Response to reviewers and editor

We have revised the manuscript titled “Evidence of middle Holocene landslide-generated tsunamis recorded in lake sediments from Saqqaq, West Greenland” (former title “Giant mid-Holocene landslide-generated tsunamis recorded in lake sediments from Saqqaq, West Greenland”) and hereby submit it for review in NHESS. In this revised manuscript all the concerns and comments of the reviewers have been addressed.

This document contains the line-by-line response to all three reviewers. It has largely been possible to implement the changes verbatim into the revised manuscript and where this was not possible, the substance of the response is unaltered.

The one exception is the inconsistent presentation of the unit above the T1 in core SAQ21-06 in the preprint manuscript, in both manuscript text and Figs 4 and 5. We are convinced that the interpretation of this unit is possibly part of the tsunami sediments is wrong and it is a lacustrine laminated gyttja. The “young” 5.8 cal. ka BP date is found within this unit, and the interpretation as lacustrine deposit also makes the chronology of the radiocarbon dates consistent.

Reviewer 2 (de Batiste) noted that we relied heavily on an unpublished manuscript Svennevig et al., 2023b) which at that time was not accepted. We sent a copy of this manuscript to reviewer 2 and this version has also since then been accepted for publication. It was awaiting this present NHESS manuscript to be accepted, but due to the delays, it is now likely that publication will go ahead without waiting.

Reviewer 3 (Bondevik) highlighted issues with the field work where some parts of the lake sediments seem to be missing in the cores (esp. the lower marine part), and the lack of multiple corings to establish lake stratigraphy. In addition, reviewer 3 pointed out the weakness of the material used for radiocarbon dating.

There are no plans for a revisit to the Saqqaq foreland as logistics is very expensive in Greenland and we do not have the time; if there is no terrestrial material to date in the cores, then what there is, must be used with its inherent limitations. Instead of conducting a new field campaign, we have included reviewer 3’s concerns in the manuscript so that the reader is aware of these limitations.

The evidence we present in the paper to show the presence of tsunami sediments in the lakes is strong where it can be tested. We cannot test the criteria of the lateral extent of the tsunami deposits using the lake stratigraphy, but we have excluded other processes that produce similar sediments, and we also have an obvious source of tsunami initiation in the Vaigat strait.

The evidence is not complete, but overall, it is strong and well-supported by additional indirect evidence.

We have followed the reviewer’s advice on improving figures and table – with one exception. Reviewer Stein Bondevik asks for a figure with elevation contour lines and watersheds. The purpose is to show that the lakes have isolated and small catchments. We chose a different way of showing this by using the stream network calculated by the GRASS software and plotting it on Fig. 2. Here, it is evident that the lakes indeed are isolated with small catchments and adding contours and watershed on this figure would make it visually cluttered and very hard to understand.

We appreciate your patience with the time it has taken to resubmit the present manuscript, as we consider NHESS to be the right journal for our work.

On behalf of the authors,
Niels Korsgaard

Response to reviewer 1 (anonymous)

Major comments

Comments to manuscript: Giant mid-Holocene landslide-generated tsunamis recorded in lake sediments from Saqqaq, West Greenland

This is a well-conducted study, a fairly well-written and very well-illustrated manuscript. I am convinced that the two layers interpreted as tsunami deposits are correctly identified. The manuscript is suitable for Natural Hazards and Earth System Sciences

The authors would like to thank the anonymous reviewer for taking the time to review the manuscript and provide very constructive comments and suggestions.

We are happy with the positive evaluation, and agree to all comments and suggestions, and have changed the manuscript accordingly.

Minor comments

The comments from the anonymous reviewer 1 (RC1) use the black font color. Our comments (AC) are using the blue font color.

However, I have some comments and suggestions:

Line 21. Higher rather than larger.

Agreed. It will be changed in the revised manuscript.

Line 22, suggest: While records of two giant tsunamis can be found in lake sediments,
Agreed. It will be changed in the revised manuscript.

Line 44. Rewrite.
Agreed. It will be rewritten as "Farther north at Karrat Isfjord, a large rock avalanche triggered a tsunami in June 2017 that inundated the nearby settlement of Nuugaatsiaq and cost four human lives (Paris et al., 2019; Svennevig et al., 2020) showing that landslides are also occurring outside of the Vaigat strait."

Line 50. I think the bottom of Vaigat is smooth because there is a thick succession of Early Holocene glaciomarine deposits.

Agreed. For example, Fig. 3. from Marcussen (2001), which is used as reference in the manuscript, shows a thick cover of Holocene sediments on the floor of Vaigat. The sentence will be changed to include this fact. "They are recognizable by their hummocky and blocky topography in the otherwise smooth U-shaped glacial trough with a Holocene sediment cover comprising the bottom the Vaigat strait (Fig. 1.)"

Line 60. Elevations
Agreed. It will be changed to plural in the revised manuscript.

Line 63. Loon Lake
Agreed. It will be changed to Lake Loon in the revised manuscript.

Line 69. Vaigat strait

Agreed. “Strait” will be changed to small letter in the revised manuscript.

Line 79. delete time.
Agreed. “time” will be deleted in the revised manuscript.

Line 80. Change: Weidick and Bennike; Table 2; Fig. 2). To: Weidick and Bennike (2007; Fig. 22).
Agreed. The manuscript will be revised as suggested.

Line 80. According to Weidick & Bennike (2007) an age of 10 cal. ka BP. provides a minimum age for the last deglaciation. This is in accordance with K-994 (see comment below to Table 1).

Agreed. This will be revised as suggested. We also agree on the later comments on Table 1. The revised sentence will read:

“Weidick & Bennike (2007, Fig. 22) provides a minimum age of the last deglaciation of 10 cal. ka BP from dating marine shells at 70 m elevation today (Fig. 2). Thus, susceptibility of the lakes to inundation from tsunami waves would have been more pronounced in the Early Holocene considering Saqqaqdalen would have been submerged more than 70 m due to glacial isostatic subsidence, and in the mid-Holocene the glacial isostatic subsidence would be c. 20-25 m (7.2-7.6 cal. ka BP).

Line 82. sediment cover.
Agreed. We believe it is in line 83. “cover” will be added at the end of the sentence.

Line 92. not previously been.
Agreed. The sentence will be changed accordingly.

Line 102. Did you hammer the piston corer into the sediment?
Yes, we hammered the corer into the sediment. The sentence will be changed to “…at the deepest part of the lake by hammering a 60 mm piston corer into the lake sediments from a Zodiac inflatable boat.”

Line 106. Change: and the and to: and the
The excessive “and” will be deleted.

Line 116. described by rather than identified by
“Identified” will be replaced with “described” in the sentence.

Line 118. inundated by
“by” will be added in the sentence.

Line 120. rewrite, for example: The tsunami deposit is separated from existing lake sediments by an erosional unconformity. Erosion was caused by...
Agreed. The proposed sentence structure work much better and will be changed to “The tsunami deposit is separated from existing lake sediments by an erosional unconformity. The erosional unconformity was caused by the turbulence from the tsunami as it crossed the lake threshold.”

Line 131. we dated samples.
Agreed. “samples were dated” will be replaced with “we dated samples”.

Line 134. what about the marine sample? See Table 1
The revised manuscript will read: “The 14C ages for samples from this study 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 ^{13}C 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)."

Line 138. Certainly more than one shell. In the 60s you needed a handful of shells, around 100 gram of shells for a C-14 age determination.
The revised manuscript will use "Shells" instead of "One or more shells".

Line 139. Change: providing the minimum age of deglaciation to: providing a minimum age of the last deglaciation.
The revised manuscript will use "providing a minimum age of the last deglaciation" instead of the current "providing a minimum age of deglaciation".

Line 159. I am surprised to see that the coring tubes cracked. What material did you use? Polycarbonate?
We used polycarbonate coring tubes. It was part of a batch which was used successfully on previous field work. The tubes should supposedly not have been exposed to frost or sunlight since then, which could explain the cracking. But as the tubes did crack, the most likely explanation is that the tubes were exposed regardless.

Line 171. by high Ti and MS values.
Agreed. "values" will be added in the revised manuscript.

Line 174. Change: The gyttja facies is interpreted to as lacustrine sediments to: The gyttja facies is interpreted as lacustrine sediments
"to" will be deleted in the revised manuscript.

Line 216. coarse not corse
This typographical error will be corrected in the revised manuscript.

Line 226. Change: The lowest part of the T1 and T2 consists to: The lowest part of T1 and T2 Consists
"to" will be deleted in the revised manuscript.

Line 236. Change: in the two, to: in the two lakes
"lakes" will be added to the sentence in the revised manuscript. See also rephrasing of paragraph in the reply below.

Line 237-241. Rephrase
The paragraph has been rephrased for improved clarity:

"Although T2 has the same general appearance in the two lakes, there are some notable differences. These reflect that lake SAQ21-09 is much more exposed to tsunamis and has experienced much more turbulence and erosion when invaded by the tsunami compared to SAQ21-06. SAQ21-09, has a coarser sand fraction at the base and no gyttja below the base of the tsunami facies which is standing directly on top of an impenetrable substrate. The sediment core also has sand lenses from distinct waves from the same landslide-tsunami wave train, indicating that aspect to the strait and exposure to the incoming tsunamis is more important than lake height for capturing tsunamis in the lake sediments. This may also explain why there is no erosional unconformity in SAQ21-06."

Line 247-251. Rephrase
We have rephrased the sentence so it will read:

"Only one sample (from 61-62 cm depth) in core SAQ21-06 included diatoms that are marine origin. This sample was taken from the massive sand layer in the base of the T1 deposit, and included diatom species, *Cocconeis scutellum*, which is a common species in marine and brackish coastal waters (Cremer 1998; Witkowski et al., 2000; Pearce et al., 2014; Oksman et al., 2022)."

Line 278. Change: minimal faulting to: earthquake activity
"minimal active faulting" will be replaced with "minimal earthquake activity".

Line 308. Change: ice sheet to: Greenland ice sheet
It will be changed to the "Greenland Ice Sheet".

Line 309. Qeqertarsuaq is the Greenland name of Disko, not Disko Bugt
"(Qeqertarsuaq)" will be deleted in revised manuscript.

Line 358. Change: Peninsula to: peninsula
It will be changed to "peninsula" in the revised manuscript.

Line 364. Change: who produced the T1 and T2 tsunamis we to: that produced the T1 and T2 tsunami deposits we
"who" will be replaced with "that".

Line 491, 572, 642. The name of the journal was Geological Survey of Denmark and Greenland Bulletin
In all three lines "*GEUS Bulletin*" will be replaced with "*Geological Survey of Denmark and Greenland Bulletin*"

Fig. 2. Where is the triangle on the map?
It is there as the top-most symbol. Halo surrounding symbol labels will be improved for better legibility of map figure.

Line 667. cal. not ca.
Typographical error which will be corrected in revised manuscript. Note to self: also fix K-994 age in this caption.

Line 681. red or black line?
Red line signature will be made thicker in revised manuscript.

Fig. 4. I am surprised to see that you did not retrieve minerogenic sediments at the base of most sediment cores. Minerogenic sediments are usually found below organic-rich gyttja in lakes in the region. I think there could be more (older) tsunami deposits in the lakes.

It was also our expectation to find minerogenic sediment at the bottom of the cores and we did bring coring tubes for multiple, overlapping cores in each lake.

It cannot be conclusively ruled out that the sediment package is thicker than what we have retrieved from the lakes. We experienced coring tubes cracking or breaking at the end and had to make several attempts

at each site to get a good core. The procedure was to hammer until an "impenetrable substrate" was reached, identified by the hammer bouncing back. Then we measured how deep the core was into the sediment before hammering aggressively to see if we could penetrate further. If we got a cracked or broken end of the core back, then we knew that this was as deep as we could go. On a few occasions the tubes would have a fist-sized rock caught in the bottom of the tube cracking it along the length of the tube. Others would have the bottom of the tube broken to pieces as it was hammered into the impenetrable substance. Multiple coring attempts were made in each lake, except for lake SAQ21-11 which was cored in one attempt.

We used up all the tubes we brought for the field work. The ones that we successfully retrieved used the procedure described above, and they are presented in this study. The rest ended up as cracked tubes.

The above information will be added to the manuscript in a modified form, so that other researchers doing field work on the site will be aware of this. We do not wish to go into the nature of the impenetrable substrate in the manuscript, as it would be speculative if it is bedrock, boulders, or something else that prevented us from getting longer cores.

Line 701. Disko island not Disko Island
It will be changed to "island".

Line 702. Change: historical landslides in Vaigat in 1952 (Niiortuut, yellow), 2000 (Paatuut, blue), and 2021 (Assapaat, green) is also indicated. To: Historical landslides in Vaigat in 1952 (Niiortuut, yellow), 2000 (Paatuut, blue), and 2021 (Assapaat, green) are also indicated.
Agreed, it should be plural. "are" will replace "is".

Table 1. Saxicava arctica is now named Hiatella arctica.
Saxicava arctiva will be changed to Hiatella arctica in the revised manuscript.

Table 1. Marine20 assumes that the age was normalised for isotopic fractionation to a delta13C value of -25 permille. However, K-994 was not normalised for isotopic fractionation. This means that you must add 400 years to the age before you calibrate it, so you must enter 9340 years instead of 8940 years. Furthermore, Marine20 assumes a reservoir age of 550 years. If you want to use a reservoir age of 400 years, you need to subtract an extra 150 years. Using CALIB this resulted in a median probability age of 10178 cal. years BP (9582-10712 cal. years BP). The uncertainty is extremely large, maybe this age determination should be put on pension.

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 ^{13}C 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 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.

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 707. delete: All macrofossil samples in this study are aquatic bryophytes.
Agreed. It will be deleted. This information is also found in column "Material" in Table 1, so it is redundant

in the caption.

Finally, I note that the authors use a mixture of American and British spelling.
We will have a native English speaker go through the manuscript.

Response to reviewer 2 (Marc de Batiste)

Major comments (RC2):

The manuscript by Korsgaard et al reports on the analysis of sediment cores from coastal lakes in Central West Greenland and the identification of possible tsunami deposits, which are then attributed, based on earlier work and comparison with historic events, to subaerial landslides/rockfalls into the fjord.

This is a relatively straightforward and manuscript, that is certainly suitable for publication in NHEES.

I have several minor comments and suggestions that I have annotated on the pdf below, and that are mostly related to sometimes rather careless language or phrasing (so that it is not always clear to the reader what exactly is meant).

My biggest "issue" is that this manuscript relies quite strongly on Svennevig et al. (2023b). This paper discusses the 9 landslide deposits identified in the fjord and derives volumes, etc. from it. However, this paper is still not published… so I had no means of double-checking some of the statements and looking at some of the features discussed here.

Marc De Batist

Author response (AC):

We are pleased with the positive review. The authors would like to thank you for taking your time to review the manuscript and provide very constructive comments and suggestions, which helped improve the manuscript.

We agree that there are several examples of careless language and welcome that these are pointed out so that imprecise language and ambiguities can be removed or cleared up.

We agree to all major and minor comments and suggestions and will change the manuscript accordingly. One change that will also affect the title of the manuscript, is that "giant" will be replaced with "very large" when characterizing tsunami magnitude. See our comments to the minor reviewer comments for further detail.

Indeed, the manuscript in review here cannot stand on its own and relies on unpublished data from Svennevig et al. (2023b) which is now accepted for publication with no changes of relevance to the manuscript under review here in NHESS. It is virtually the same manuscript which we forwarded a copy of to you.

Minor comments

The comments from reviewer 2 (RC2) use the black font color. Our comments (AC) are using the blue font color.

Line 14. How much is giga?

Giga refers to the common prefix used on the metric scale indicating that the volume is in the 10^9 scale (m^3). 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 m^3), tsunamigenic landslides; however, their ages are unknown.

Line 19. How many (orders of magnitude difference) exactly?

It will be changed to "one to two orders of magnitude."

Line 22. How much is giant? And how does giant compare to giga?

We will change “giant” to “very large” in the abstract and the manuscript title. The change is incorporated into the response to your comments for line 23. There is no scientifically agreed on scale based on run-up heights or landslide volumes. Large, very large, giant – are subjective, but some (i.e., large, very large) are used to signify events that are very different from the normal. We have decided to discard the use of “giant” and use “very large” instead, so that the manuscript, including the title, are less attention catching, but still essentially non-quantitative.

Line 23. Shouldn't this be two separate sentences? I assume the rationale is that the two tsunami deposits must be linked to the two recent most landslides and that the older landslides are therefore pre-lake?

You are correct in your assumption of the rationale. We have made the sentence(s) clearer: “While we found deposits from two very large tsunamis in the lake sediments, landforms from at least nine giga-scale landslides are present on the seafloor of Vaigat. We infer that these deposits represent the two most recent tsunamis and older very large tsunamis must have happened between the last deglaciation and the oldest sediment in the lakes (c. 10 to 8.5 cal. ka BP).”

Line 28. Perhaps also good to mention the Aysen fjord event in Chile, in 2007... which was captured on photo (https://elpais.com/elpais/2007/04/23/album/1177310269_910215.html)?

Sepulveda et al., 2010 will be added to the sentence, which will be as follows:

“Landslide-tsunamis are among the most devastating natural disasters in fjord settings with several recent examples from Norway and Alaska (e.g., Blikra et al., 2006; Sepulveda et al., 2010; Higman et al., 2018; Svennevig et al., 2020, 2023a).”

In fact, the reference Sepulveda et al (2010) also appear on a similar context in lines 324-325 in the manuscript under review:

“Subaerial marine terminating landslides have the potential to produce very large run-up tsunamis in confined fjord settings (e.g., Dahl-Jensen et al., 2004; Sepulveda et al., 2010; Higman et al., 2018; Svennevig et al., 2020).”

Line 49. "underwater landslide deposits" Because there are many more landslides indicated on the map, most being subaerial ones. The nine you refer to are the ones visible in the bathymetry...

Agreed. It will be much more precise. The revised sentence will read: “In the Vaigat strait, nine large underwater landslide deposits were mapped.”

Line 50. This is a bit confusing... the fjord bathymetry is smooth, but that is because it has been filled with sediments and not because it is glacially carved (which is what you seem to imply by phrasing it like this).

Agreed. This sentence will be changed to: “They are recognizable by their hummocky and blocky topography in the otherwise smooth U-shaped glacial trough with a Holocene sediment cover comprising the bottom the Vaigat strait (Fig. 1.)”

Line 51. Do you have seismic data? If not, you do not really have an idea of the volume involved, do you? Only of the surface that is covered by the deposit...

...and…

Line 53. Ah you do... perhaps good to mention this first! [Refers to Line 51]

We have rephrased/switched sentences at the end of the paragraph so it will read:

“Seismic profiles from Vaigat show the presence of localized chaotic accumulations of sediment interpreted as either old submarine slides or submarine aggradations from subaerial landslides, with a general thickness of 50-100 m that can locally exceed 200 m (Marcussen et al., 2001; Pedersen et al., 2002). Based on long runout, large volume, and giant displaced blocks Svennevig et al. (2023b) suggests that these rock avalanches had a significant tsunamigenic potential, which should leave onshore evidence of tsunamis. However, nothing is known of when the landslides occurred after deglaciation or magnitude of the tsunamis they could have generated.”

Line 63. Do you mean specifically in lakes? Or in general?

Yes, it means in general. We will rephrase to:

“Evidence of tsunamis is in general rare in Greenland and has only been encountered by coincidence three times in relation to paleoclimatic studies or relative sea level reconstructions using lake sediment records.”

Line 80. I probably don't quite understand the sentence... but it was more submerged then because of glacial isostatic subsidence, no? Later on, uplift moved the land upward and made it less susceptible to tsunami inundation, no?

You are completely correct. The revised and much improved sentence will read:

“Weidick & Bennike (2007, Fig. 22) provides a minimum age of the last deglaciation of 10 cal. ka BP from dating marine shells at 70 m elevation today (Fig. 2). Thus, susceptibility of the lakes to inundation from tsunami waves would have been more pronounced in the Early Holocene considering Saqqaqdalen would have been submerged more than 70 m due to glacial isostatic subsidence, and in the mid-Holocene the glacial isostatic subsidence would be c. 6-25 m (5.8-7.6 cal. ka BP) relative to today.”

Line 84. Garde & Steenfelt, 1994?

Agreed. Will be changed accordingly.

Line 95. Sorry... I am not entirely sure what you mean with this? Do you mean in terms of tidal effects?

Yes. This will be specified in the revised manuscript.

Line 97. I think you never even mention the water depth in the fjord offshore Saqqaq... although that would be an interesting metric to know. It is indicated -very roughly- on Figure 8... but could be mentioned in the text.

Agreed. It would not be the best place to add it at line 97. It will be added to line 73 where it will read: "The Vaigat strait is generally between 500 and 600 m deep with depths of up to 620 m south of Saqqaq, except where landslide deposits and dykes and sills (Paleocene intrusions) are present."

Line 112. Can you provide a few words of motivation as to WHY exactly these elements/ratio's in this specific setting?

We will elaborate in why we use Ti and Ca/Fe in this setting. The single sentence has been expanded to a several paragraphs:

"In this study we use the element Titanium (Ti) as a proxy for minerogenic input to the lake (e.g., tsunami and glaciofluvial deposits) and the element ratio Ca/Fe as a proxy for marine influence in the sediment i.e., in tsunami deposits.

Titanium is suitable to differentiate between marine and glaciofluvial deposits with a high minerogenic content, and lacustrine deposits with low minerogenic content. As Titanium is a common constituent of rocks such as gneisses or schists, it primarily indicates a terrigenous continental source (Rothwell & Croudace, 2015). In our setting, high Ti values are interpreted as minerogenic deposits flushed into the lake from either shoreline or onshore surroundings as the tsunami wave loses energy and the water runs back to the sea. The Ca/Fe ratio is used as a proxy for input of biogenic carbonates of marine origin (Ca) relative to detrital clay (Fe) of terrigenous origin into the lake and can be used to indicate tsunami deposits (Chagué-Goff et al., 2017)."

Line 117. Not in reference list

It is a typographical error. It will be changed to "Long et al. (2015)", which can be found in the reference list.

Line 134. Bronk Ramsey

Agreed. Will be changed accordingly.

Line 155. Figure 4 shows that only cores 06 and 09 contain tsunami deposits... that's only 2 cores, not 4? Or am I missing something?

This is an issue also raised by a different reviewer (Bondevik) and will be revised. The revised manuscript will throughout emphasize that we found tsunami sediments in two lakes, and we use the data from the two other lake cores to constrain timing and run-up height of the tsunami events. Also, consequently, the two lake cores with data of little relevance will be mentioned but not presented in detail.

Line 184. Euh... I can see on the figure that it is slightly over 40 cm thick in core 09... but it appears to be much thinner (30-something cm) in core 06. This sentence seems incomplete...

Agreed, the data are presented inconsistently. The text is correct, and Fig. 4 shows the layer correctly, but there is an error in Fig. 5 and Table 1. Sample Ua-74363 is inside the tsunami deposit, not above (an interpretation error from the notes at the time of sampling). This will changed so all data are consistent.

Line 195. Did you notice any preferential orientation or imbrication of the clasts, which could point to a flow direction? And if so, did you observed different flow directions at different levels in the deposit? Such fabric analysis could help fine-tuning your reconstruction of the succession of events (separate waves?) that caused the deposit...

We did not notice any preferential orientation or imbrication. I think X-ray tomography may be the only option to perform such an analysis and this was not planned (and not available) as part of the analysis.

Line 232. Do you now mean T1 and T2?????

Agreed, this is confusing and will be changed to:

"We interpret the deposits in the two cores as tsunami deposits. While identification of the tsunami deposits is based on visual description of sediments and structures, and sedimentological proxies, the correlation of the units between the two cores with tsunami deposits is primarily based on the laminated gyttja separating the two tsunami units T1 and T2 in core SAQ21-06, constrained by the age control. This correlation is supported by the rough match of visual appearance and sedimentological proxies of T1 and T2 in the two cores. Since we did not map the entire lake stratigraphy, we cannot exclude the possibility that the c. 42 cm unit of laminated gyttja could be a large rip-up clast and T1 and T2 may be one tsunami event."

Line 261. you mean of older organic matter then? As there are no carbonates in the catchment...

Yes, it means older organic matter. The interpretation of dates will be revised so the that the type of dated material is taken better into account. Line 261 will be updated to and discuss older organic carbon as according to the updated interpretation found below.

Bondevik et al., (1997b) found that radiocarbon dates sampled from just above the tsunami facies commonly show older dates than samples from inside the tsunami facies. The redeposition of older sediments and carbon can occur hundreds of years after the tsunami facies was deposited. This has implications specifically for the bulk sediment sample just above the T1 tsunami facies in core SAQ21-09, which has an age 6.7 cal. ka BP. This date would be significantly affected by redeposition of organic carbon post tsunami and show an age

that is significantly older than the time it was deposited. In core SAQ21-06 the youngest date in the core is sampled inside the T1 tsunami facies (as shown in Fig. 4). It is an aquatic moss and provides a maximum age of 5.8 cal. ka BP for the most recent tsunami deposit in the T1 unit, i.e., if the unit represents one landslide-tsunami event then the whole the unit would have this maximum age. If the T1 unit represents multiple landslide-tsunami events, then it would show the maximum age of the youngest event.

Bondevik, S., Svendsen, J. I., Johnsen, G., Mangerud, J. & Kaland, P. E.: The Storegga tsunami along the Norwegian coast, its age and runup. Boreas, Vol. 26, pp. 29-53. OSIO. ISSN 0300-9483, 1997b.

Marty, J. and Myrbo, A.: Radiocarbon dating suitability of aquatic plant macrofossils, J Paleolimnol, 52:435–443, DOI 10.1007/s10933-014-9796-0, 2014.

Line 267. In theory there could be 3rd... and that is that Lake 07 was not inundated by the tsunami (or that it was but recorded no trace of it). I agree that this is highly unlikely, but perhaps you should nevertheless mention it?

Bondevik pointed out that dates aquatic mosses are very sensitive to reservoir effects, and it is an aquatic moss providing the date at the bottom of core SAQ21-07. It should be treated as a maximum age and it is possible that it has an age that is 1.5 ka too old, explaining the absence of tsunami traces. If this is the case, and it seems likely, the dates constraining T1 provide a bracket of 5.8-7.2 cal. ka BP in which one or more tsunami could have occurred, but would be present in core 07. The tsunami dated in lake Tasiusarsuit was found to have an age of 6.0 cal. ka BP (Long et al., 2015) which falls within this bracket.

Line 278. So... essentially you now ruled out other possible mechanisms to create the event deposits you described above... and you galvanize your interpretation that they indeed represent tsunamis. Perhaps you could add that in a short sentence?

Agreed. We will add the sentence:

“Lake inundation by tsunami waves is the remaining depositional process that can explain the presence of sediments with the stratigraphical signatures of the deposits identified here.”

Line 309 Bennike

This typographical error will be fixed.

Line 312. Is that today's water depth at the mouth of the fjords, or was that the water depth 7-8 ka ago?

The statement will be revised to reflect water depth at 8 cal. ka BP. It will read: “The maximum water depth at the mouths of the two fjords would have been 280 (Kangia) and 380 m (Torsukattak) at 8 cal. ka BP restricting the vertical height of the icebergs to less than 400 m and consequently a theoretical upper limit of the open-water tsunami wave height to c. 4 m.”

Line 319. Winds

This typographical error will be fixed.

Line 329. In the abstract they are called "giga". Normally, using terminology like "giga" and "giant" sounds good (and may be OK in a title to catch the attention), but it does not say very much. It is highly subjective and

non-quantitative. I have now come to page 11 of this manuscript and I still have no idea how big they really are!

Agreed. Giga refers to the common prefix used on the metric scale indicating that the volume is in the 109 scale (m3). 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), potentially tsunamigenic landslides; however, their ages are unknown.

We will change “giant” to “very large” in the abstract and the manuscript title. The change is incorporated into the response to your comments for line 23. There is no scientifically agreed on scale based on run-up heights or landslide volumes. Large, very large, giant – are subjective, but some (i.e., large, very large) are used to signify events that are very different from the normal. We have decided to discard the use of “giant” and use “very large” instead, so that the manuscript, including the title, are less attention catching, but still essentially non-quantitative.

Line 330. "et al." implies that the subject of this sentence is plural.

Agree. “argues” will be changed to “argue”.

Line 335. Landslides

This typographical error will be fixed.

Line 339. Can you briefly explain what input it uses and what output it generates exactly? [SPLASH eqn.]

Currently the manuscript reads: “The SPLASH equation is based on a limited number of landslide tsunamis events, and none of them are of the magnitude we infer in the present work; however, it provides an estimate of the magnitude without using more advanced tsunami models which is beyond the scope of the present paper.”

In the revised manuscript the SPLASH equation will be described in more detail:

“The SPLASH equation is a parametrization of a power-law relationship between landslide volume, distance, and vertical run-up of the tsunami wave. Parameters were found by least-squares fitting data from a limited number of large rock slope failures and the observed run-up at different distances from each landslide. It provides a semi-empirical prediction of run-up height from intended for preliminary regional studies. We use it here since it provides an estimate of the magnitude without using more advanced tsunami models which is beyond the scope of the present paper.”

Line 460. Not called in text?

This excess reference will be deleted in revised manuscript.

Line 475. Not called in text?

This excess reference will be deleted in revised manuscript.

Line 502. 2022?

This typographical error will be fixed.

Line 504. Bronk Ramsey

Agreed. Will be changed accordingly.

Line 511. Not called in text?

This excess reference will be deleted in revised manuscript.

Line 519. In full?

The authors list will be written out in full in the revised manuscript.

Line 586. I think this should be Bronk Ramsey (listed under B). This is also what the journal itself suggests: "Bronk Ramsey, C. (2009). Bayesian Analysis of Radiocarbon Dates. Radiocarbon, 51(1), 337-360. doi:10.1017/S0033822200033865"

Agreed. Will be changed accordingly.

Line 597. Not called in text?

This reference is now used in the text, see your comments to line 112.

Line 624. 2023a?

Agreed. Will be changed accordingly.

Line 627. 2023b?

Agreed. Will be changed accordingly.

Line 650. No depth values on the contour lines!

Depth values will be added to contour lines on revised figure.

Line 695. I do not understand where this comes from? Fig 5 clearly shows that you do have sediment younger than 6 ka BP!

Agreed. It is nonsensical. It will read: “There is no sediment older than 8.5 cal. ka BP in our core record, and after c. 6 cal. ka BP a tsunami would have to have a minimum run-up of up to 19 m to reach a lake.”

Line 707. Bronk Ramsey.

Agreed. Will be changed accordingly.

Response to reviewer 3 (Stein Bondevik)

RC3: General comments (major RC3 comments)

I got very excited when I was asked to review this paper about tsunami deposits in lakes in Greenland. The photos of two of the cores show very nice examples of tsunami deposits in lakes, its facies are similar to the Storegga tsunami facies in coastal lakes along the Norwegian coast. However, after reading through the manuscript I ended up being disappointed. My main criticism is the field work. The work in the field has not been a thorough investigation – only one core from each lake basin!! The possible tsunami deposits found in two of the lakes are interesting and I think deserves a better study. I am also not convinced that there are two tsunami layers as the authors claim – it might just be that the two layers belong to one event, but this is difficult to solve without better mapping of the deposits in the field.

AC: Thank you for thorough review which has helped improve the manuscript substantially. Our response to your review is in blue font.

We agree that the study would have benefitted from more comprehensive fieldwork and would have made it more conclusive. Working with the available data, we will revise the manuscript to make these weaknesses and implications apparent to the reader, and in this way address the concerns of the reviewer.

Our response in point-by-point:

1) Field work/single core per lake.
   It is normal procedure in Greenland lake coring is to bring a single core (or site) per lake back to the lab for detailed analysis and fieldwork equipment and logistics are usually “light”. In addition to this, reconnaissance using a Russian corer is often, but not always, done in the field. Mapping of the complete lake stratigraphy (with transect) seems to be rarer than a reconnaissance. In a lone example, Wagner et al. (2006), a single core is used to interpret a layer as from the Storegga tsunami.

   That we did not do this means we are not able to test one of the criteria for identification of a tsunami layer. We do meet other criteria and we also eliminate other processes which could create a stratigraphy which could be mistaken for a tsunami layer. In addition, we point to a likely source of tsunami waves – the very large Holocene rock avalanche deposits found at the bottom of the Vaigat strait. We consider this strong but not perfect evidence of tsunami invasion of the Saqqaq lakes.

2) One or two tsunami layers.
   We agree with the reviewer, that if we had mapped the lake stratigraphy, it would most likely have been possible to be conclusive if there are one or two tsunami layers in the lakes. However, we maintain that the most likely explanation is that there are two layers, T1 and T2. We base this on the available data; the apparent limited erosion at the base of T2; the 14C ages below, above and inside

the T2 tsunami layer are quite close; and although it is possible that a 42 cm thick monolithic block of horizontally laminated gyttja could be a rip-up clast, it seems more likely to represent the time between T1 and T2.

We will address this concern by making the reader aware of the real possibility that there is only one single tsunami layer in the revised manuscript.

3) Radiocarbon dating / What is the age of the T1 tsunami layer?
We agree with the reviewer's concern, and he has been extremely helpful on this subject, specifically pointing out the issues with using mosses (aquatic bryophytes) and bulk sediments for dating, and how type of dated material and stratigraphical context is important. This has led to a revision of the age of T1, which makes the dates and facies consistent across the cores. It is interpreted as a unit representing one or more tsunamis occurring in the 5.8 to 7.2 cal. ka time bracket. As the reviewer think, the tsunami layers found by Long et al. (2015) in a lake in south Disko Bugt falls within this bracket and could be related.

4) Fieldwork/Impenetrable substrate/short cores.
We expected to get much longer cores. However, we were unable to penetrate what we during fieldwork subjectively judged to be bedrock or a boulder layer. At the same time, we had an issue with coring tubes that broke at the end and sometimes cracked, so there is a limitation as to how aggressively we could hammer the cores down. This leaves open the possibility that we could have penetrated this layer with other equipment, but we were unable to penetrate. It may be bedrock, a boulder layer or something else that prevented us from coring deeper.

We will add this information to the description of the field work. Here, we will also include that we did not do a core transect for lake stratigraphy and conclusions consequently are open to other interpretations.

5) Reporting evidence on six lakes when tsunami layers are only found in two lakes.
Agreed. The revised abstract and manuscript will throughout reflect that we found tsunami deposits in two lakes and used another two to constrain run-up magnitude and timing. The two cores that do not have relevant information, being too short, will be mentioned briefly but not presented in detail.

More author comments (AC) to major comments from the reviewer (RC3) below. Author response to minor comments and suggestions from the reviewer can be found in the file attachment.

Wagner, B., Bennike, O., Klug, M., and Cremer, H.: First indication of Storegga tsunami deposits from East Greenland. J. of Quat. Sci. 22, 321-325. https://doi.org/10.1002/jqs.1064, 2006.

RC3: Fieldwork:

The manuscript presents only one core from each lake basin. Tsunami deposits change a lot over short distances, particularly in a lake basin. To document lateral changes the authors need to map the deposits across the lake basins. Ideally, in order to conclude that this is a tsunami deposit they should have shown that the sediments really were deposited into the lake from the seaside in terms of lateral changes in grain size, thickness and erosion. With only one core this is impossible. There are plenty of examples of lake basins

studies using multiple cores, also in Greenland, as done for instance by Long et al., 1999 – a beautiful paper in that respect is that by Long et al., 2015. To document tsunami deposits preserved in a lake stratigraphy you need many cores.

AC: The field work had been planned to take multiple, overlapping cores in each lake, but problems with the core tubes limited this to one per lake. (Two cores were obtained from lake SAQ21-09 and is available for download with the other core data in the data repository. It is not presented in the manuscript, as it contains the same information).

In the fieldwork section and results interpretation in the revised manuscript we will explain that we did not do a core transect and make sure the reader understands the implications thereof.

RC3: The only core is, since it is the only one core, too thin – 60 mm in diameter. This diameter is sometimes too small to find reliable material for radiocarbon dating. A thicker core, or instead several cores of the same unit, would yield a larger volume for each cm core depth to search for terrestrial plant remains for radiocarbon dating.

AC: We agree that multiple cores (or core tubes with larger diameter) would have provided more sediment volume that would in turn have increased the chances of finding dateable terrestrial plant remains. The prerequisite is that there are terrestrial macrofossils present in the sediment - if there is no terrestrial material in the layers, then greater sample volume does not help.

Below is a brief, and certainly not complete, survey of existing literature which shows some of the challenges with finding the optimal terrestrial macrofossil for dating in Greenland.

- Long et al, (1999) and Long et al (2015) exclusively rely on bulk sediment sampling (using 50 mm piston corer taking the part of a core home for lab work, and 40 mm Russian corer for lake stratigraphy).
- Bennike (2000) uses 15 terrestrial plant fossils, 5 limnic plant fossil, and 3 bulk sediment samples to create age models for four lakes. (60 mm piston corer for lab samples, reconnaissance using 50 mm Russian corer).
- Larsen et al. (2017) uses 8 bulk sediment, 6 terrestrial macrofossil, and 1 mixed terrestrial/aquatic mixed for creating age models for three lakes (single core per lake, probably 60 mm piston or percussion).
- Philipps et al. (2017) uses 15 aquatic macrofossil and 1 bulk sediment sample to create age-depth models for two of three lakes in the central Uummannaq Fjord system (the latter dated the contact). (Single core per lake, coring diameter not specified). Core lengths 82, 109 and 113 cm.

In general, the work is done with a single core per lake taken home for lab work, and dependent on the type of investigation, lake stratigraphy or reconnaissance is also done with a Russian corer.

Use of bulk sediment and aquatic macrofossil is widespread and the problems well-known. In the literature above these problems are not introduced to the reader. The reason may have been that since researchers are aware of these problems, and they know the implications are when they see a table of radiocarbon dates where bulk sediment or aquatic samples are present.

Larsen et al. (2022) captures this in the discussion of the use of bulk sediment dating:

“To test if ages of bulk sediment samples are reliable, we compared ages of macrofossils and bulk samples from two intervals in Smaragd Sø and found discrepancies of several centuries (Table 1). Though these are

clearly significant, we have used these bulk ages in the study out of necessity. They should conservatively be regarded as the maximum limiting ages for those deposits."

The problems with aquatic macrofossil and bulk sediment are well-known, but if nothing else is found for dating, then with its limitations it can be used out of necessity. Thus, use of non-optimal dating material is frequent, and in some cases comprises all dates in the study (e.g., Long et al., 1999, 2015).

We have revised our interpretation of our dates elsewhere and refer to this for further details.

Bennike, O: Palaeoecological studies of Holocene lake sediments from west Greenland, Palaeogeography, Palaeoclimatology, Palaeoecology 155, 285–304, 2000.

Larsen, N.K., Astrid Strunk, Laura B. Levy, Jesper Olsen, Anders Bjørk, Torben L. Lauridsen, Erik Jeppesen, Thomas A. Davidson: Strong altitudinal control on the response of local glaciers to Holocene climate change in southwest Greenland, Quaternary Science Reviews 168, 2017.

Larsen, N.K., Siggaard-Andersen, M.L., Bjørk, A.A., Kjeldsen, K.K., Ruter, A., Korsgaard, N.J. & Kjær, K.H.: Holocene ice margin variations of the Greenland Ice Sheet and local glaciers around Sermilik Fjord, southeast Greenland. Quat. Int., 2021. doi.org/10.1016/j.quaint.2021.06.001, 2022.

Long, A.J., Roberts, D.H., and Wright, M.R.: Isolation basin stratigraphy and Holocene relative sea-level change on Arveprinsen Ejland, Disko Bugt, West Greenland. J. Quat. Sci. 14 (4) 323–345, doi.org/10.1002/(SICI)1099-1417(199907)14:4%3C323::AID-JQS442%3E3.0.CO;2-0, 1999.

Long, A.J., Szczuciński, W., and Lawrence. T.: Sedimentary evidence for a mid-Holocene iceberg-generated tsunami in a coastal lake, west Greenland. Arktos 1:6, https://doi.org/10.1007/s41063-015-0007-7, 2015.

Philipps, W., Jason P. Briner, Ole Bennike, Avriel Schweinsberg, Casey Beel, Nathaniel Lifton: Earliest Holocene deglaciation of the central Uummannaq Fjord system, West Greenland, Boreas, 2017. https://doi.org/10.1111/bor.12270

RC3: The cores are short and does not penetrate the entire stratigraphy. The authors say that "All core lengths reflect the thickness of the sediment package until the corer encountered bedrock or impenetrable substrate". According to the sea level curve and deglaciation dates the lakes should contain marine deposits in their lower parts. The longest cores reach back to 8000 years BP. Also, I wonder, are the top sediments present in the cores? The two cores showing tsunami facies has radiocarbon ages of 5000-6000 years only 20-30 cm below the core top. Has nothing or very little of sediments been deposited in these two lakes since the mid-Holocene?

AC: We will add to the revised manuscript that it is a possibility that there are more sediments in the lakes than what we are presenting, so that this possibility remains open. We cannot know what prevented us from coring deeper and it is a speculative guess.

Lack of top sediment. The sediment dates are indeed quite old close to the top in cores 06 and 09. The best explanation is that we misjudged when we hit the lake bottom and opened the piston too late. We do have the complete core from lake 07 which has a complete top with dates and no trace of tsunami in it.

RC3: Only two of the six lake basins have evidence of tsunami deposits. But the authors write in the abstract: "Here we report evidence from six coastal lakes …..evidence of at least two tsunami events." One of the six lakes has only a 9 cm core, another has 54 cm long core with a radiocarbon age of 2.8 ka BP. These two basins do not contain any sediments relevant for this study and should not be used to increase the number of sites. It doesn't look good…

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

RC3: Radiocarbon ages:

The study has not used the best material for radiocarbon dating. Both bulk organic sediments and aquatic moss is not material recommended for reliable radiocarbon ages. Several studies have shown that aquatic bryophytes yield too old ages because the plants take up their CO2 from dissolved inorganic carbon from the lake water. Even lakes without carbonate rocks in their drainage area could have a reservoir effect – that also could vary quite a lot through time (see many publications about this problem, for instance Marty and Myrbo (2014)).

The other material dated in this study is bulk gyttja. The same problem could arise here, with a lake reservoir age, that sometimes yield too old ages, possibly several hundred years older than terrestrial plant fragments.

AC: Bondevik et al., (1997b) found that radiocarbon dates sampled from just above the tsunami facies commonly show older dates than samples from inside the tsunami facies. The redeposition of older sediments and carbon can occur hundreds of years after the tsunami facies was deposited. This has implications specifically for the bulk sediment sample just above the T1 tsunami facies in core SAQ21-09, which has an age 6.7 cal. ka BP. This date would be significantly affected by redeposition of organic carbon post tsunami and show an age that is significantly older than the time it was deposited. In core SAQ21-06 the youngest date in the core is sampled inside the T1 tsunami facies (as shown in Fig. 4). It is an aquatic moss and provides a maximum age of 5.8 cal. ka BP for the most recent tsunami deposit in the T1 unit, i.e., if the unit represents one landslide-tsunami event then the whole the unit would have this maximum age. If the T1 unit represents multiple landslide-tsunami events, then it would show the maximum age of the youngest event.

Reviewer De Batist (RC2) suggests that a third (i.e., younger) tsunami could explain the young age in top of T1 but could not reconcile this with the fact that there is no trace of tsunami in core SAQ21-07. We also used the bottom date of 7.3 cal. ka BP of core SAQ21-07 to constrain the timing of T1, exactly because there is no trace of tsunami, and it is very unlikely that a tsunami would leave no trace.

Reviewer Bondevik (RC3) points out that aquatic moss when used for dating may show an older age from reservoir effects as the moss could have sourced its carbon from other old organic matter in the water column or if there is infrequent mixing of the lake (Marty & Myrbo, 2014). We do not know the exact species of moss, but it is likely that since the moss obtains its CO2 from the lake water and not the atmosphere, that it could an age considerably larger than the actual age of the deposit. An age of the bottom of core SAQ21-07 that is 1.5 ka too old would explain why there is no trace of a younger tsunami.

This interpretation of the age control makes the 14C dates and facies consistent across the cores.

Our interpretation is that the T1 unit represents at least one tsunami event which occurred in a time bracket of 5.8 and 7.2 cal. ka BP. Reviewer Bondevik thinks that it is possible that T1 represents the tsunami layers found by Long et al. (2015) in southern Disko Bugt. These tsunami layers date to 6.0 cal. ka BP and

interestingly fall inside that bracket, so it is a distinct possibility which we will include in the discussion of the Long et al. (2015) paper.

Bondevik, S., Svendsen, J. I., Johnsen, G., Mangerud, J. & Kaland, P. E.: The Storegga tsunami along the Norwegian coast, its age and runup. Boreas, Vol. 26, pp. 29-53. OSIO. ISSN 0300-9483, 1997b.

Marty, J. and Myrbo, A.: Radiocarbon dating suitability of aquatic plant macrofossils, J Paleolimnol, 52:435–443, DOI 10.1007/s10933-014-9796-0, 2014.

RC3: Other comments:

The reader should be presented with a detailed map of each lake including the drainage area together with the location of the core.

AC: We would prefer not to include an additional figure with detailed maps as we think it would add little to the study. Contextual location of coring sites and sills is presented in lesser detail in Fig 2. Table coordinates for coring sites coordinates lake dimensions, lake depth at coring site and sill heights is shown in Table 2, which also shows overall lake dimensions such as length, width, and area.

We will add a kmz/kml file to the data repository with coring site and sill coordinates, allowing the user to inspect the locations in Google Earth by a double click on the file, or open it in a GIS software.

RC3: What is the age of the T1 tsunami layer? The original interpretation of the tsunami unit T1 includes sediments called “Organic detritus with clasts” (Fig. 4). However, a bryophyte age within these sediments gives an age of 5.8 ka yr BP. This means that the T1 layer is as young or younger than 5.8 ka BP. The authors claim that the T1 layer should be present in the stratigraphy in lake basin 19 m elevation, that is dated to begin at 7300 yr BP, but it is not. The way the authors solve this discrepancy is to place the upper boundary of the tsunami layer T1 at the top of the organic conglomerate layer, and not include the layer of organic detritus with clasts above with the 5.8 ka yr date (Fig. 5). How should we interpret the clasts in the organic detritus layer if not deposited by a tsunami? Could we exclude a tsunami deposit in a lake basin stratigraphy based on ONE SINGLE CORE? I think it is possible that the T1 layer represents the same tsunami event as Long et al., 2015 discovered (around 6.0 ka BP) in the southern part of Disko Bugt.

AC: Agreed, the 5.8 cal. ka date is in the tsunami facies. The discrepancy between the blue marking of tsunami facies in Figs. 4 & 5 will be fixed so that Fig. 5 will be changed according to the markings in Fig. 4. This means that the organic detritus with clasts in Fig 5. will be marked as tsunami facies as it is in Fig. 4. Then it will also be consistent with the text in the manuscript.

See also how the interpretation of the radiocarbon ages has been revised in previous response to your comments on radiocarbon ages and its implication on how core SAQ21-07 was used prior to revision to constrain T1 to an age of 7.3 cal ka BP. T1 is now assigned a bracket of 5.8-7.2 cal. ka BP.

RC3: Do the sediments really show two separate tsunami events, or could it be only one event? In core SAQ21-09 the T1 tsunami layer is resting directly on the T2 layer. However, in SAQ21-06 there is about 40 cm of laminated gyttja in between. But is this really original lake sediments and not a rip-up clasts? This problem could have been solved by several cores mapping the layers better in the field. I think it is quite possible that T1 and T2 could reflect one tsunami event.

AC: Figs. 4 & 5 show that in the core SAQ21-06 there is c. 42 cm of laminated gyttja between tsunami units T1 and T2. We are convinced from this that we have at least to distinct tsunami events in the two lakes with sediments and this is based on that the 42 cm of laminated gyttja. Erosion at the base of the tsunami layer seem to be small and radiocarbon dates below, inside, and above the tsunami layer are close.

We will change line 232 to make the possibility that it is a rip-up clast clear to the reader (mainly last sentence):

"We interpret the deposits in the two cores as tsunami deposits. While identification of the tsunami deposits is based on visual description of sediments and structures, and sedimentological proxies, the correlation of the units between the two cores with tsunami deposits is primarily based on the laminated gyttja separating the two tsunami units T1 and T2 in core SAQ21-06, constrained by the age control. This correlation is supported by the rough match of visual appearance and sedimentological proxies of T1 and T2 in the two cores. Since we did not map the entire lake stratigraphy, we cannot exclude the possibility that the c. 42 cm unit of laminated gyttja could be a large rip-up clast and T1 and T2 may be one tsunami event."

RC3: The comparison to the basins studied by Long et al., 1999 at Arveprinsen and south in the Disko Bugt (Long et al., 2015) is interesting, but depends on the age(s) being correct. See comments above.

AC: With our revised interpretation of the dates the story has changed and the tsunami layer in the south Disko Bugt (Long et al., 2015) now falls inside the age bracket of the T1 layer at Saqqaq.

RC3: See also detailed comments directly on the attached pdf, both text and figures.

AC: Our responses to your detailed comments are available in the attached pdf file.

RC: Conclusion:

Should this manuscript be published? I wouldn't publish it, mainly because of only one core from each lake basin. I strongly advise the authors to return to the lake basins and map the deposits using for instance a Russian peat corer. It is important to know if these sediments were deposited from the seaside since they do not contain any marine fossils. Is it really evidence here for two separate tsunami events? The radiocarbon dates should also be tested, are the aquatic moss ages OK? If yes, then the T1 tsunami layer could be younger or as young as 5.8 ka BP – that changes the story - so it is also important to check out the aquatic moss ages.

AC: We would very much like to revisit the lakes at Saqqaq and elsewhere and get much better data, also more distal (to the landslides) sites, but we have work with the data we got home. We believe that with the caveats mentioned above, they have sufficient quality to allow for interpretation and contextualization.

Specific points summarized in this conclusion is addressed elsewhere.

RC3: References:

Long et al., 1999: Isolation basin stratigraphy and Holocene ….Arveprinsen…West Greenland. JQS 14, 323-345.

Long et al., 2015: Sedimentary evidence for a mid-Holocene…tsunami…West Greenland, Arktos 1:6.

Marty, J and Myrbo, A (2014): Radiocarbon dating suitability of aquatic plant macrofossils, Journal of Paleolimnology volume 52, pages 435–443 (2014).

1 April 2023

Stein Bondevik

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.

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]