EGUsphere, referee comment RC1
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Comment on egusphere-2022-698

Anonymous Referee #1

Referee comment on "Polar firn properties in Greenland and Antarctica and related effects on microwave brightness temperatures" by Haokui Xu et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-698-RC1, 2022

General Comments

This manuscript presents an exciting attempt to integrate active radar and passive microwave measurements to characterize firn properties (namely density) in both Greenland and Antarctica. The authors use airborne, high-bandwidth radar sounding data to constrain the presence of refrozen melt layers in firn and then use that information as an input to forward modeling of passive microwave brightness temperatures. The authors pull in a lot of different datasets (both modeled and observed) to provide a comprehensive analysis of near-surface firn properties.

Overall, while the scientific scope is extremely relevant and worthy of publication within The Cryosphere, I believe how it is presented and described within then manuscript would benefit from a thorough revision to 1) clarify/streamline the structure and 2) address how this research more clearly fits within the broader field.

Regarding the former, the authors make use of many different datasets and models (e.g., global climate model outputs, CFM, active radar, in situ measurements, UWBRAD, SMOS, forward model of brightness temperatures) but it isn't always clear how these pieces fit together to meet the main objective of the paper. To me, the manuscript is missing an overarching structure that the authors can use to guide the reader; the lack of which makes the manuscript difficult to follow.

Regarding the latter, the manuscript currently reads as a long Introduction followed by a flood of Results and then one paragraph of Conclusions. The authors do not make space for discussing the relevance of their results with respect to other work or how they see their work contributing to the field into the future. Currently, the authors leave all this to the reader to intuit for themselves, which can limit the impact the manuscript will have (i.e., be explicit and tell the reader why your work is important to them).

Below I have included suggestions for how the authors might consider revising their manuscript in light of these points as well as other comments and questions that I think would add to the manuscript.

Specific Comments

1) To address these issues raised in my General Comments, I'd recommend the authors consider adopting a more conventional manuscript structure (Introduction ☐ Methods ☐ Results ☐ Discussion ☐ Conclusions). As currently structured, the Introduction attempts to contextualize the research (which I think it does fairly well up until line 66) but then diverges into presenting all the datasets and methodology to be followed through the remainder of the paper. I would recommend considering the inclusion of a stand-alone Methods section where the entire procedure the authors envision can be described (perhaps including a flowchart?) along with all the models (CFM, forward model) and datasets (in situ, Snow Radar, UWBRAD). I believe this would provide the necessary structure needed to organize the current Results section and a point of reference for a newly added "Discussion" section.

2) By their own admission (line 63), the authors are not the first to recognize that passive microwave measurements contain information on firn density fluctuations (note that Tan et al. (2019) does not appear in the reference list). However, there are noticeable absences from what the authors present as the existing research in this direction. How do the passive microwave Greenland density results of Houtz et al. (2019, 2021) and Mousavi et al. (2021) fit together with what is presented here?

Houtz et al. (2019) "Snow wetness and density retrieved from L-band satellite radiometer observations over a site in the West Greenland ablation zone" *Remote Sensing of Environment* 235 https://doi.org/10.1016/j.rse.2019.111361

Houtz et al. (2021) "Quantifying Surface Melt and Liquid Water on the Greenland Ice Sheet using L-band Radiometry" *Remote Sensing of Environment* 256 https://doi.org/10.1016/j.rse.2021.112341

Mousavi et lal. (2021) "Evaluation of Surface Melt on the Greenland Ice Sheet Using SMAP L-Band Microwave Radiometry" *IEEE JSTARS* 14 https://doi.org/10.1109/JSTARS.2021.3124229

3) As a whole, the manuscript reads as under-cited. The authors often make statements that appear to require or refer to other sources without any indication of what those sources are. I have compiled the following list of example locations.

Line 35: “For example…”

Line 43: “Because the material…”

Line 45: “For example…”

Line 68: “In our previous…”

Line 132: “The second and third…” (specifically in reference to the NEGIS density profile)

Line 168: “UWBRAD measures…”

Line 171: “The University of…”

Line 204: “These peaks are…”

Line 242: “The exponential form…”

Line 249: “Although the top…”

4) In Section 2, the authors demonstrate the ability of the CFM to produce roughly equivalent density profiles to what has been measured in situ. I am left to wonder however, why these three specific in situ examples where chosen (i.e., Summit, NEEM and NEGIS)? The authors have already referred to the SUMup dataset (i.e., Montgomery et al., 2018) which contains many more in situ density profiles, some with better depth sampling than the NEEM and NEGIS examples and more contemporaneous with the UWBRAD and Snow Radar measurements. Is there a specific reason these three sites are preferred compared to others?

5) Table 1 presents a comparison of the Site 1 in situ density profile and the modeled CFM result. Variations in CFM density are constant with depth while the in situ ones decrease. At the same time, the vertical correlation lengths in the CFM results seem to be consistently smaller than those based on the in situ data. How are the metrics by which the "reasonable agreement" (line 166) between the CFM and in situ density profiles (i.e., standard deviations <0.03 (line 151) and correlation lengths <20 cm (line 152)) chosen? They seem rather arbitrary.

Furthermore, the caption states that one meter of data are used to estimate these properties but why is the depth interval at which these properties are reported two meters? Why not also present the vertical density standard deviation and correlation length between one and two meters?

6) The authors use "sites", "locations", and "points" when referring to individual measurements interchangeably. I would recommend using one term consistently through the entire manuscript. Furthermore, the specific sites (i.e., what is referred to as "Site 1") appear to change between sections. For example, in Section 2, "Site 1" refers to the location of the Summit in situ density measurement (line 136) but in Section 3, "Site 1" refers to the northernmost intersection of the UWBRAD and Snow Radar flight lines (Table 3, Figure 3). This is very confusing for the reader. I would recommend against Section-specific naming conventions.

7) The authors have many figures that I believe could be condensed. For example, could Figures 1 and 2 be combined into a single figure with three sub-panels overlaying the measured and modelled density profiles? Could Figure 5 and Figure 6 be combined since they present different versions of the same information? Could Figures 9, 10, and 11 be combined into a single figure since they all share the same x-axis?

8) I recommend the authors clarify the relationship between their representation of the vertical density profiles used in Section 2 (line 126) and Section 4 (line 234). Is it necessary to include both a one-dimensional representation (i.e., Section 2) and the three-dimensional representation (i.e., Section 4)?

9) In Section 4, the authors introduce a horizontal correlation length for their density profile. What is the physical justification for including horizontal density variations? Furthermore, the emission model appears to be one-dimensional, so what does the additional horizontal density fluctuation contribute to the analysis?

10) A key takeaway of this work seems to be the need for the co-acquisition of active radar and passive microwave data to asses any influence from refrozen layers. How do the authors perceive the broader applicability of their methodology moving forward knowing the current spatial and temporal coverage disparity between where active radar measurements (i.e., airborne at specific points in time) exist compared to passive microwave (i.e., satellites in continual operation)?

11) In Section 5, the authors attribute cross-frequency brightness temperature variations in the UWBRAD results between locations 1-3 and location 4 as the impact of more refrozen layers at location 4. Can they authors elaborate on why they believe it is solely the number of layers that affects the measured brightness temperature and not their position (i.e., the relative depth between layers as well as they're absolute position within the firn column)? Would it be more intuitive to expand on what is presented in Figure 11 to include the effects of multiple layers as well as their relative positioning.

12) In Section 5, the authors introduce numerous exponential functions that are used to decrease different model parameters with depth (e.g., exp(z/33), exp(z/55), and exp(z/5) on line 315 and exp(z/30) and exp(z/40) on line 356). How were these specific functions chosen?

13) It is not clear why Antarctic SMOS data are included in the study. Why not use SMOS data from Greenland that overlap with the active radar and UWBRAD datasets? How does the Antarctic SMOS analysis contribute to findings of the paper?

14) The authors repeatedly use "reasonable" as a qualitative, catch-all term for describing the agreement between two sets of data/observations without providing any justification. What specifically about these comparisons deem them to be indicative of a "reasonable agreement" from the authors perspective?

Line 164 (and line 166): The CFM results in Figure 2 do not reproduce the in situ variability at the top of the firn profile. What aspect of the CFM density profile is it that the authors are using to deem the agreement with the in situ profile reasonable? Is it simply the mean density?

Line 239: What degree of vertical offset (i.e., +/-15 cm?) do the authors allow between the CFM density peaks and the active radar peaks for them to be illustrative of a reasonable agreement. How does this compare to the vertical resolution of these two datasets?

Line 311; When applying the iterative method to refine the model parameters, how do the authors define what a reasonable match is? Is there some error level, threshold, or similarity metric the authors use? If so, what is it?

15) Could the authors please clarify how the accumulation rates presented in Table 7 are calculated? Are they an output from the forward modelling of the brightness temperatures or are they simply calculated based on the depth to the assumed 2012 melt layer identified from the Snow Radar measurements?

Technical Corrections

1) Please be consistent with referencing style. Sometimes references appear in square brackets while for others the authors use round brackets (i.e., [] vs ()). There are also some citations (e.g., line 237) with references that are missing publication years. Please follow the TC guidelines for citations.

2) Please ensure consistent symbols throughout the manuscript. For example, ε is used in the equation on line 239 but □ is used in lines 240 and 243.

3) Please ensure units are provided for every axis for every figure. There are some figures which have units for every axis (e.g., Figure 1), some with units for one axis only (e.g., Figure 2, Figure 4-7), and some without units completely (e.g., Figure 12).

4) Please modify Figure 4 as radar echograms are typically presented with distance along the x-axis and depth along the y-axis. Furthermore, I would recommend highlighting the exact portions of the echograms the individual profiles are pulled from. Finally, since the authors average echo profiles over one-kilometer sections, I would suggest denoting the x-axis of the modified echogram in groundtrack distance as opposed to latitude. I think this will be more intuitive for the reader as to how much averaging is done.

5) Please explicitly label subfigures (i.e., a, b, c, d, …) instead of using positional cues (i.e., top-left, right-most, etc.).

6) Please follow the TC guidelines on numbering manuscript subsections in Section 5.

7) Please ensure that the CReSIS **S**now **R**adar is capitalized when appropriate as it is the formal name for the system.

8) Please ensure the labelling and captions for each figure is correct. For example, the label on the right side of Figure 6 appears incorrect as well as the caption. I assume this should be point 4?

9) Please be consistent in how the amplitude of the active radar data are expressed. The echogram as part of Figure 4 presents the data in dB while all other plots (Figures 4-7) seem to present linear amplitudes. I would recommend plotting everything in dB as this is the more conventional representation for these data.

10) Please follow the TC guidelines with regards to labeling equations.

11) Please follow the TC guidelines with regards to unit notation (i.e., with negative exponents, non-italicized).

12) Line 247 makes reference to a "region 1" that seems to be associated with Figure 8; however there is no region 1 identified in Figure 8 so I am unsure to what the authors are referring.

13) Please use consistent notation in Table 5 as is used in the text (i.e., what is $corl_p$?). Also, please use a consistent font size within the table.

14) Please use a consistent Table style following the TC guidelines.

15) "frin" line 71

16) Instead of just the bandwidth, please provide the actual upper and lower frequency bounds for Snow Radar signals on line 84 as is done on line 172.

17) Units are missing for the depth in line 221.

18) There is an extra colon in line 233.

19) There is an extra colon in line 279.

20) Please provide units for the reflection amplitudes on line 280.

21) There is an extra colon in line 282.

22) There is a space missing on line 316 (i.e., "asl_p" compared to "as l_p").

23) Please fix the legend in for the Point 3 subplot in Figure 13 to match the rest of the subplots.