Minor comments

The comments from the reviewer 3 (RC3) use the black font color. Our comments (AC) are using the blue font color.

Line 14-15. Delete "giga-scale tsunamigenic"

Giga refers to the common prefix used on the metric scale indicating that the volume is in the 10^9 scale (m^3). We will also add "potentially" so that in the revised manuscript the sentence will read: "Recent mapping of the seabed in the Vaigat Strait has revealed several prehistoric giga-scale (volumes of 10^9 m3), tsunamigenic landslides; however, their ages are unknown.

Line 15. Holocene? Younger than deglaciation?

Agreed. It will be specified that their age is younger than last deglaciation.

Line 15. Well, only two of the six lake basins show possible tsunami deposits. And two other lake basins reveal only very short cores, one is only 9 cm long.

Agreed. The revised abstract and manuscript throughout will reflect that we found tsunami deposits in two lakes and used two to constrain run-up magnitude and timing. The two cores that do not have relevant information will be mentioned but not presented in detail.

Line 48, (348 & 353?). Geology is more important than the authors. Especially Svenning is emphasized many places in the paper, rewrite.

These comments are difficult to understand. If it is a reference to that the Svennevig (2023b) manuscript was in review at the time of submission of the present NHESS paper, then it has now been accepted for publication I Geology.

Line 102. We need much more information about the coring. This is very important and a weak point of this study. See comments.

Agreed. See our response in your main comments.

Line 125. Were you able to find terrestrial material? None is mentioned in the table of ages.

You are correct, this is not accurate. No terrestrial macrofossils are listed in the tables because we did not find any. In the revised manuscript the sentence will read. "It was not possible to find any terrestrial macrofossils although they are ideal for 14C dating (Strunk et al., 2020)."

Line 128. Ohoy....not true - see comments. The material used for radiocarbon ages is another weakness with this study.

Agree. We can eliminate calcareous rocks as a source for older carbon, but as you mention, we should expect problems from aquatic mosses and bulk sediment sampling due to reservoir effects. Especially if sampled from the stratigraphical level just above the tsunami facies (Bondevik et al., 1997b). See other responses on revised interpretation of dates.

Line 137. Say why you use these ages - for sea levels. Also here indicate the change in uplift rates between the two locations - any tilt of shorelines between the two sites?

We will add the motivation for using the Arveprisen Ejland RSL.

We do not consider that the data for uplift rates and paleo shorelines to be accurate enough to quantify this. According to the hypothetical shoreline relation diagram (Long et al., 1999, Fig. 3.) Saqqaq would have seen slightly higher uplift rates than Arveprinsen Ejland and Saqqaq and would have deglaciated before Arveprinsen Ejland. This means that when we apply the RSL curve from Arveprinsen to Saqqaq, we underestimate the tsunami run-up heights but cannot confidently say by how much. The revised manuscript will read:

“There is no local relative sea level curve for Saqqaq, so we recalibrate existing isolation basin radiocarbon dates from Long et al. (1999) from Vaskebugt (Kangerluarsuk), Arveprinsen Ejland (Alluttoq) (See Fig. 6 for RSL curve). This location is situated 40 km to the southeast of Saqqaq (Fig. 8) and would have deglaciated a few hundred years later than Saqqaq, which would have seen slightly higher uplift rates (Long et al., 1999). Consequently, when using the Arveprinsen Ejland Ejland RSL curve on Saqqaq, it will underestimate how much uplift has occurred at any given time.

In addition to the radiocarbon dates used for the RSL curve from Long et al. (1999) we also use radiocarbon dates from Weidick (1968, 1972) where shells provide a minimum age of the last deglaciation at Saqqaq (Weidick, 1968) and a minimum height of the relative sea level (Weidick, 1972) at that time. We have recalibrated all samples, so they are standardized with the radiocarbon ages used in this study (Table 1). The 14C ages for samples from this study and Long et al. (1999) were calibrated into calendar years using the OxCal v4.4 (Bronk Ramsey, 2009) and the IntCal20 calibration curve (Reimer et al., 2020). K-994 has not been normalised for isotopic fractionation to a delta 13C value of -25 permille i.e., we have added 400 years before calibrating the date into calendar years using Marine20 (Heaton et al., 2020) using a local dR= -49±59 from West Greenland (Pearce et al., 2023).”

Pearce, C., Özdemir, K. S., Forchhammer, R. C., Detlef, H., and Olsen, J.: The marine reservoir age of Greenland coastal waters, Geochronology Discuss. [preprint], https://doi.org/10.5194/gchron-2023-7, in review, 2023. [Table 5, zone 5]

Line 153. Disappointing....you should try to be honest and show to the reader the problem with your data. Only two of the six basins you have investigated contains tsunami deposits. Well, don't try to hide this fact - I am sure you are honest scientists - don't let your readers have a chance to think otherwise.

Agreed. The revised manuscript throughout will reflect that we found tsunami deposits in two lakes and used two to constrain run-up magnitude and timing. The two cores that do not have relevant information will be mentioned but not presented in detail.

Line 154. Hm.....no relevance for this study- 9 cm long core published for completeness? Difficult to understand. Instead just say that you cored this lake and you only managed to recover 9 cm of lake sediments. There is no good arguments that you should present the detailed stratigraphy of these 9 cm core. It is just annoying..

Agreed. The rationale behind presenting cores 05 and 08 was that the information would be available to any researchers who might want to expand on the work in the future. It is correct that there is no useful information for this study in these cores, so they will be deleted in the revised manuscript. Researchers can still access the data from the core from the Data repository and understand their context from this discussion.

Line 176. Ohh...can this be correct? The basin is located below the marine limit....deglaciation was completed by 9500, so any influence of a glacier would than be in a glacio-marine environment. And if this is correct, where is the marine sediments and the isolation contact that is younger than the deglaciation? And why is the transition from the silty sand facies to the gyttja dated to 2.8 ka yr BP?

You are correct that this core has too little information and cannot be interpreted reliably. We will exclude it from the study as you suggest.

There is a good chance it is indeed glaciomarine sediments as you say, but the age above it is confusing and indicates a lot of sediment is missing. Regardless, if the origin of the sediment is glaciolacustrine or glaciomarine, an explanation for the hiatus and age above the minerogenic sediments would be the desiccation and erosion of the dry lake surface.

Figure 1. Map should also show Arveprinsen

Arveprinsen Ejland is outside of the frame in this figure. When the location at Arveprinsen Ejland is discussed first time in the paper it refers to figure 8 (Line 141 "…Arveprinsen Ejland (Alluttoq) (See Fig. 6 for RSL curve). This location is situated 40 km to the southeast of Saqqaq (Fig. 8)…"

We will add "Arveprinsen Ejland to Fig. 8.

Figure 2. What is this? Not explained.

It can be found in the legend as "Kingittoq Mb". We will see what can be done to improve the symbology in the legend so that it better resembles the use of the symbology in the map.

Figure 2. Map should have contour lines, maybe every 10 m.

We have not used contour lines on this figure as they would make the figure “busy” and chaotic and very hard for the reader to understand. Instead, we use a color bar showing the elevation combined with a terrain shaded relief. The purpose is to enable the reader to understand the concepts discussed in the manuscript, but it is of course not possible to read an exact elevation off the map.

We would like to maintain it as it is.

Figure 4. Delete SAQ21-05.

Agreed. It will be deleted.

Figure 4. SAQ21-06: Also part of T1? You show otherwise in Fig. 5.

Agreed. Figure 4 is correct. Fig. 5 will be changed according to Figure 4.

Figure 4. SAQ21-07: Insert altitude of lake thresholds.

Lake threshold altitudes are found below the cores. They will be moved to the top so that the placement in figure 4 is consistent with the placement in figure 5.

Figure 4. SAQ21-08: Delete. Instead of macrofossil say what it is: "Aquatic moss".

We will change the legend to “Aquatic moss”.

Figure 5. Is this a rip-up clast? Looks that way...

Yes, it is. It is one large monolithic clast which does not currently have a signature on the lithological description. This will be fixed in the figure in the revised manuscript. In the extra core from this lake (not presented in manuscript but available in the data repository) this block is almost 90 deg rotated from horizontal.

Figure 5. Looks like an isolation contact...but maybe not? Fresh diatoms?

That was also our hope that it was a much-coveted isolation contact or that it at least was a brackish transition to basin isolation. But virtually all data speaks against it. Primary sedimentary structures are laminations, the 8.1 cal. ka BP age in the top corresponds to 13 m a.s.l. at the time, Ca/Fe values are low, and diatom species are typical of ones that inhibit freshwater or weakly brackish systems. Of course, uplift rates are high at that time and at 9.1 ca. ka BP this lake would be submerged by ~6 m.

Overall, our interpretation is that this is not an isolation contact. A likely explanation for the high Ti, low Ca/Fe, and the grey colors is increased precipitation leading to more terrestrial minerogenic sediments being flushed into the lake.

Figure 5. Beautiful tsunami facies! (T1)

We agree!

Figure 5. Difficult to see any clasts in here....(T2)

The image compression is doing the damage/smudging of here. We have to find a better balance between maximum file size of the figure (to reduce compression noise) or we may have to make a separate figure so the facies can be inspected by the reader. But the exact implementation depends on formatting procedures we cannot know at this stage.

Below, the T2 tsunami facies is shown in better resolution. Erosional contact in the bottom, organic rip-up clasts, plant fragments, in a matrix of sand (the large fragment in the top is the aquatic moss providing the 7.6 cal. ka BP age of the tsunami). Laminated sediments in the shows is the resumption of lacustrine sedimentation.

Line 708. Is this correct? If you use delta R values you shouldn't correct for 400 years.

Agreed. This has also been fixed in line 134, where the revised manuscript will read: “K-994 has not been normalised for isotopic fractionation to a delta 13C value of -25 permille i.e., we have added 400 years before calibrating the date into calendar years using Marine20 (Heaton et al., 2020) using a local dR= -49⍰59 from West Greenland (Pearce et al., 2023).”

Our revised calibration yields a median age of 10026 cal. ka BP and a mean of 10025. The min-max bracket is 9525-10521, and sigma 256. Table 1 will be updated accordingly, as will Figs. 2, 6, and 7 where k-994 is occurring.

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