Reviewer 1: Prof. Irene Schimmelpfennig

We would like to thank Prof. Schimmelpfennig for her 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**.

Dear editor and authors
This manuscript reports new cosmogenic multi-nuclide data from proglacial bedrock and sediment record data from a proglacial lake at a small ice cap in northern Sweden, used to reconstruct the local Holocene glacial history and to interpret the underlying paleoclimatic conditions. The study and data are very useful for the understanding of the Holocene glacier and climate evolution in Scandinavian high latitudes, especially due to the combination of different geochronological and geomorphic/paleoenvironmental approaches, which allow for a more complete scenario than the application of one of the approaches alone. The manuscript is very well written, structured and illustrated. I appreciated reading it and recommend its publication in Climate of the Past. I suggest a few minor issues be addressed though.

Thank you for this general positive appreciation of our manuscript.

My main comments relate to the interpretation of the cosmogenic nuclide data:

Altogether, the fit between the modelled and measured nuclide concentrations and the overall consistency between the complementary records are remarkably good. However, several uncertainties seem to be underestimated or have been ignored, which could in some cases explain the observed slight misfit between modelled and measured nuclide concentrations (or in other cases worsen it). I therefore suggest to add them to the discussion.

We address the concerns regarding the interpretation of the cosmogenic data point-by-point, below, and outline what changes we propose to the manuscript as a consequence of those concerns.

First. One of the key pieces of information for the interpretation of the exposure-burial history of the ice cap is the timing of glacier retreat from Lake 1063 (calculated as 8.1±0.1 ka from the exposure ages of the three nuclides), but its uncertainty is likely underestimated and should be better discussed.

1. Lines 322-323: I'm not convinced of using the weighted mean approach for this calculation, as it underestimates the uncertainty of the result. Is the weighted mean age calculated with the internal or external uncertainties of the ages from the three nuclides? Is a ~1% uncertainty of this age realistic given that the production rate uncertainties for the different nuclides are 7-11%? I don't think so.

Thank you for highlighting this issue, as the age is of crucial importance to our modelling history. Per this request, we have now used external age uncertainties: ^{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 have 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. Hence, we retain the value of 8.1±0.1 ka.

We have incurred the following modification to the manuscript (L. 365-376): "All simple exposure ages for this sample agree within 1σ **external (and internal) uncertainty** (Fig. 2). **Using external uncertainties, the weighted mean and the larger of either the standard error in inverse-error-weighted mean, or the inverse-error-weighted average variance (Bevington and Robinson, 2003) yields 8.0 ± 0.1 ka, while the straight mean and standard deviation yields 8.1 ± 0.1 ka. As these values are not significantly different, we take the straight** mean ^{14}C-^{10}Be-^{26}Al exposure age of 8.1 ± 0.1 ka (1σ) as the deglacial age for that location (Koester, 2023)."

In conjunction to this question, we have also amended the figure caption of Figure 2 and listed that uncertainties shown are internal uncertainties (L. 148): "Simple exposure ages **with internal uncertainties** (in ka) are shown in white boxes."

Finally, we have added this reference to the list of references (L. 785-786): "**Bevington, P., and Robinson, D.: Data Reduction and Error Analysis for the Physical Sciences, McGraw-Hill, New York, 320 p., 2003**."

The effect of snow cover on the nuclide concentrations has completely been ignored. I guess that there is quite a lot of snow fall in that region. Although I'm not in favor of applying a default snow correction to the interpreted ages, a potential snow effect on this glacier retreat age should at least be acknowledged with an approximate idea of how much this could make it older.

We concur with the reviewer that snow cover does influence cosmogenic nuclide production in the rock surfaces after ice retreat, and we appreciate the opportunity to address this question. There are two aspects that we can highlight in this discussion. First, Riukojietna has been one of the reference glaciers in Sweden following pioneering mass balance studies of Rosqvist and Østrem (1989) since 1986 (reported to the World Glacier Monitoring Service). Hence, we have measurements constraining winter accumulation on the ice cap (so, immediately above the Lake 1063 sampling site). On average, on the lower part of the ice tongue – roughly at the elevation of sampling sites 16-003 and 16-004, the average peak annual accumulation (in April) is 1.2 m. Hence, let's assume that site 16-005, at lower elevation and less shielded by topography, had a lower peak annual accumulation of 1 m. To calculate its influence, we have additionally assumed a linear increase in snow cover from zero on October 1 each year to a maximum of 1 m on April 15 the following year and back to zero on June 30 that same year. This constitutes to some burial over 9 months of the year. Second, we simply need to assume (erroneously, no doubt) that the last four decades of accumulation are typical for this site and so use this history to calculate the shielding of cosmogenic nuclide accumulation at site 16-005 since 8.1 ± 0.1 ka. Assuming a mean snow density of 0.25 g cm^{-3} and an attenuation length of 160 g cm^{-2}, the annual effect would be a reduction in production rate of ca. 4.75% vs. assuming no snow cover. If we assume that applies throughout the exposure history, then that would imply a shielding factor of 0.9525 for that sample and a ca. 5% increase in age for ^{10}Be and ^{26}Al, and an 8% increase for ^{14}C (all within published production rate uncertainties). This 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. Because of all the inherent uncertainties laid out above (see also Ye et al. (2023)* for a detailed view of the potential pitfalls of using modern snow-cover corrections over millennial time scales), and considering that production rate sites normally do not compensate for snow cover (for

much the same reasons), we feel it does not warrant inclusion beyond the discussion in this response.

**** Ye, S., Cuzzone, J.K., Marcott, S.A., Licciardi, J.M., Ward, D.J., Heyman, J., and Quinn, D.P., 2023, A quantitative assessment of snow shielding effects on surface exposure dating from a western North American ^{10}Be data compilation: Quaternary Geochronology, v. 76, 101440, doi:10.1016/j.quageo.2023.101440***

Second. The muogenic contribution to total 14C production is still not well constrained, but it is substantially higher than for 10Be and 26Al. Therefore, 30 m of ice cover might not be sufficient to shield a surface completely from 14C production (Lines 62-63, 309). According to model results in other studies, 14C production is still ~4% of that at the surface at 30 m beneath the ice (Hippe, 2017, http://dx.doi.org/10.1016/j.quascirev.2017.07.020). The effect is probably still small in this setting, but the related uncertainty could be worth being mentioned in the discussion.

The depth-dependence of the muon component is included in our modeling via the Balco (2017) code. A value of 4% of the surface production of ca. 40-45 at g^{-1} yr^{-1} would be on the order of 1.5-1.8 at g^{-1} yr^{-1} beneath 30 m of ice. Resulting concentrations for continuous burial at 30 m ice depth for ≤6 kyr would be less than the measurement uncertainties for the samples (c. 8000 at g^{-1}). At saturation, c. 25 kyr burial at that depth, the concentration would be less than twice that (c. 15000 at/g). The effect is even less significant at our site given that we are already accounting for that production.

10Be and 26Al ages are indistinguishable within uncertainties for all five bedrock samples. This is stated in line 325, followed by disregard of the 26Al in the following interpretations (except that both 10Be and 26Al indicate inheritance, line 509). However, it would be interesting to explore and explain the implications of this apparent age consistency for the long-term history of the ice cap: in lines 409-410 it is stated that that the combined information from the 14C-10Be concentrations and the deglaciation age (~10 ka) can only be explained through pre-LGM exposure. But when did this pre-exposure occur at the earliest? It should be possible to infer this from the fact that 26Al has not yet notably decayed compared to 10Be.

We agree that reconstructing the pre-LGM exposure history of Riukojietna would be valuable. However, the long half-lives of ^{26}Al and ^{10}Be, even though they differ by ca. a factor of 2, mean that a distinguishable difference in nuclide ratios requires on the order of > 100 kyr to develop given current analytical and production rate uncertainties. In our case, the inherited ^{26}Al and ^{10}Be component corresponds to < 5 kyr of exposure even in the oldest samples. This contribution is therefore too small to meaningfully constrain the pre-LGM exposure history of our sites. For this reason and to maintain a concise narrative, we therefore prefer to not unfold this aspect further in the paper.

I suggest to shorten and simplify the abstract, which is currently very long and contains a lot of details.

Indeed, the abstract is too long and has been shortened 18% in length following revisions suggested by Anonymous reviewer 2. Most of the shortening occurred in 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.”

Minor suggestions per line:

- Line 44: I suggest rephrasing to clarifier that “as a result of…” refers to the first part of the sentence

  Here, we disagree with the reviewer. The part of the sentence the reviewer refers to is a subordinate clause (and so could be discarded without loss of the structure of the sentence), which would then read “We use these results to infer that Riukojietna persisted during the Holocene Thermal Maximum (ca. 8-5 ka) […] as a result of increased precipitation due to atmospheric circulation changes.”

- Lines 102 (and conclusions, line 595): I suggest removing “direct” in front of evidence, as complementary information is needed to interpret the nuclide concentrations and to use them as reliable constraints on the ice cap dimensions and exposure-burial durations.

  Done; L. 54: A popular metod…; L. 117 and 119, removal of direct and indirect, respectively; L. 555 changed “directly” to “more directly”; L. 710 and 711, removal of direct and indirect, respectively.

- Line 234: add that these are spallation production rates

  Done

- Lines 239-243 and caption of figure 2: specify if the ages are shown with their internal or external uncertainties

  L. 269: addition of “and 1σ uncertainties”
  We have changed Figure 2 caption as well, as mentioned above (L. 148): “Simple exposure ages **with internal uncertainties** (in ka) are shown in white boxes.”

- Lines 339-340: “hence corresponding units are in years” could be removed, as the information is already provided 4 lines above.

  Done

- Line 361: should “LOI record against age” be in quotation marks? Otherwise the wording is unclear.

  We think that with your comment, you identified the missing of a word (“of”) which made it difficult to understand. Hence, changed to (L. 423): “Composite versus age …”

- Lines 392-393: The relevance of the part “in relation to…Pajep Luoktejaure” seems unclear in this sentence. I would remove it from here and make a new sentence afterwards, e.g. “This is corroborated by the minerogenic input seen…” or “This is used to constrain the timing of minerogenic input…”, depending on what you want to say here.

  We are pleased that the reviewer picked-up on this inconsistency, and decided to simply remove the clause. The sentence now reads (L. 464-466): “Thus, this result establishes that

Riukojietna did not cover or expand beyond the Lake 1063 outlet sampling site after 8.1 ± 0.1 ka (mean of all three nuclide ages; Fig. 2).”

- Line 401: the white dots would be better visible if they had another color.

  Thank you for noting this – we have replaced with a different color (Bordeaux red).

In conclusion, we like to thank the reviewer for her 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.**”