The Cryosphere Discussions

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Comment on tc-2022-173

Anonymous Referee #1

Referee 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-RC1, 2022

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 am 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?

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?).

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?

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

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.

Line 414: or lake ice?

Line 488: LF7 to LC7

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