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Climate of the Past Discussions

Interactive comment on "Glacier response to North Atlantic climate variability during the Holocene" *by* N. L. Balasacio et al.

Anonymous Referee #2

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Balascio et al. present a very interesting dataset that constrains the Holocene history of glacier fluctuations of a small cirque glacier in East Greenland. The authors then take it a step further and compare their record to other regional records of glacier variability in an attempt to argue for a synchronous pattern of regional glacier change across multiple timescales. Overall, this is a solid paper with a lot to like and probably the best local glacier reconstruction from the region. However, there is a key conclusion that I do not agree with, and I do not think the dataset presented here supports their claim. Mainly, the authors' end conclusion of glacier synchroneity over the last ∼1000 years (more info below). Nevertheless, this one issue does not detract from the overall quality of the paper and this manuscript should certainly be published in Climate of the Past after a few minor revisions. Lastly, it was a pleasure to read and review a manuscript that is well written and finely tuned. Perhaps I have just had a patch of bad luck in

receiving a string of poorly written manuscripts to review, but the overall quality of this draft was much appreciated.

Key strength: My sense is that this dataset and manuscript are going to end up on the short list of glacier reconstructions developed from proglacial lake sediments that actually capture a robust glacier signal instead of just capturing noise or some hard to decipher combination of the two. With a few exceptions, records of glacier variability that rely on fluctuations of sediment delivery to proglacial lakes can be extremely problematic, and frankly, can do more harm than good by making it into the literature. The record presented here from East Greenland circumvents this issue by taking advantage of an exceptional geomorphic setting. The catchment is extremely small and consists of the glacier-cirque region and the lake. . . that's it. The end result is a sediment package that is almost certainly dictated solely by glacier fluctuations and cannot really be influenced by other non-glacial processes – the processes that severely limit the utility of similar reconstructions from proglacial lakes with much larger catchments. I think the authors realize this fact, and have even dedicated a nice little section ("Sedimentation in proglacial lakes") to highlighting the uniqueness of the Kulusuk Lake. This is a great component of this manuscript that the authors have highlighted. My only suggestion here would be to throw in a sentence or two in the final conclusions section that again highlights the unique lake setting and why the authors were able to generate such a clean glacier signal from proglacial sediments. I think it is that important. Nicely done.

Comparison to Greenland Ice Sheet fluctuations: The authors have noticeably stayed away from comparing their record of cirque glacier fluctuations to recorded fluctuations of the Greenland Ice Sheet margin. I'm guessing that the authors wanted to compare "apples to apples" and just stick to other cirque/mountain glacier records. Rather, you can back out a climate record from cirque glacier fluctuations, but not really from ice sheet fluctuations. I think that approach is fine, but a brief paragraph that makes a few links to the GIS would make this paper stronger and likely garner more citations, while at the same time it would remain clear that this paper's focus is on climate records that

can be deduced from cirque glaciers. It looks like Carlson eludes to this very same point with his posted comment on the interactive discussion page that accompanies this manuscript. Again, a paragraph making the link between key advances seen in the Kulusuk record and key advances of the GIS margin would make for an important paragraph. Luckily for the authors, but unfortunately for the scientific community, the list is going to be short as the detailed Kulusuk record spans an interval where detailed GIS margin records are lacking. The authors could mention the ∼1.5 ka advance seen in both the Kulusuk and GIS records that Carlson suggests, and also mention the clear 8.2 ka advance seen at in the Kulusuk record and the glacier margin record at Jakobshavn Isbræ. For the 1.5 ka advance from the southern GIS per Carlson's suggestion, I would add the caveat that this is the only place along the GIS margin where this advance is seen, and unlike the 8.2 ka event for example, there is not a clear and well-established cooling event at 1.5 ka that can easily explain the synchronous advance of both types of ice margins. The 1.5 ka advance could indeed have been driven by cooling, but it could just as easily been driven by ice dynamical processes and the timing is just pure coincidence. Again, I would add this record to the text, but just include the aforementioned caveat. For the 8.2 ka records, the authors could even mention that the coeval response of the small and responsive Kulusuk glacier and Jakobshavn Isbræ speaks to the sensitivity of GIS outlet glaciers. Rather, here is direct evidence that at least a portion of the GIS is able to respond just as quickly to a climate perturbation as a small 'responsive' cirque glacier. This would be an interesting and important point because the authors use the small and responsive cirque glacier argument as part of their initial motivation for this study. The appropriate references for the 1.5 ka advance of the GIS are Bennike and Sparrenbom, 2007; The Holocene, v 17 and Winsor et al., 2014, QSR, v. 98. For the 8.2 ka event related GIS papers, the authors could consult Young et al., 2011, Geophysical Research Letters, v. 38 and Young et al., 2013, QSR v. 60.

Comparison to other regional records of glacier variability over the last ∼1200 years: The authors try to make the case that the Kulusuk record is coeval with the Baffin

Island record of ice cap expansion. The authors state "Kulusuk glaciers increased in size ca. AD 1250–1300 and again ca. AD 1350 and AD 1450, precisely when ice caps on Baffin Island (Miller et al., 2012) and Iceland were expanding." I agree that there is synchronous ice-cap expansion at ∼ AD 1250-1300. This is the first sharp peak in the Baffin Island probability plot and also coincident with a period of extreme volcanism (cited cooling mechanism in Miller et al., 2012). However, I think the authors here are misinterpreting the Baffin probability plot a bit, maybe in a bit of an effort to argue for more synchroneity that there actually is. Mainly, a period of synchronous glacier expansion at ∼1350 AD is a bit of a stretch. I see this pulse in the Kulusuk record, but this coincides with a period of ice growth and melt in the Baffin probability plot, not just ice expansion. Rather, the entire Baffin probability hump is not one large period of ice-cap expansion, nor can you pick out pulses of ice-cap expansion beyond the 1275 and 1450 AD peaks; those are the only clear pulses of ice-cap expansion (both peaks linked to volcanism). To make a claim about synchronous glacier growth at 1350 AD is not supported. Moreover, the overall comparison between the Kulusuk record and the Baffin Island record is a bit tenuous because while I agree there are similarities between the two beginning ∼1275 AD, this relationship breaks down back in time. In fact, the Baffin Island record depicts ice cap expansion between ∼AD 875-975 whereas the Kulusuk record depicts the exact opposite – a significant period of glacier recession at the exact same time. I think at best the authors can claim there appears to be a synchronous advance at ∼1250-1300 AD, and that glaciers remain extended after AD 1450, which is also seen in the Iceland and Baffin lake records. I would modify this text accordingly and make note that prior to ∼AD 1275 there does not appear to be much similarity. This does not include the mention of glacier expansion coincident with Bond events seen in the Kulusuk and Iceland lake records, this is all fine and good.

Minor comments: Page 2011, line 15: maybe add "geomorphic" before "evidence" Page 2020, line 21: "possibly" instead of "possible" Page 2021, line 13: "Likely" seems a bit strong here. This paper presents a valid hypothesis, but "likely" makes it sound

as if this hypothesis is set in stone. Figure 3 caption: Does there need to be letters in the figure that correspond to the a,b.c in the caption? Also, I see no dashed line on the PC1 plot. I see the dashed line down in Figure 4, but not Figure 3.

Interactive comment on Clim. Past Discuss., 11, 2009, 2015.