

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2020-337

Anonymous Referee #2

Referee comment on "Recent North Greenland temperature warming and accumulation"
by Helle Astrid Kjær et al., The Cryosphere Discuss.,
<https://doi.org/10.5194/tc-2020-337-RC2>, 2021

This paper combines some new snow/firn core data with GPR transects and some climate model output to investigate trends in air temperature and accumulation in the accumulation zone of North Greenland. The results may be of interest to the cryosphere community but I agree with Reviewer 1 that (1) the novelty of the results is not sufficiently well communicated and (2) the overall presentation is poor. I also have some additional major concerns which I detail below but, in short, I recommend that this manuscript is rejected.

General comments

Lack of systematic uncertainty analysis: Without a rigorous uncertainty analysis it is impossible to conclude whether the magnitude of the derived trends in temperature or accumulation are significant (e.g Fig. 2). The authors state that the temperature records have uncertainty of " ± 0.5 C" but there is no mention of this number in the methods. Likewise, there are no uncertainty bounds on the temporal trends in accumulation in Figure 2. When the authors do provide some uncertainties, there is almost no effort to justify these estimates.

Poor presentation: Figure presentation is generally poor and the tables are confusing. See specific comments for more details.

Unsupported conclusions: There are statements in the conclusion that are presented for the first time without any evidence to back them up. The authors find a "slightly larger accumulation in the northern Greenland area than that mapped by Burgess et al. (2010)" but this is not quantified in the results or presented in the figures. The authors also document an the increase in temperature of "0.9 to 2.5 C/decade" appears here and in the abstract but I can find no reference these numbers in the results, tables or figures. Does this range represent different firn cores? Or an uncertainty range? Over what decades?

Specific comments

P1: Some more background is needed in the introduction before you dive straight into expected increases in accumulation expected from Clausius-Clapeyron relationship. For

example, over what time period is the Arctic warming significant? If the Arctic is warming, why is it surprising that the central part of the ice sheet is warming? Have any previous studies observed changes in accumulation over the past few decades? What do the regional climate models predict?

P1 L39-40: How can uncertainty be *due* to surface mass balance? Please clarify.

P1 L41: Precise is one thing but I would argue that accuracy is more important in this case. Consider your choice of words here.

P2 L3-4: Again, the logic of this statement is flawed. The lack of ground truth alone does not necessarily mean that satellite radar altimeters are uncertain. Please revise.

P2 L4-6: Please include some references for these statements.

P2 L8: It's the Amazon River, the Sahara Desert and should be the Greenland Ice Sheet. Please consider capitalizing "ice sheet".

P2 L8-10: If accumulation trends vary depending on the "exact region" then they are not in disagreement. Please revise this sentence. Otherwise, please provide some examples of studies being in disagreement for the same region and same time period. If no such studies exist, then that point should be made clearly in this section. As the next few sentences are written, it's not obvious which studies can be directly compared.

P2 L11: What do you mean by "changed", increased or decreased? Please be more precise.

P2 L8-29: This section is just a laundry list of previous studies and, without some sort of order, it is hard to follow. I recommend re-structuring into studies that found 1) an increasing trend, 2) a decreasing trend and 3) no trend.

P3 L32-34: Rather than providing a reference from the 90s, the authors should be able to test this assumption and I encourage them to do so.

P3 L34-36: What is meant by "annual cycle"? Do the authors mean "seasonal cycle"? Please also provide more justification for the statement that the "annual/seasonal cycle" is dampened by 5% compared to the surface. A reference from the 90s seem insufficient given the enormous amounts weather station and climate model data now available to test this assumption.

P5 L24-25: Please provide some justification for these assumed uncertainties in firn density and annual layer position.

P5 L33: Earlier (P3 L19) the authors state that "precipitation-evaporation" was derived from HIRHAM but now it is "snowfall accumulation". Was this change in terminology deliberate? If so please clarify, otherwise use consistent terms.

P6 L11-12: The authors state that the borehole temperatures vary by location, what is the significance of this statement? Other than a sanity check, this result seems trivial and could be removed.

P6 L13-14: There sentence is not a result of the analysis so it should either be deleted or moved to the methods.

L6 L16-17: Deriving a robust relationship between mean annual values of $\delta^{18}\text{O}$ and temperature is critical for deriving temperature trends but there is no description of the

methods used to do so until this statement here. This sentence should be moved to the methods section and some justification of using the equation presented by Johnsen et al. (1992) should be provided

P8 L25: Where does the uncertainty of " ± 0.5 C" come from? There is no mention of this number, or a systematic uncertainty analysis, in the methods.

P14 L10: The increase in temperature of "0.9 to 2.5 C/decade" appears here and in the abstract but I can find no reference these numbers in the results or figures. Does this range represent different firn cores? Or an uncertainty range? Over what decades?

Figure 2: Please add some uncertainty bounds for these different methods.

Figure 3: It is difficult to compare different products here, please use a different way of visualizing this data.

Figure 5: Plotting an line through an aggregation of all the accumulation values is meaningless. Of course you get a high R^2 because there is spatial variation in accumulation which (hopefully) the model captures. More important for this study, which is based on individual cores, is whether HIRHAM represents the interannual variability and trends. In this case, it looks like HIRHAM does poorly.

Figure 6: A map of shear stress would be better than an elevation map for identification of ice stream boundaries. Also it would be more intuitive to label the axes with lat/lons.

Figure 7: How do you justify "where the back-diffusion can be trusted"? I can find no justification in the methods. Also note misspelling of "diffusion".