Reviewer 2: Anonymous

We would like to thank the anonymous reviewer for a professional assessment of our contribution, which has resulted in several revisions. Our responses are indicated by a **bold orange** typeface. Changes to the manuscript (with line numbers referring to the track-changed version) are in **bold blue**.

Summary & General Impressions:

Stroven et al. use ^{14}C, ^{26}Al, and ^{10}Be exposure dating in 5 bedrock samples and a proglacial lacustrine sediment record to reconstruct the Holocene behavior of the now small Riukojietna ice cap in northern Sweden. I thoroughly enjoyed reading this paper – it is well written and the figures are excellent. I was particularly impressed to see the ice thickness reconstructions and how they were employed in the forward modeling exercise to i) include nuclide production through thin ice and ii) implement a thickness threshold for subglacial erosion. Overall, I agree with their use of multiple dating approaches and the technical approach to the modeling exercise. However, I'm not yet convinced that the data support the preferred interpretation that this low-altitude ice cap survived the Holocene Thermal Maximum (albeit at times inactive) as the title and text imply. Given the data presented in the paper, I'm more convinced by the conservative interpretation (also stated in the text, although somewhat buried) that ice cap was smaller than today for most of the Holocene, which allows that the ice cap survived the HTM, but does not require it. Below, I describe a few additional considerations, exercises, and datasets that I felt were missing or incompletely described in the manuscript, as well as a short list of minor comments and technical corrections.

Thank you for this general positive appreciation of our manuscript.

Specific comments:

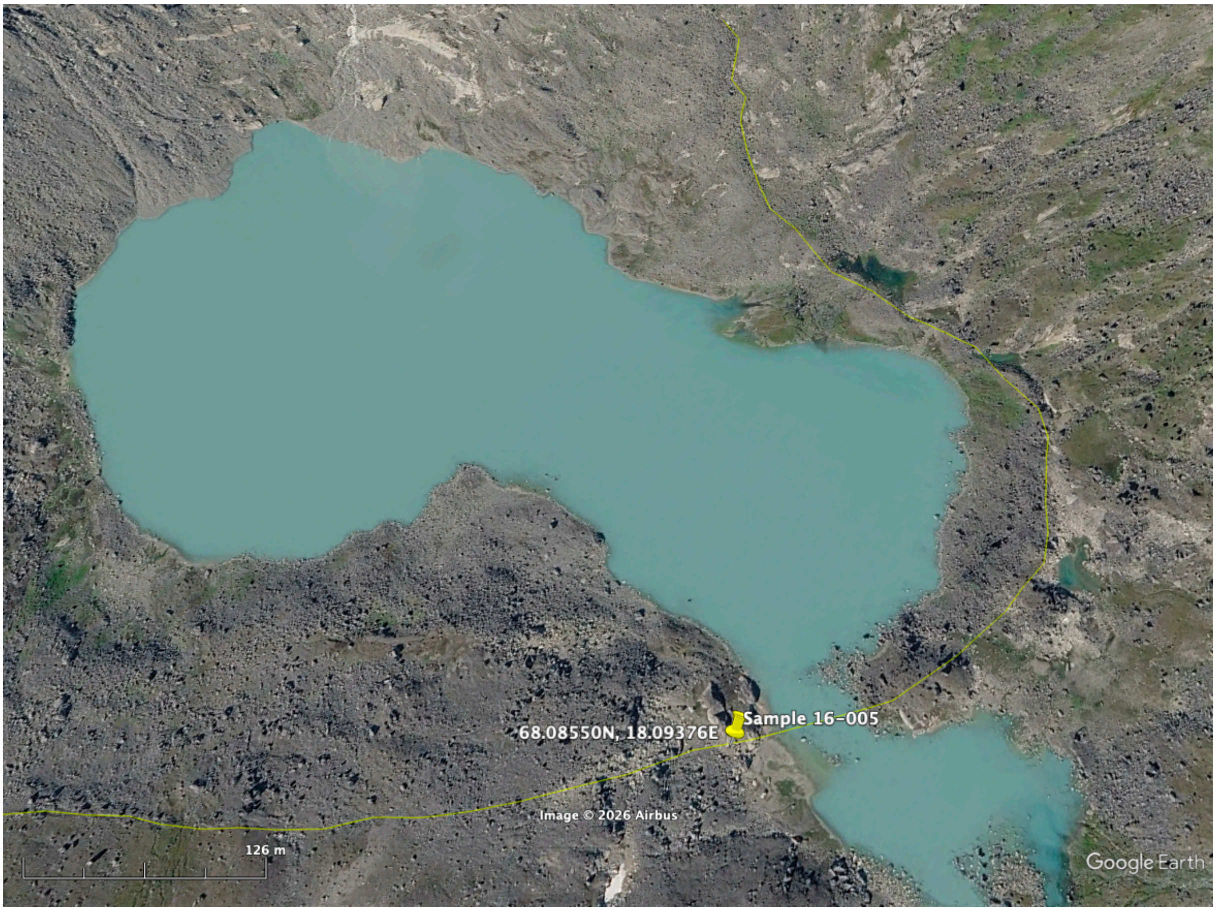
1. **Deglaciation age from lake outlet sample (16-005):** This single triple-nuclide exposure age from near the LIA moraine is a key constraint in the inferred glacial history, so it would be helpful to have a little more information:

> **Uncertainty:** It's reassuring that all three nuclides give consistent exposure ages, although the stated uncertainty of 0.1 ka is quite low. Given that these are three independent measurements, I wonder if performing standard error propagation is a more appropriate approach for evaluating the uncertainty than using the standard deviation. This would only increase the uncertainty slightly but may give a better estimation of the true error.
>
> **Thank you for the opportunity to discuss this age constraint in more depth. It was also highlighted by Reviewer 1 (Schimmelpfennig), and we reiterate a version of our response to her query here:**
>
> **Thank you for highlighting this issue, as the age is of crucial importance to our modelling history. We have now used external age uncertainties that include production rate and other uncertainties for each nuclide: ^{10}Be – 7.9 ± 0.6 ka; ^{26}Al – 8.1 ± 0.8 ka; ^{14}C – 8.2 ± 1.8 ka. Weighted mean and the larger of either the standard error in inverse-error-weighted mean, or the inverse-error-weighted average variance (Bevington, P., and Robinson, D., 2003, Data Reduction and Error Analysis for the Physical Sciences: New York, N.Y., McGraw-Hill, 320 p.) yield 8.01 ± 0.14 ka. Straight mean and std dev: 8.06 ± 0.13 ka. Although we consider using weighted values as most representative, there is no significant difference either way. The weighted approach best accounts for the various production rate uncertainties, de-weighting the ^{14}C result, but doesn't really affect the overall age**

because the individual results give basically the same central tendency, regardless of how the uncertainties are calculated. We regard that a straight up error propagation (square root of the sum in quadrature) as suggested by Reviewer 2 significantly overestimates the uncertainty (yields result of 2.1 ka, dominated by ^{14}C production rate uncertainty). We might have acted differently if the values were more spread out. In addition, following another comment by Reviewer 1, we considered the potential effect of snow cover on this sample, based on modern measurements of winter accumulation at Riukojietna since 1986. See our response to that comment for more details, but the upshot is that if we assume the modern snow cover measurements apply throughout the Holocene, then that results in a weighted mean value for 16-005 of 8.4 ± 0.2 ka (using external uncertainties) and a straight mean of 8.5 ± 0.3 ka – both within 2 σ of the values without snow shielding. Given the agreement within uncertainties for snow-corrected and uncorrected values, and that production rates typically do not include snow shielding corrections, we keep the age for 16-005 as 8.1 ± 0.1 ka.

Lines 187-189: What is the relationship of this sample to the LIA moraine? It is hard to tell from the map whether it was collected outboard of the moraine, but the photo in Figure 2d and Figure S4 suggest it was collected at or slightly inboard the moraine. If so, could the age slightly underestimate the true deglaciation age if there were a few hundred years of LIA ice cover (with or without erosion)? How much burial and/or erosion is allowed before the three nuclide concentrations are no longer concordant?

It is clearly an oversight to have not described the sampling location in more detail in relation to the LIA moraine distribution. In fact, the location of the bedrock outcrops is neither outboard or inboard, it is situated in a gap in an otherwise unbroken end moraine sequence (see light yellow curved outline in Google Earth imagery below), and would have been the location of the main subglacial meltwater outlet (a Röthlisberger tunnel, presumably). So, the sampling location would have remained free from sediment burial due to seasonal washing by the river. Hence, the cosmogenic nuclide age of the bedrock should not differ from the age of moraine formation. We note that the coordinates in Table 2 refer to longitudes as °W – which would place them solidly into the North Atlantic – and so we have modified to °E (so thank you for directing our attention to the precise location of the sample – we might never have realised this problem!). We have added this sentence to the first description of sampling site 16-005 (L. 210-213: "The last sample was collected from a bedrock outcrop adjacent to the outlet of Lake 1063 (Riuko-16-005), in the following referred to as the 'lake outlet sample'. The bedrock outcrops in line with the LIA moraine and constitutes the subglacial meltwater outlet of Riukojietna at that time."



2. **Lacustrine record & interpretation:** The preferred interpretation that Riukojietna never disappeared completely during the Holocene hinges in large part on the interpretation that glaciogenic sediment entered Pajep Luoktejaure until ~5.0 ka (e.g., Lines 545-549). Given the importance of this interpretation, I suggest some additions to help convince the reader of this important claim:

Section 3.2 (starting on Line 244): Several described datasets are not included in the paper that I suggest adding to the supplement. These include the core logs and LOI data from cores PL2 & PL4 (Line 251), which are stated to have overlapping stratigraphy with the long core, as well as the x-ray images and grain-size data described for PL1-169 and PL3-2 (Lines 254-259).

Thank you for this suggestion. We now include a supplementary figure which shows the LOI for PL2-133 in addition to PL1 and PL3. Sediment stratigraphies of PL1 and PL2 were indistinguishable (so we did not duplicate), and this was substantiated by the LOI measurements. PL4 was visually inspected and found to also replicate the stratigraphies of PL1 and PL2, and was therefore never sampled. PL5 remains unopened for future reference. X-ray imagery is of "old-school" uneven quality (now that we all use ITRAX-type core scanners) because the individual sections were imaged at variable intensities, so we see no point in publication of these. Grain-size measurements on 14 samples from suspected glacial silt/clay units returned results with 85%-99% silt/clay (<63 μm). We now show the sampling locations in Fig. S1 and the individual results of %silt/clay in Table S1.

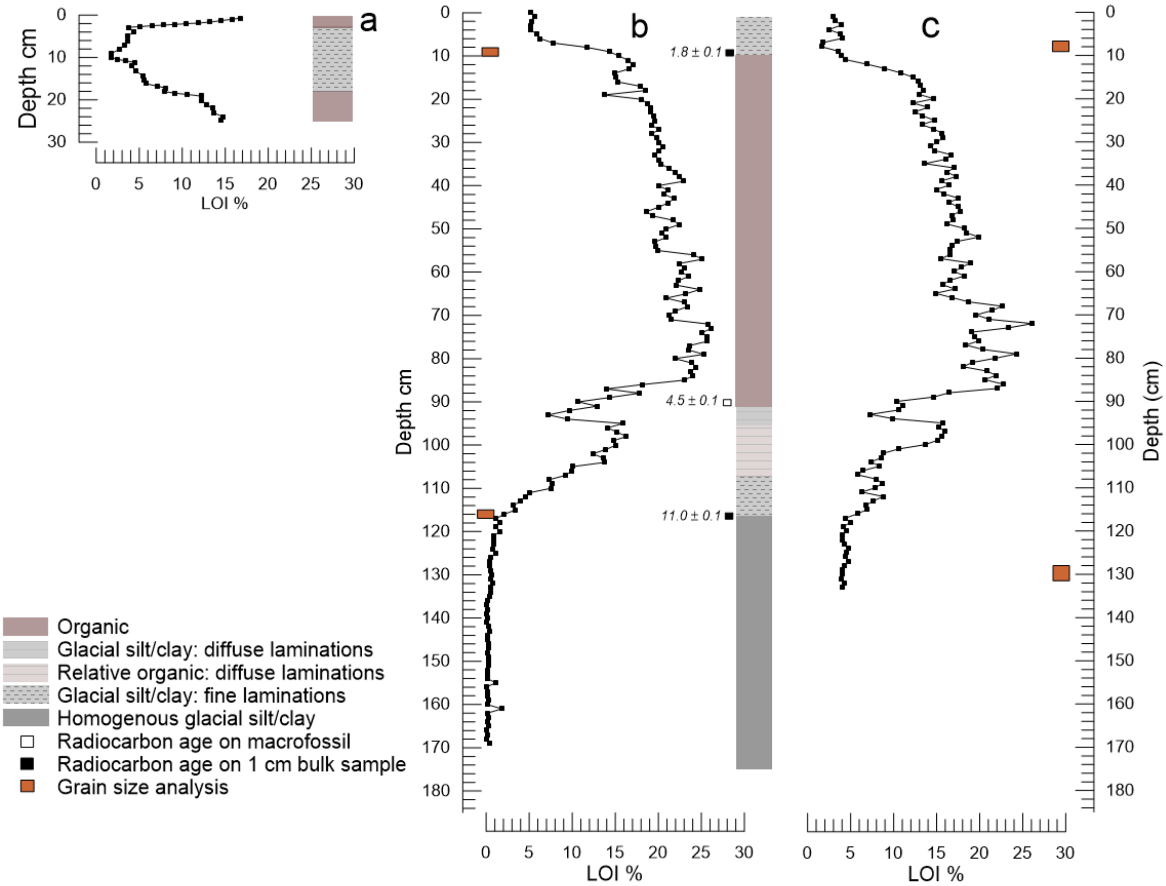


Figure S1: *Sediment stratigraphy and loss-on-ignition (LOI) for a) PL3-25, b) PL1-169, and c) PL2-133. A common sediment stratigraphy is presented for both PL1-169 and PL2-133 as they were identical in their overlapping section. Note the location of four core sections (orange boxes; a total of 14 samples) targeted for grain size analyses, and all were found to be heavily dominated by silt/clay (85-99%; Table S1).*

Table S1: Grain size analyses on 1-cm core sections of cores PL1-169 and PL2-133 (see Fig. S1).

Core	Depth (cm)	Percent <63µm	Core	Depth (cm)	Percent <63µm
PL1-169	8	97	**PL2-133**	6	99
	9	98		7	99
	10	99		8	99
	115	91		9	99
	116	89		128	92
	117	92		129	88
				130	85
				131	95

Finally, we have amended the text as follows

L. 286-289: "Grain size distribution was analyzed in a SediGraph 5100 on 1 cm slices of specific sedimentary structures (i.e., finely laminated minerogenic sections at 115-**117** cm and 8-10 cm depths in core PL1-169**, and replicated by results from finely laminated minerogenic sections at 128-131 cm and 6-9 cm depths in core PL2-133; Fig. S1, Table S1)**.**"**

L. 409-411: "Results from grain size distribution analyses show indeed that the sampled sections consist of silt and minor amounts of clay (>**85%** silt/clay; i.e. <63 µm; **Table S1**).**"**

I looked for and couldn't find the x-ray images in the cited paper (Rosqvist et al., 2004), but did find relative density plots for different cores from Lake 1009 and Pajep Luoktejaure (Karlén, 1981, cited elsewhere in the paper) so perhaps this was mis-cited?

Rosqvist et al. (2004) was referred to as this is one of the last papers where the methodology applied to the cores of Riukojietna was described. Before the development of Itrax core scanners in the current millennium, sediment cores were X-rayed in 25 cm overlapping sections and each of them had individual X-ray exposure histories; Programs were developed that helped automate the collation of individual X-ray sections into a normalised expression for the entire core. We have added (L. 284) Rubensdotter and Rosqvist, 2003 to the list because it has a more extensive presentation of the scanning technique.

Line 440-442: Did the authors consider other sediment sources from the catchment that could explain the diffuse laminations in the higher organic content section between 107 and 91 cm, such as episodic contributions from the slope above the lake, which I imagine would have been quite an active periglacial landscape upon retreat of the FIS?

Yes we considered alternative sources explicitly in the introduction (L. 92-95; "However, several other factors influence the sedimentation rate in proglacial lakes, such as hydrological regime, intermediate sediment storage capacity, <u>and the activity of other geomorphological processes</u> within the catchment (Leonard, 1986; Rubensdotter and Rosqvist, 2003; Jansson et al., 2005)" and, based on the in-depth studies of Rubensdotter and Rosqvist (2003) and the physiography of the Pajep Luoktejaure catchment, we concluded under study site (L. 140-143) "…a thin and blocky till cover, <u>gentle slopes</u>, and an absence of sediment-rich landforms in the catchment implies that glacial flour is the dominant source of minerogenic material in downstream lakes. As such, we expect lake records from the catchment to primarily reflect fluctuations in glacier activity and extent".

3. **Integration of records & role of forward modeling:** It appears that the modeling exercise is used to validate the glacial history inferred from the lake record/single down valley exposure age. However, there are some inconsistencies between the measured ^{14}C concentrations and those modelled with using this history and I would have liked to see an exploration of alternate exposure/burial histories that might better explain the measured ^{14}C concentrations as well as the ^{10}Be concentrations in samples -003 and -004. Doing so would fully realize the authors' stated approach of "combining direct evidence for ice-free and ice-burial durations from cosmogenic nuclide chronometry… and indirect evidence of extent and glacial activity derived from proglacial lacustrine records" (Lines 102-104). I believe the following suggestions would be relatively easy to implement with the existing code.

Calculate the exposure & burial durations implied by the simplest two stage model for -003 & -004 – at present, these are inferred from the two-nuclide diagram, but appear overstated for -003 & -004 as 4–5 kyr (Line 413; it looks closer to 2.5–5 kyr). I agree that -001 & -002 have clear inheritance and can't yield an interpretable burial age.

Yes, the plotted points suggest a range of perhaps 3.5-4.5 kyr. If we consider the upper and lower limits of the corresponding error ellipses, then that expands to perhaps 2.5-5 kyr, per the suggestion above. However, we are also arguing that their history is likely complex as well. Since they are from the same landform and their values are not significantly different (overlap at 1 sigma) we argue that some sort of mean is appropriate – hence about 4 kyr.

We have therefore modified said statement to the following (L. 568-571): "Like the highest-altitude samples, the samples from the bedrock knob (Riuko-16-003 and 16-004) also plot in the complex exposure field of the ^{10}Be-^{14}C two-isotope plot (Fig. 3b), consistent with a mean burial duration of 4 kyr in the simplest interpretation of continuous exposure followed by burial and exposure by recent ice retreat.".

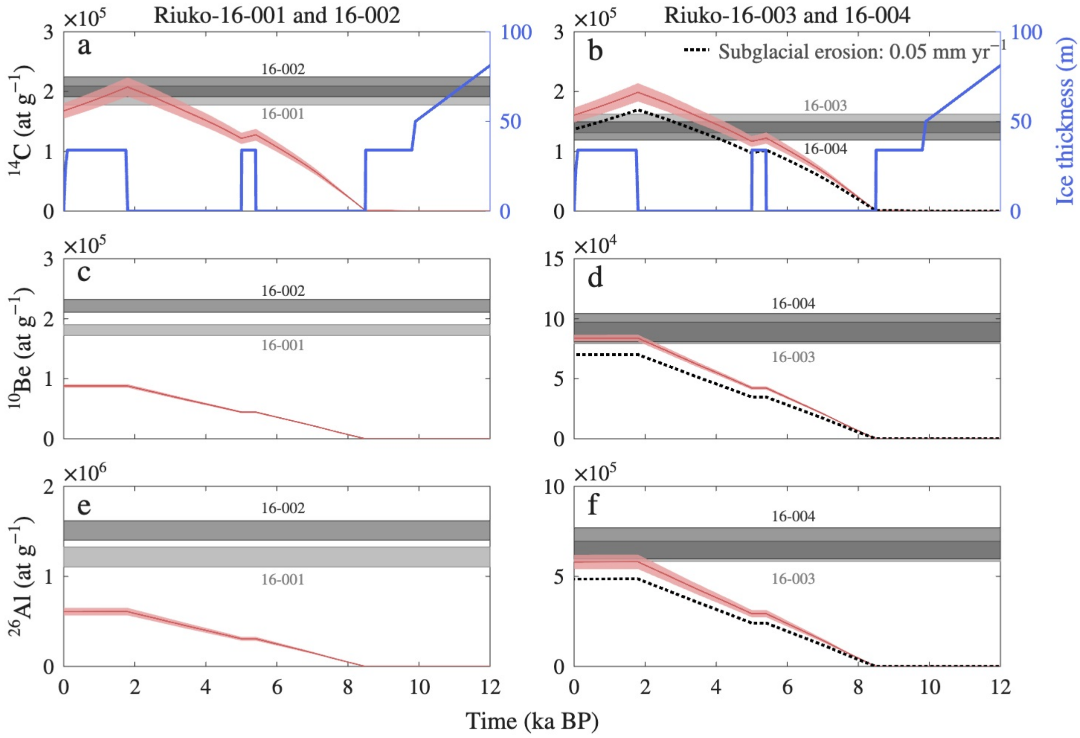
Evaluate the two-state history in light of the preferred history from Section 5.2 – Based on the apparent ^{10}Be ages and where the data plot on the two-nuclide diagram, it appears the combined histories will be something like 8.5–11.5 ka. The high end of this is obviously too long given the regional deglaciation age of 9.8 ka and implies some ^{10}Be inheritance as stated, but the lower end might be allowable.

If we just take the burial and exposure times from the lake record, that's about 5.9 kyr exposure and about 3.9 kyr burial. Assuming the reviewer is just asking about -003 and -004, 3.9 kyr of burial works with where 003 and 004 plot on Panel B, but 6-6.5 kyr on the x-axis is too old relative to the higher-resolution lake record. If you include the confidence ellipses then perhaps something like 8.5-11.5 kyr would come from that when the samples are considered separately. But as above, the samples also overlap with each other at 1 sigma in both directions and were from the same landform so we argue that the burial is more likely somewhere around the mean of about 4 kyr, which puts us back at about 10 ka – consistent with the lake record and independent deglaciation age.

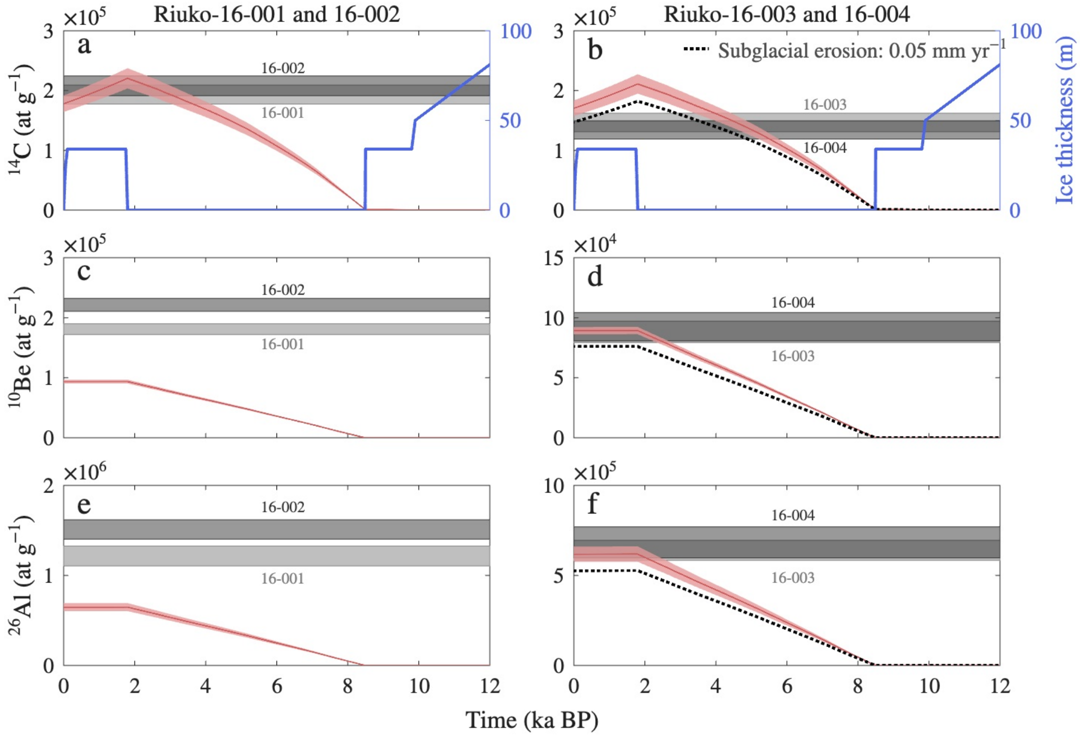
Consider deglaciation of the plateau/knob samples before 8.1 ka – I suspect that if you loosen the deglaciation age (see Specific Comment 1 about the lake outlet sample), you can more closely (or completely) match both the ^{14}C and ^{10}Be for samples -003 and -004 (with a little more LIA erosion) and hit the ^{14}C for -001 and -002 (with very little or no LIA erosion). Finding such a history would make it harder to argue that the ice cap never disappeared completely, but this possibility should be explored. An easy way to do this would be to just iteratively play with the deglaciation age and sample-specific erosion rates. Alternatively, you could fit the deglaciation age & erosion rate for sample -003, then apply that exposure history to the other samples to see if you can match the measured concentrations with less erosion. If there are no histories that better match the measured nuclide concentrations, or if the histories and/or erosion rates are unrealistic, then the current preferred history, and inference that -003 and -004 have a small degree of ^{10}Be inheritance, is probably most reasonable

See our previous responses to Specific Comment 1 by this reviewer and to the similar comment by reviewer 1 (Schimmelpfennig) – there is just not much leeway on that age. As such, we don't view a looser deglaciation age constraint as a particularly likely scenario to explore. However, as with sample 16-005 mentioned above, we did consider the potential effects of modern snow shielding (mean measured peak accumulation of ca. 1.2 m at the

site of samples 16-003 and 16-004) over the Holocene histories of these samples (see our response to reviewer 1 for details). The effect of this scenario would is an increase in simple exposure age of ca. 6% for ^{10}Be and ^{26}Al and ca. 7% for ^{14}C. We also ran the forward model (both with and without mid-Holocene readvance) with the snow shielding (including a shielding-corrected age for 16-005 of 8.5 ± 0.3 ka) – results for all nuclides are essentially identical to the non-snow-shielded runs in Figs. 6 and 7 (see below). Given the measurement uncertainties as well as uncertainties in the other parameters (including production rates, snow shielding, and erosion rates), we argue that further efforts along these lines would not be a particularly informative exercise, and would be unlikely to tell us anything substantively new that is not already reasonably well-constrained within those uncertainties.



Forward model run with modern snow-cover corrections through the Holocene – with mid-Holocene re-advance.



Forward model run with modern snow-cover corrections through the Holocene – with no mid-Holocene re-advance.

Minor comments & technical corrections:

1. **Abstract:** Although well written, the abstract is quite long and detailed. I suggest shortening it by removing the more specific details and instead summarizing the key results and findings from the lake sediments, bedrock samples, and the modeling exercise. For example, the description of the sediment record could be shortened to something like "We infer three periods of glaciogenic sediment deposition >9.8 ka, from 5.4–5.0 ka, and after 1.8 ka, with intervening gyttja that indicates minimal or no glacial influence." Similarly, the cosmogenic-nuclide results could be shortened, for example: "Cosmogenic ^{14}C, ^{26}Al, and ^{10}Be concentrations in a bedrock sample near the LIA moraine indicate continuous exposure since 8.1 ± 0.1 ka. Nuclide measurements in four bedrock samples from recently exposed bedrock indicate complex exposure/burial histories. We perform a forward modeling exercise to determine whether the cosmogenic-nuclide concentrations in the recently exposed bedrock samples are consistent with the glacial history inferred from the lake sediment record and the deglaciation age of 8.1 ka..."

   Both reviewers asked that the abstract should be shortened, and this reviewer offered guidance to which section too shorten to achieve this objective. We have adopted the suggestions offered above, and have achieved an 18% reduction in abstract length. Most of this was a shortening of the final paragraph, which now reads (L. 37-44): "We perform a forward modeling exercise to determine whether the cosmogenic-nuclide concentrations in the recently exposed bedrock samples are consistent with the glacial history inferred from the lake sediment record and the deglaciation age of 9.8 ka. Riukojietna persisted during the Holocene Thermal Maximum (ca. 8-5 ka), in contrast to earlier suggestions that Scandinavian glaciers vanished during the Holocene, as a result of an inferred increase in precipitation due to atmospheric circulation changes. The glacier has been in a retracted

state similar or smaller than today during the late Holocene, as climate grew colder and drier. This approach combining short- and long-lived cosmogenic nuclides with lake sediments can thus provide new constraints on high-latitude Holocene glacial and paleoclimate history."

2. **Line 20:** Specify 5 bedrock samples.

   This is specified in line 25.

3. **Line 318:** State range for each nuclide individually.

   Agreed. The sentence now reads (L. 360-362): "The corresponding simple exposure ages range between 3.2 ± 0.2 ka and 8.2 ± 0.5 ka for in situ ^{14}C, 6.3 ± 0.3 ka and 14.3 ± 0.5 ka for ^{26}Al, and 6.1 ± 0.3 ka and 14.7 ± 0.4 ka for ^{10}Be (Fig. 2; Tables S1 and S2)."

4. **Lines 403-417:** Consider moving the discussion of the complex histories to the modeling section (5.3) to improve flow.

   This was a good suggestion – accomplished. Consequently, we have reformulated section 5.1 to (L. 460): "5.1 Constraints on the Holocene history of Riukojietna from the lake outlet sample"

5. **Lines 422-430:** Is this paragraph necessary? It's clear from the ^{10}Be and ^{26}Al inheritance in the plateau samples that the southeastern tongue is less erosive, but there's not enough information here to evaluate the relative sediment contributions of each tongue to Pajep Luoktejaure throughout the entire Holocene. I might be missing something, but I'm not sure knowing the relative contributions would influence the interpretation of the lake sediments.

   This is an interesting point, but not one we were trying to make – we merely use the sediments to pin-point times when the glacier would have grown enough to produce glacial flour that could reach Pajep Luoktejaure. We have included this paragraph mostly because the southern tongue seems an obvious alternative source of meltwater and sediment – and believed we needed to address this potential source. It also brings out the strength of field measurements of cold-based conditions and found it backed-up by subglacial preservation (non-erosion) as inferred from cosmogenic nuclides over much longer timescales – as observed by the reviewer. We would like to retain this paragraph for the sake of completeness, but would also consider deleting it if the editor thinks that is required. Because of changes to Figure 2 asked for by Reviewer 1, we have further amended the figure by removing the black arrow (because we don't see the evidence that the tongue extended that far ever during the Holocene), and deleted its mention from the Figure caption. Hence, "Black arrow indicates a possible earlier extension direction of the ice cap." was removed.

6. **Line 435:** Restate that the age comes from the next lake down.

   We added (L. 500-501): "…(age constraint from nearby lake Vuolep Allakasjaure)"

7. **Line 439:** To avoid confusion with a glacier readvance, I suggest rewording "remained in a relatively advances position during the early Holocene" to something like, "had not yet retreated from the glacier catchment during the early Holocene."

   The original suggestion is both clearer and better, so we kept the phrasing as is.

8. **Line 545**: Should 4.8 ka be 5.0 ka?

   Good catch: thank you! We indeed refer to the period before 5.0 ka, and particularly to the period between 8.1 and 5.4 ka where diffuse laminations support the presence of the glacier but where LOI indicates it to be smaller than LIA and perhaps smaller than today. So, we changed the sentence accordingly: (L. 658-660) "However, although the ice cap perhaps remained smaller than today until shortly before 5.4 ka, diffuse fine silt and clay laminations indicate that the glacier persisted and produced sediment." And L. 687-688: "Thinning and retreat of Riukojietna is implied by the cessation of glacigenic sediments into Pajep Luoktejaure after 5.0 cal ka BP"

9. **Figure 4c:** It would be helpful to indicate on the figure how the timing of inferred glacial activity was determined since a range of methods were used (radiocarbon from this core, radiocarbon from the lake below, inferred from the age model, exposure dating). It would also be helpful to add lines that connect the stratigraphy/inferred glacial activity across the panels.

   We disagree with the reviewer. The figure is best viewed with data listed that only pertains to core PL1-169 that is plotted. Having said that, we recognize that more information is required to go from panel b to panel c than what was originally supplied, and so have amended the figure caption as follows (L. 419-432):
   "Figure 4: Stratigraphy and loss on ignition (LOI %) from (a) surface gravity core PL3-25 and (b) Livingstone piston core PL1-169 in Pajep Luoktejaure (Fig. 2a). High LOI indicates high organic content, interpreted as relatively low glacial activity, while low LOI indicates low organic content associated with a relatively active glacier and minerogenic influx. Core PL1-169 has 3 radiocarbon tie points (in cal ka BP; Table 3). (c) Composite core of LOI record versus age, where 1 – 13 cm are from PL3-25 and 13 – 130 cm are from PL1-169. Age vs. depth for PL1-169 below 130 cm is poorly constrained. (Fig. S1). In this conversion we replaced the basal bulk sediment age of 11.0 ± 0.1 cal ka BP with a terrestrial macrofossil date of 9.8 ± 0.2 cal ka BP from the same stratigraphic position (first radiocarbon accumulation after deglaciation) from nearby lake Vuolep Allakasjaure (Table 3). Blue fields indicate our inferred periods of glacier activity. The lowest field is informed by the cosmogenic nuclide retreat age of Riukojietna from the outlet of Lake 1063 at 8.1 ± 0.1 ka. The youngest period of inferred glacier activity is informed by the oldest ages of glacier reactivation of 1.8 ± 0.1 cal ka BP (on macrofossil and bulk sediment; Table 3) and final retreat from the LIA moraine at 1910 CE. During the mid-Holocene short period of glacier activity between 5.4 and 5.0 cal ka BP, LOI values are on-par with the previous and subsequent periods of inferred glacier activity."

10. **Figure 3b**: Are the vertical dashed lines (decay trajectories) also meant to represent exposure isochrons? If so, what is the spacing? Including fewer decay trajectories that correspond to interpretable exposure durations for a simple exposure/burial history would aid the reader in quickly interpreting the figure, although this may require restricting the x-axis limits.

    The near-vertical lines are not exposure isochrons but rather decay trajectories if fully buried after reaching a given surface concentration. They do not represent re-exposure trajectories as production and decay would follow different paths. And if there is inheritance in ^{10}Be then that two-stage interpretation is not meaningful. We have modified the caption to reflect this (L. 391-394): "Near-vertical dot-dashed lines are decay trajectories followed during burial, plotted in A) for every fifth age in a logarithmic array of 100 ages generated from 100 to 10^7 yr that lie within the x-axis limits, and in B) for every

other age in a logarithmic array of 100 ages generated from 100 to 2x10^5 yr that lie within the x-axis limits."

In conclusion, we like to thank the reviewer for a useful evaluation of our work, and have added this statement to our Acknowledgements: "**We thank Irene Schimmelpfennig and an anonymous reviewer for helpful comments that improved the manuscript.**"