The Cryosphere Discussions

The Cryosphere Discuss., author comment AC1
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Reply on RC1

James A. Smith et al.

Author comment on "Holocene history of the 79°☐N ice shelf reconstructed from epishelf lake and uplifted glaciomarine sediments" by James A. Smith et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2022-173-AC1, 2023

Reviewer 1.

Review for Smith et al., "Holocene history of 79°N ice shelf reconstructed from epishelf lake and uplifted glacimarine sediments." In Discussion at The Cryosphere. Reviewed October 2022.

Smith and colleagues present a new multi-proxy data set on epishelf lake sediment cores and nearby outcrops and discuss implications for the past and future stability of the 79°N ice shelf in Northeast Greenland. Where possible, they present new radiocarbon dates on marine foraminifera and molluscs that constrain the timing of sea water in the epishelf lake basin that is interpreted to reflect times of a retreated or absent 79°N ice shelf.

I enjoyed reading this paper and was quite excited (and convinced) of their main finding—that the 79°N ice shelf was retreated or gone for thousands of years in the Holocene--on their chronology, between 8.5 and 4.4 ka. Zooming out, it is fascinating that in the last few years we have learned that two other modern North Greenland ice shelves were gone for thousands of years in the Holocene, with the Petermann Ice Shelf gone from ~7.0 – 2.2 ka (Reilly et al., 2019) and the Ryder Ice Shelf from ~6.3 – 3.6 ka (O'Regan et al., 2021). Thus, it is likely that there were about 2 thousand years in the middle Holocene where there were no (or significantly retreated) major floating ice shelves in North Greenland! Whoa! This makes for an interesting natural laboratory, as it is well documented that Arctic atmospheric, oceanic, and sea ice forcing where quite different in the early and middle Holocene. Accordingly, I couldn't agree more with the authors statement, "In this context there is an urgent requirement for numerical modeling, utilizing the timing of changes presented in this study together with information on ocean and atmospheric forcing, to investigate the response of NEGIS to retreat or loss of the ice shelf."

The paper is well written and well-illustrated. The observations are novel and from particularly valuable and rare types of samples. I think this paper will be suitable for publication in The Cryosphere and I only have a few minor comments.

Could you discuss how changes in relative sea level might influence and/or complicate your signal? While it is likely difficult to constrain, the amount of sea water that can enter the lake is likely a function of both the ice shelf draft and the sill depth of the epishelf lake. Because the sill depth was deeper in the early Holocene, is it possible that it would have been easier for sea water to enter the epishelf lake basin at that time? Could this complicate your interpretation—why or why not? If the current halocline is 145 m and the core site is 90 m, would tens of meters of RSL be significant when discussing the early Holocene?

Our findings show that relative sea-level (RSL) at Blasø fell from the marine limit at ~ 33 m a.s.l around 8.5 - 7.6 cal. ka BP through the Early and Mid-Holocene (cf. Bennike and Weidick, 2001). Water depths in the basin during this time would have reduced due to uplift but the basin was clearly marine and connected to the sea throughout this period. From 4.4 cal. ka BP the basin returned to freshwater conditions as the 79N grounding line re-advanced and the ice shelf reformed. In many regions of Greenland this Neoglacial re-advance of ice is associated with crustal depression and a rise in relative sea-level (Long et al., 2009), but irrespective of this whether this occurred in the Blasø area, marine conditions were unable to penetrate the basin due to the ice shelf grounding along the northern edge of Blasø through the Late Holocene. In contrast, the rapid rise in sea-level following the LGM, probably played a key role in driving deglaciation of the adjacent continental shelf.

The timing of ice shelf retreat/absence discussed here is entirely dependent on radiocarbon dates on marine carbonates. Probably the largest uncertainty on these ages is the choice of reservoir correction, which you use 550 years (Delta R of 150 on Marine13), which has been used in other North Greenland studies. Can you discuss, perhaps in Section 3.5 and/or 4.3, how large of an uncertainty there could be on this choice of reservoir age? I imagine the epishelf lake receives a great deal of meltwater, and you mention elsewhere that you think there is likely an old carbon effect from the local geology. I don't think you need to change your chronology (you've made an assumption and supported it with previous work), but it would be worth acknowledging the uncertainty and how large you estimate that uncertainty could be (e.g., decades, centuries, millennia?).

We agree that the choice of marine reservoir (MRE) is important, which is why our approach followed previously published studies from the region so that our chronology is comparable with existing literature. As noted below, the choice of MRE/calibration curve (Marine13 vs. Marine20), results in minimal differences in the calibrated ages i.e., delta R = 150±50 (Marine13) or delta R = 0±0 (Marine20). In this example, the 'uncertainty' is 'decades'. In our opinion, as long as the method of calibration is clearly documented, and the 14C data is publicity available, then future work can re-calibrate our 14C should approaches change.

However, because several recent papers e.g., Hansen et al. (2022), Davies et al. (2022), Pados-Dibattista et al. (2022) have applied Marine20 to calibrate 14C ages from the NE Greenland Shelf we plan to re-calibrate all of our ages with Marine20 in our revision. We intend to follow Hansen et al. (2022) who applied a delta R of 0±0 years. This takes account of the increased reservoir ages in the Marine20 calibration curve, and results in near-identical calibrated ages (compared to Marine20).

Regarding meltwater influence, the reality is that all near-shore, glacier-proximal sites would have been influenced by glacial melt during deglaciation, and there is no easy way of assessing divergence between dated-remains that were influenced by significant input of freshwater and those in the deeper ocean which likely remained isolated from this. To do this we would require independent chronological control, either from terrestrial macro-fossils incorporated into the lake sediments, detection of well-dated tephra and/or

application of other chronostratigraphic tools not influence by marine reservoirs i.e., relative paleointensity dating. Note that our future work intends to explore some of these dating methods.

In our revision – and as noted in the comments above and below – we intend to re-calibrate using Marine20 (delta R of 0±0).

We will briefly discuss this 'uncertainty' in section 3.5 and in doing so we will refer to O'Reagan et al. (2021) who also outlined some of these issues.

Line 94: The Bentley et al., in prep study sounds fascinating, but the water column data would be useful here in this study. Is there a possibility that those data could be presented here as well?

Apologies, our original plan was to submit both papers simultaneously so that reviewers would have oversight of all the relevant data. Bentley et al., is now under review for TCD so can be viewed here:

https://tc.copernicus.org/preprints/tc-2022-206/.

Line 154-155: Or terrigenous source variations (i.e. siliciclastic vs carbonate rocks)? You discuss limestones in this region elsewhere?

Yes, that's right, limestones are the likely souce. We will amend this sentence.

Line 213-214: I have no problem with you using Marine 13. But to be fair, the Marine13 paper makes a similar caveat about the complexities of working in high latitude environments (Reimer et al., 2013)—the problem of unknown, large, and variable ΔR is not unique to Marine20.

We agree with the reviewer's comments – Marine20 was just more explicit in voicing the complexities associated with 14C calibration in high latitudes/polar environments. The polar community has long been aware that this also applied to Marine13. Unfortunately a reviewer of a separate (earlier) submission asked us to revert to Marine13 because of the explicit statement in the Heaton et al. (2020) paper ('it is not suitable for calibration in polar regions'), so we followed this recommendation for the current paper. The reality is that as long as everything is clearly documented, then the chronology in our paper will be forward (and backward) compliant as calibration curves and marine reservoirs develop and/or change.

However, as an illustration, if you follow the recent paper by Hansen et al. (2022), who advocated a delta R 0±0 years because this essentially replicates/is directly comparable to a delta R of 150±50 years/Marine13 (e.g., Larsen et al., 2018), then the resulting 14C ages are within the analytical error of the ages presented in our original submission (Marine13 = delta R 150±50) (Table 1, attached). Similarly if you follow Heaton et al. (2020) and use the nearest radiocarbon data point in the Marine20 database (MapNo. 31 = delta R 3±60; Funder, 1982) then the calibrated ages are also very similar.

See Table 1 (attached)

Note that we intend to re-calibrate our ages in the revision following Hansen et al. (2020) and will update our text accordingly. Our justification for doing this is that several other recent papers broadly follow this choice of delta R e.g., Pados-Dibattista et al., 2022 (delta R = 0±50 years); Davies et al. 2022 (delta

R = 1 ± 32 years).

Line 414: or lake ice?

Thanks – this should be lake ice – we will update our text!

Line 488: LF7 to LC7

Thanks!

Figure 1: Indicate what the brown triangles represent in the caption. (grounding zone?)

We will add this information to the caption (and yes, triangles indicate position of grounding line).

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Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2022-173/tc-2022-173-AC1-supplement.pdf