Response to RC2: Spatial characterization of near-surface structure and meltwater runoff conditions across Devon Ice Cap from dual-frequency radar reflectivity

Dear Anonymous Reviewer,

We thank you very much for your review of the manuscript. The comments/suggestions (italicized below) were very helpful and constructive for improving this work. We've addressed each point below with our responses and proposed revisions provided as bulleted text.

Sincerely,
Kristian Chan, on behalf of the co-authors

This study uses four radar datasets (3 airborne, 1 ground based) to evaluate the firn characteristics of Devon Ice Cap in the Canadian Arctic. The general characteristics of the firn were already classified using the ground-based dataset, and the new element here is using all the airborne data together to look at the firn. These data are used as a way to assess the spatial distribution of firn properties in more detail, within the general framework of the ground-based survey. Conclusions about ice-slab thickness and melt channel distribution are derived largely using the variability in return power of the surveys within different "zones" of firn, relying on the ground-based survey to get the general structure (i.e. large slabs, thin lenses, etc). The implications for meltwater runoff are discussed, making a nice story. The main novel element here is inferring properties of ice lenses in firn using multiple airborne radars that do not resolve the ice lenses/slabs explicitly, but instead have some return-power sensitivity to the near-surface properties.
This study is novel, generally well written, and well-suited to The Cryosphere. I have two major comments and a variety of small points that I think are important to address before publication, but then I think it should be a nice contribution.

Major Comments:

There is insufficient analysis of whether one could conduct a similar study in the absence of some independent radar measurements that actually resolve the bottom of the ice slabs (i.e. the GPR)—perhaps this was never the goal of the study, but the title and some of the language suggest otherwise, which I think sets the reader up to be dissatisfied at what is otherwise a nice paper. The suggestion in the title, abstract, and conclusions is that the dual-frequency reflectometry can be used on its own to garner insight into firn properties (and extra-terrestrial applications cannot rely on such validation). As I read the paper, the analysis of things like the ice-slab thickness in Zone II (Section 3.2.3 and Discussion) relies on already knowing that this area has thick ice slabs, and otherwise the variations could be misinterpreted as density variations or similar. If the paper can be altered to use the GPR as validation rather than as a necessary component, that would be ideal; for example, is there some objective measure that would allow the picking of the zone boundaries from these model results? I assume that the answer is no since otherwise it would be discussed (which is worth adding to the text); I think this study will merit publication without that analysis, although in this case I think textual/title alterations are needed throughout to make clear that what is really happening is analysis of things like ice-slab thickness when the general firn structure (zonal classification in this case)

already independently known, effectively requiring a third radar dataset (GPR) or other extensive in-situ measurements.
I find Section 3.1 to be lacking in purpose, in part because it reads something like a failed attempt to distinguish the zonal classification based solely on reflectometry; it is doubly unconvincing due to insufficient error analysis. In lines 201-203 there are claims about which model fits better where, but there is not even an analysis of the relative RMS misfits of the two models in the two zones. At a bare minimum, such basic model-data misfit analysis is needed to make any claim about what model fits where. However, given the section title I was hoping it would essentially answer the other main point raised above. I understand that this may be beyond the scope of the work or not supported by it, but then I am left wondering what this section really adds (perhaps adding some error analysis would change my mind, and I could better understand what we could conclude out of this section). Perhaps some roadmap under the general "Results" heading could help as well.

- Thank you for the comments and suggestions. We agree that there are limitations to this method, particularly without GPR measurements. However, with dual-frequency reflectometry on its own, one would be able to determine if layering is present in the near-surface firn, because in the case with layering, the radar response is dispersive (i.e., frequency-dependent). For example, if we consider the case of homogenous firn without layers, the assumption made is that the coherent power (Pc) is mainly sensitive to surface density variations. In this case, the radar response is non-dispersive, because the strongest reflection is that from the surface, and mono-frequency radar data is sufficient to invert the surface return for density. However, a dual-frequency system would be able to confirm whether Pc is mainly affected by surface density or the presence of ice slabs, because Pc would appear to be the same for both radar returns in the absence of ice slabs. For this work, one of the goals was to apply this dual-frequency approach to show that, indeed, the coherent power is not representative of surface density. In regions without a priori knowledge of the general firn structure, the dual-frequency method would provide insight into the presence of ice slabs at characteristic depths within the near-surface (if both systems utilize different bandwidths).

  The firn zone boundaries were derived completely independent of the GPR measurements, by comparing the balance between the coherent and incoherent power of the total surface power recorded by the MARFA airborne radar. What we find are changes of the near-surface structure consistent with these zonal boundaries, validated by the GPR data and imagery as well. To better communicate that this auxiliary GPR data was used for validation, we will reorganize section 3 by moving current Section 3.1 to the last part of this section. We will also rename current Section 3.1 to reflect its purpose in this study, which is to serve as ground-truth and validation of our interpretation of the dual-frequency airborne radar datasets. Thus, the dual-frequency airborne radar results would then be the focus of this section. The thin layer model was developed also for validation purposes and does not form the main focus of this section, although we do provide some error/sensitivity analysis of ice slab thickness (from the GPR) and firn density (from the firn cores) as inputs to the model. We believe that this is sufficient for the purposes of the model and additional error analysis is beyond the scope of this work.

To better highlight what we can learn in the absence of measurements such as GPR, we will add additional text to discuss the advantages of a dual-frequency system compared to a mono-frequency system, as mentioned above, and a roadmap in the 'Results' section as suggested. We will also clarify the limitations of this method for future applications to other regions of interest. We believe that these edits would hopefully make clear the purpose of these subsections and the overall goals of this work.

Line Comments:

52: I would suggest removing the IPR acronym. These are all ice-penetrating radars, and the terminology is unnecessarily confusing.

- Thank you for the suggestion. We will make clear whether we are referring to land/surface-based vs. airborne ice-penetrating radar in the text.

57: What such methods? The low frequency ones?

- Yes, this is referring to low frequency methods for near-surface characterization. We will clarify this in the text.

58: I am skeptical of this claim—does Mars have surface melt? Could ice lenses and slabs be possible? While other dual-frequency applications matter there, the relevance of this study should be justified or the line should be deleted.

- Here, we are only referring to general near-surface properties on Mars (e.g., thin surficial layering of CO_2 ice) investigated with lower radar frequencies. The main idea here is to refer to studies where near-surface properties can be studied even if features cannot be directly resolved.

69: What is compact ice? It is not defined nor is it a common term. I think it just means glacier ice as opposed to firn. Perhaps "fully compacted" or "fully densified" would be more appropriate. While I put this as a line comment, I think it is important to change "compact" throughout the paper, since it is not quite the technical term and the word has multiple plain-language meanings.

- Thank you for pointing this out. We will change the terminology here and throughout the manuscript.

82: What does dual phase mean?

- It refers to the ability to record data from either the left or right antennas separately on the survey plane (see Scanlan et al., 2020 and Young et al., 2016), which is the main difference between the HiCARS and MARFA systems. However, this does not change the interpretation and analysis of the data for the purposes of this work.

Scanlan, K. M., Rutishauser, A., Young, D. A., & Blankenship, D. D. (2020). Interferometric discrimination of cross-track bed clutter in ice-penetrating radar sounding data. Annals of Glaciology, 61(81), 68-73.

Young, D. A., Schroeder, D. M., Blankenship, D. D., Kempf, S. D., & Quartini, E. (2016). The distribution of basal water between Antarctic subglacial lakes from radar sounding. Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences, 374(2059), 20140297.

99: There are plenty of homogeneous media for which the arguments in line 100 apply—perhaps just delete this sentence

- We will remove this sentence.

Table 1: The layout here is confusing. I think I would have understood better if the epsilon_eff column were deleted and there were separate columns for z_0 for firn and for ice. Also should specify that this is not a universal firn number—it assumes 410 kg/m^3 or something similar.

- Thank you for the suggestions. We will reformat this table to make it easier for the reader and specify the corresponding density value that is representative of the chosen permittivity for firn in this particular case.

115: It would be helpful to have a half sentence about why the bin size (in spatial terms) is different for the different systems.

- We will include this information.

126: RMS height of what? I guess this should be surface elevation

- It is the standard deviation of the surface topography measured along a profile (see Shepard et al., 2001).

  Shepard, M. K., Campbell, B. A., Bulmer, M. H., Farr, T. G., Gaddis, L. R., & Plaut, J. J. (2001). The roughness of natural terrain: A planetary and remote sensing perspective. Journal of Geophysical Research: Planets, 106(E12), 32777-32795.

129: The hypothesis that the return power variation is dominated by variations in r^2 is a large and critical assumption that is brushed aside too flippantly. I guess there was some work in Rutishauser et al., 2016, to justify that it is not dominant, but I think it is a bit too important to be relegated to a reference, since strong dependence on the roughness may invalidate any conclusions. Addressing this could be as simple as estimating the maximum variation resulting from a realistic range of roughnesses compared to the variation in return power.

- Thank you for the comment. We agree that this is an important assumption, as strong dependence on surface roughness will affect the coherency of the signal. Looking at Figure 3 of Rutishauser et al., 2016, the majority of the laser-derived roughness values are concentrated at σ_h = 0.05 m. Propagating this value into Eq. 2 of this manuscript, specifically the exponential part of the equation representing the effects of surface roughness, we find that this contributes 0.07 dB to the coherent power (Pc). On the other hand, the effects of r^2 vary on the order of tens of dB (Figures 3 and S5). Moreover, the σ_h values from laser altimetry in Rutishauser et al., 2016 were derived using a baseline of 171.5 m. We expect the surface roughness at the wavelength-scale of interest (λ = 5 m) to be much smaller, because the surface roughness scales with the baseline length scale of interest (Shepard et al., 2001). Thus, the 0.07 dB surface roughness contribution to Pc already represents a highly conservative value. We can incorporate this calculation into the manuscript as well.

139: Is this a typo? Why exclude rock based on aircraft elevation rather than imagery, etc.

- This is ambiguous. We will clarify that this refers to surface elevation and not aircraft elevation.

143: At least a brief overview of the GPR system belongs here—the reader should not have to go to Rutishauser et al. just to find out the frequency

- We will add some relevant details of the GPR system here.

155: Layers of what, and should this be i.e.? Generally I would assume density is the only important factor in such shallow reflections—if not, what else should be included.

- Thank you for the question. This refers to any type of layering within the near-surface that results in a dielectric contrast strong enough to generate interference with the return from the surface. This can be governed by density variations but also any phase changes in the firn, such meltwater/brine. We will remove the parentheses and its text, because this sentence should be agnostic to any specific assumptions of the near-surface in this part of the section.

326: potentially insightful

- We will make this edit in the text.

391: Rephrase slightly to clarify that the ambiguity is due to tradeoffs between density and layer thickness

- We will reword this line to mention this.

395: Caution against?

- We will make this edit in the text.

411-420: I would highly recommend moving this paragraph upward into discussion—I do not find it to be particularly convincing, and I don't really think it is a conclusion as such. It is not *my place to demand such a change, but take this as a stylistic suggestion of a way to make the paper more impactful.*

- Thank you for the suggestion. We will explore where in the discussion section this paragraph might fit better.