundra regions, etc.

Response: This is an error from our side, thanks. Please see lines 487 – 489.

## Reviewer 3#: Comments and Responses

### Major comments:

1. More information or interpretation about the Lev concentration differences between LIG and Holocene would be welcome, especially when the composition of the speleothem differs between the two periods.

Response: We added a paragraph at the start of the discussion to answer how Lev has been used as a wildfire detection proxy and how concerns regarding its fidelity have been addressed. The mineralogical composition of the stalagmite and levoglucosan concentrations have shown no relation.

I would like to see more discussion about the fire interpretation, I agree Lev level cannot be interpreted as charcoal records for example, but I expect more discussion about fire detection level with this proxy. This would help readers to better understand the impact of this original study.

Response: This comment is similar to the previous comment above and we have expanded on this in our discussion. Please see lines 255 – 270.

2. Please check the data availability and explain why some values seem to differ from what is shown in the manuscript.

Response: We checked and amended the datasets from inaccuracies.

#### **Technical/minor comments:**

L43-46: Long first sentence, might be worth it to cut it in two sentences

Response: We received a similar comment from another reviewer and agreed, this has now been split into two sentences. Please see lines 43 – 46.

L45: affects -> change to affect

Response: We mean to say that the effects of global warming act upon (affects) droughts and wildfires. We corrected the spelling accordingly. Please see line 45.

L50-55: a bit early in the introduction to specify Siberia characteristics, especially when the next paragraph focuses on a larger scale, recommend moving this section further in the introduction and/or in methods.

Response: We agree and moved the information on Siberia to the next paragraph. Please see start from line 54.

L70-71: please check consistency throughout the text for “palaeo” and “paleo” and follow journal guidelines

Response: We changed all paleo to palaeo (British English) spelling.

L73: lake surface sediments is reductive, research mentioned focused on the whole Holocene period and more as you specified L74, recommend removing “surface”

Response: We removed surface – this is also in line with a comment from Reviewer 2.

L75: are available only since the early 2000 -> are available since the early 2000 only

Response: We agree and moved only. We changed the following sentence to avoid repetition of word ‘only’ to ‘sole’. Please see line 75.

L87-89: Recommend making two sentences and cutting after vanillyl (V). Can you specify which ratio represents what please? I understand it is in order of appearance, but it is worth it to add the information to clarify, recommend adding “, respectively” if that is the case.

Response: We cut the sentence into two, added respectively to the second sentence. Please see line 88 – 90.

L90: local wildfires: can you specify what range this would represent? No distance is mentioned above L86-87 “are well-preserved during transport from soil to cave” it would be nice to have an idea

Response: We added information on localisation of wildfires and lev signals. Please see lines 84 – 85.

L78-91: would combine both paragraphs as they both describe methods used

Response: Preference here – kept the same.

L96: not a fan of the word “contrasting” I recommend removing it. Contrasting is too strong and as you do not exposed hypothesis, I believe you can not be as assertive in your final sentence of introduction.

Response: We removed contrasting, as also per comment from Reviewer 1.

L99: Recommend adding study site info in the Material and Methods section unless it is a journal specificity

Response: It seems from other papers that Study Site can be its own section separate from Introduction and Methods sections. We decided to move it to the methods as this seemed most common. Thank you for the suggestion. Please see line 99 for start of section 2. Materials and Methods.

L101: missing comma in the reference

Response: We added this as per comment from Reviewer 2.

L104: Is it mandatory to specify Fig. A1? I only see one appendix A. Recommend removing it if possible.

Response: We removed the unnecessary numbering system from the appendix as suggested.

L105: what determines warmer and colder areas? If it is worth mentioning it in the manuscript, please do so.

Response: Warmer and colder areas in the cave are first and foremost determined by seasonal (temperature contrast driven) ventilation through the different entrances. Currently, five entrances are known, but it is possible that other openings through fractures and rockfall might be present that facilitate ventilation. The warmer areas are confined to cave passages near (ca. 40-50 m) an entrance, whereas the inner passages remain poorly ventilated and cold. We changed the sentence to incorporate this information. Please see lines 106 – 108.

L117-118: When were recorded local temperatures? Like the mean cave air temperature between 2010 and 2018? Please specify.

Response: We added the age bracket. Please see lines 124 – 126.

L119-120: you mentioned cave was located at 750 m a.s.l but we do not know the average elevation of the studied region, I do not visualize how higher the cave is elevated compared to the Zhigalovo meteorological station for example, can you specify please?

Response: We added elevation information to make this clearer and provide a better understanding of the landscape. Please see line 120 for Zhigalovo a.s.l., line 102 for Botovskaya a.s.l and lin 103 for general area a.s.l.

L125: Please check consistency for English spelling, British or US, for example characterized and characterised, choose one spelling for the whole manuscript.

Response: We have checked spellings throughout for consistency.

L126: Usually the first time species are cited in the manuscript, the full scientific name is specified, for example *Larix sibirica* Ledeb.

Response: We added the appropriate author botanist abbreviated citation (Ledeb + L.). Please see lines 134 – 138.

L149: dates cannot be milled, samples to date can though. I recommend reformulating the sentence.

Response: We changed the wording to be more appropriate for this sentence, as per comment from Reviewer 2.

L154: I would decapitalize Lignin

Response: We decapitalised the L in Lignin. Please see line 159.

L156: Recommend adding reference to Fig. 2 when mentioning the hiatus

Response: We added an appropriate reference. Please see line 161.

L164-165: Please provide more information about LOPs extraction and please specify if you had to combine samples too.

Response: We further explained the methods and made the wording clearer. Please see line 168 – 169.

L166-167: Please combine the last sentence to the paragraph.

Response: We agree and moved the sentence. Please see line 174.

L171: Recommend adding reference to Fig.2 for the hiatus. Carbonate powders were measured with instead of “measured on” maybe?

Response: Added from previous comment at start of methods. Changed to with. Please see line 179.

L208: remove “much” and replace with a number or ratio in average

Response: We added average values of Lev for these intervals. Please see line 216.

L210: phrasing is not clear. I do not see low values at 6.8 compared to low values of 5 and 4-2 ka BP. I would only mention 2 lower values.

Response: We agree and removed 6.8 ka BP and made the phrasing clearer. Please see line 218.

L200-210: it might be journal guidelines, but I would remove the spaces between the paragraphs.

Response: The Word template separates paragraphs with spaces, so we left this the same.

L220-221: I am not comfortable with this sentence. It should not stand in a paragraph alone. A paragraph should be 3 sentences ideally. You are saying “similar values” for the S/V ratio with variability, I see what you mean but I recommend reformulating, for example values constrained between 0.5 and 1.5 with variability. To compare both ratio, you should talk about C/V constancy I think.

Also, why not mention the decrease of this ration during LIG in this paragraph?

I suggest rearranging these paragraphs to facilitate reading. The first paragraph focuses on the ratios reading, and the second on the time period if you think it is worth keeping it. Right now, you are mixing both in two different paragraphs and I find it is not working.

Response: We joined paragraph and reformulated as suggested. The decrease of ratio in the LIG is within the 0.5-1.5 degree of variability as mentioned which is shared in the Holocene too. The first paragraph now summarises the range of S/V and C/V values, and the second details the trends from both time periods. Please see lines 228 – 230.

L223-224: there is also a decrease between 120 and 119 ka BP, so you cannot say the ratio is constant. I suggest L223: “while the C/V ratio remains constant until ca 120 ka BP, aside....”

Response: We agree and added the suggested phrasing. Please see lines 231 – 232.

L225-226: True for S/V but not for C/V

You should add more figure references.

Response: We emphasised that C/V variability is low during the Holocene and exchanged ‘relatively constant’ to ‘low variability’. We also added more figure references. Please see lines 235 – 236.

L229-230: data for O is not shown, please add it to the graph or in supplementary material but you cannot talk about it without explaining or showing it to the readers.

Response: We include the disclaimer that oxygen isotope data was measured with carbon isotopes as this is common practice with isotope ratio mass spectrometry. This information seems necessary as questions to the whereabouts of the data would arise if we weren’t to include the data at all. We have previously (other comments and responses) explained our reasoning for not including the data. Please see lines 242 – 243.

L229-231: Interpretation should be in the discussion section and not results I believe, I suggest moving it further down.

Response: Part of the disclaimer for D18O and is not discussed elsewhere – likely should stay there. Did not move.

L231-234: Please specify values when you can.

Response: We added further details and values corresponding to ages mentioned. Please see lines 240 – 242.

L233: nice phrasing

Response: Thank you!

L275: remove regular and remove brackets what is regular for this time period might not be for another time period

Response: We agree and changed the sentence as suggested. Please see lines 300 – 302.

L291: supported instead of indicated

Response: We agree and altered the wording. Please see lines 316 – 318.

L341-342: Fluctuating S/V ration

Response: We changed values to ratios. Please see line 369 – 370.

4.1

I like your interpretations and raised arguments. I'm not fully convinced by the explanation of the increased C/V around 122 ka BP but based on the available data, I agree with your suggestion.

L369: I would like to see some mention of the vegetation shift in comparison with your S/V ratio. The shift from spruce to pines around 6.5 ka BP seems to match the increased S/V ratio. Pine forest tend to be more open forest which could lead to the increase of hardwood species eventually. I would like to see more discussion about the regional vegetation observed and your record if possible.

Response: Thank you for your comments on the conviction of this part of the discussion. We agree with the comment as we show mixed forest in this part of the Holocene, and potentially it could be true that an increase of our S/V ratio may indicate a shift to more pines thus more open forests that facilitated a higher hardwood composition. We cannot confirm this, but it does align well with the pollen data so is a legitimate theory. We have added this information in the discussion. We added a sentence at lines 396 – 398.

L371-380: I do not think human influenced the early Holocene fire record as the Lev concentration seems similar to moisture variations, expect during the late Holocene.

Response: We mention the establishment of human communities as they might potentially have impacted our (youngest) record, not that they did so with certainty. The language used in this section reflects that we tentatively discuss this topic with high caution.

L382-386: I appreciate the distinction between fire regime and fire pattern, nicely done.

Response: Thank you!

L388-391: Indeed, the triptic vegetation-climate-fire is hard to decipher.

Response: We agree!

L432-433: I would have thought that absolute value of Lev would be linked to the stalagmite composition too? If so, please add a mention of it there and eventually discuss it earlier.



Response: This is just referring to the actual dataset rather than the normalised Lev values.

## Data availability

When looking at the data and quickly plotting some graphs, I find higher value for S/V ratio and different trend from C/V

Response: All data has been (re)checked and should reflect results from the study.

## References

Please check consistency between references, some avec capital letters at each word, some do not.

Please check Elias et al reference, "Amazonia" is misspelled and I believed associate editor are not supposed to be mentioned

Kharuk et al 2021 is cited twice, please remove one

L621: homo sapiens in italic

Response: We added italics to the reference mentioning homo sapiens and removed the Kharuk duplicate. All references were checked for unnecessary capitalisations. The Elias reference has since been removed.

## Figures

Figure1

In the legend, brown-orange colour is missing, as well as country borders in black and the North arrows for both inset and the map. A scale in the inset would be nice also. Zhigalovo meteorological station could be better indicated as well, and you could add a symbol in the legend.

Response: Thank you for your comments for this figure, they have been very helpful. We agree that some edits can be made to improve the quality. We added a legend entry classifying the brown background colour as Dwc climate. We also added the longitude and latitude gridlines to the insert map and made the north pole clear. Due to the large variance of distance, we did not add a scale to the insert map. We also did not indicate the location of the meteorological station at Zhigalovo as this is already indicated below Botovskaya cave (Zhigalovo), nor add North (arrow) as the gridlines indicate the direction of north. We will add the Dwc climate reference in the caption.

Figure 3:

I recommend adding vertical lines to facilitate reading of the curbs.

Response: Thank you for your comment. We checked by adding grid lines, but the figure becomes messy and we decided to leave them out.

Figure 4:

Nice graph! Interesting to see such a shift between 121.8 and 121.4.

Response: Thank you!

Figure 5:

Nice graph again! I recommend adding vertical lines to facilitate reading.

Response: Thank you for your comment. We checked by adding grid lines but the figure becomes messy and we decided to leave them out.

## Appendices

### Appendix A

I cannot see all details and elements mentioned in the legend. Could you provide a larger figure and higher quality so we can zoom in? I recommend orienting the map with the North pointing to the top as usual maps to facilitate representation. You mention a route between Old and New world regions, but I do not see it, can you highlight it please? The scale is not easily readable and is missing some graduations.

Response: We agree and made the scale and legend bigger. We added a line which separates the old and the new world areas of the cave. Changed the figure caption to 'Fig. A: Mapped areas of Botovskaya cave with locations of specimen. Old and New world regions are joined in the area crossed with a grey dashed line. The location of sample SB\_pk7497-1 is highlighted in the northeastern part of the New World. Key features are listed within the figure'.

### Appendix B:

Specify which blue line for the temperatures recorded inside, it is not as evident. If it is the light blue line, it cannot be steady for years, please specify mean temperature.

It is hard to distinguish different temperatures curves, please add some transparency or change the colors to make it easier to read.

The increase of temperatures outside the cave since 2013 is not easily spotted I find, especially with the data from 2016, it might be worth it to add a linear regression or yearly mean temperature to spot the increase.

I recommend changing the name of Y axis to "Temperature (°C)" as some temperatures are taken 55 km away from the cave.

Why are dates so unconventional with a lag of ca 1 month every year? It makes the graphs less readable, recommend changing the label if possible.

Add a reference to Fig. 1 when you mention Zhigalovo.

Response: We corrected the figure so that temperature inside is recorded as light blue line. Specified that the red line is the mean temperature. Changed dates to winter (lowest temps)

every year (Feb). Temperature increase is minor, removed text. Changed grey (daily) temperatures to yellow colour with transparency so red and blue lines can be clearly read.

## Additional changes in our revised manuscript

Abstract, Introduction, Materials and Methods, Results, Conclusions, Appendix: Most changes here were suggested by reviewers. Minor/no adjustments made by co-authors.

Discussion: We made a minor adjustment to figure 4 to remove the colour bar on the right axis which was used as a legend feature to demonstrate the ages associated with the colour coding. We found this could confuse readers into thinking that it was another Y axis. We opted to remove it and instead explain the differences of colour for the datapoints in the caption.

References: We have added missing authors to the reference list.

Supplementary materials/Assets table: We have significantly altered the assets table to include all relevant datasets for reproducibility, as well as clear direction to any readers to the relevant datasets as per each figure.