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Climate of the Past
Discussions

Interactive comment on "Episodic Neoglacial expansion and rapid 20th Century retreat of a small ice cap on Baffin Island, Arctic Canada and modeled temperature change" *by* Simon L. Pendleton et al.

Anonymous Referee #2

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The paper by Simon L. Pendleton et al. entitled ÂńEpisodic Neoglacial expansion and rapid 20th Century retreat of a small ice cap on Baffin Island, Arctic Canada and modeled temperature changeÂż documents the retreat of ice retreat with the help of radiocarbon dated plants that are re-exposed as the margin melts away. Glacier variability is sought understood with the help of a simple mode explaining glacier variability solely as a function of corresponding shifts in summer temperatures.

I found the data interesting and the text well written. On the other hand, the authors have made decisions that I find hard to understand. My main objection to this paper is

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Discussion paper

that does not present the data in a bigger picture. The data is hardly discussed with reference to other glacier records from the Arctic and the same is true for the climatic inferences. Given the expertise available in the author team I'm a bit surprised how limited this part of the ms is. If the main purpose with the paper is to Âńestimate the changes in summer temperature required to reproduce the observed record of ice margin advance.Âż then a better job is warranted.

I wonder though if there isn't a larger (and more interesting?) story to be told. By changing the title to Âń20th Century Ice Cap Retreat Ends Arctic Neoglacial CycleÂż I see an opportunity to expand the relevance of this excellent dataset from Baffin Island. Does this small ice cap pattern resemble other records? Is there a spatial manifestation that is worthwhile pursuing? Is really summer temperature the only interesting and relevant parameter with respect to this glacial cycle? What about precipitation? Are there regional shifts etc.

As it reads now the data is not utilized to its full potential. It will require some work to improve the ms, but is in my opinion within the scope of major revision. That revision also needs to take a few other points into account:

1. The modelling section needs to be included in the paper itself, not just in the SI. Text can easily be moved from the supplement and into the paper itself. 2. Assuming, as the authors have done, that annual accumulation is constant (and ignoring wind) is problematic. In rationalizing this choice the authors write in the SI that ÂńAlthough precipitation records are sparse near the study site, ice core records from the âĂÍsummit of the Greenland Ice Cap show that regionally, precipitation varied by only ∼6% over the last 1200 years (Alley, 2004)Âż. This might be true, but it might also be an underestimation of the precipitation changes at the study site because we do not know how well the two sites correlate. Considering that this can have some bearing on the summer temperature estimate it deserves to be discussed more than it currently is (and also wind, as acknowledge by the authors). 3. A pan-Arctic perspective would require the authors to dig deeper into the available literature. Citing for instance Jason

Briner et al on the use/value of threshold lakes is inappropriate. Look for instance up studies carried out by John A Matthews and Wibjörn Karlén (Geology, 1992) decades ago. Citing papers that are in review (Crump et al) is something I don't recommend. Referencing Miller et al (2016) for maximum Neoglacial during the LIA is fine, but there are a number of other datasets from Svalbard that both have discovered this earlier and also datasets that contradict this observation (see for instance Reusche et al. 2014). 4. Figure 1and 2 is of poor quality. I challenge the authors to find a better way to present the sites and the data. A conceptual model might be in place. 5. As of now the glacier dataset is only presented in Figure 3. Choosing not to compare this new dataset to other records is a bad decision. Moreover, the data is not discussed with respect to the normalized probability due to radiocarbon dating, which is represented by a grey curve – why? I'm not a big fan of the dashed line, which most likely is hand-drawn? For obvious reasons (how do you account for uncertainty when drawing that line?) I suggest that it is removed. 6. Why not show the ESM output in figure 4 compared to existing paleorecords of actual data? Surely differences between a model and existing datasets are worthwhile discussing considering that the authors end up concluding that a Âńminimum average cooling of $\sim$0.44°CÂż is required to explain the observed variations in horizontal ice cap fluctuations? 7. The authors state in the SI that ÂńAt the time of submission, the simulation was still running and had only reached 1270 CE.Âż Fair enough, but not a very convincing argument. Should we still trust the data? For what reason? Why not wait until the run was complete?

I whish the authors the best of luck with revising the ms and hope my comments have be helpful.

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