



EGUsphere, referee comment RC2  
<https://doi.org/10.5194/egusphere-2022-368-RC2>, 2022  
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## **Comment on egusphere-2022-368**

HP Marshall (Referee)

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Referee comment on "Snow stratigraphy observations from Operation IceBridge surveys in Alaska using S and C band airborne ultra-wideband FMCW (frequency-modulated continuous wave) radar" by Jilu Li et al., EGU sphere,  
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This is a valuable, well executed study, demonstrating the ability of ultra-wideband airborne FMCW radar to measure annual accumulation layers on mountain glaciers, as well as to distinguish between dry snow and percolation/wet snow facies, and ice facies. The methodology is well described, and the combination of the radar observations with a one-dimensional physically based depth-age model is solid. The authors show good agreement between the radar-derived accumulation rate estimates and some limited field observations, lending confidence to the approach. The results are significant both in terms of the radar observation advances, and the implications of the measured accumulation rates. This will be an important paper, and just needs a bit more detail and sensitivity analysis before publication.

General comments:

- 1) A few more details on the processing approach (i.e. what FFT windowing method was used, what kind of horizontal filtering was applied) would be helpful to add.
- 2) The sensitivity of the results to the chosen permittivity should be evaluated. A relative seasonal snow permittivity of 1.89 was used, based on work on other glaciers - how does this compare to this study site? If field measurements are not available from the time of these flights, how much do results change for a conservative range of seasonal snow density/permittivity?
- 3) How were the layers picked? Manually? Semi-automatically with control points? Automatically? This needs more detail.

4) The model appears to produce a depth-age scale, but also a depth-density result. How does this compare to the ice core data? The authors state that although the model was tuned to Greenland, it represents the firn well - it would be useful to show this with field observations from this site, which should be available from the ice core. For example, Greenland gets a huge amount of wind influence, and the authors state that the main site is not wind affected. Its possible the assumed surface density in the model, for example is a bit too high.

5) This is the most important general comment -- the interpretation of the linear increase in accumulation rate is the most important result from a glaciological/snow science point of view, but needs a bit more work to test this trend, and explain why it might be happening. Is this linear increase over the past several decades expected based on regional climate models? Other glacier observations? Even if we had a linear increase the last 2 decades, why would we expect this to be the case in the decade previous (which was used to extrapolate to the early 1990s)?

I'm honestly a bit worried that this trend is caused by an assumption of constant density, or some artifact in the model. Can you back this linear increase in accumulation up with any other regional data from field observations or models? Or how about the ice core - does that show a linear increase in accumulation rate? I would expect the chemistry in the ice core would have resulted in a depth-age scale, so accumulation time series should be available from that?

Related to this - your model assumes a steady state - that the accumulation rate is balanced by the melt/densification and flow divergence. You show this with the little change in surface elevation between 2018 and 2021. Then what does the increasing accumulation over time then imply? Greater basal melt to balance this increase? Or a volume flux or densification rate increase with time?

Is it possible that a bias in the densification model is leading to the linear increase in estimated accumulation? I think a sensitivity analysis is needed here, along with error bars for the plot of accumulation vs time.

Detailed suggestions/edits in the attached annotated PDF.

Great paper, I look forward to the final version!

cheers,

HP

Please also note the supplement to this comment:

<https://egusphere.copernicus.org/preprints/2022/egusphere-2022-368/egusphere-2022-368-RC2-supplement.pdf>