Spatial characterization of near-surface structure and meltwater runoff conditions across Devon Ice Cap from dual-frequency radar reflectivity
Chan et al.

Locations where 'revised' is mentioned below, where applicable, refer to locations in the revised clean manuscript (i.e., without tracked changes).

<u>Response to comments from Reviewer 1:</u>

Comment	The introduction could benefit from a small introduction on the Devon ice cap and why it is a particularly good place to characterize the firn column. Either in a new paragraph (which I would prefer) or incorporated in one of the existing paragraphs.
Location	Introduction
Response	We added some introductory text on Devon Ice Cap in a new paragraph.

Comment	It would be a nice addition to have an overview map of the Canadian arctic or Canadian-Greenlandic arctic pointing out the location of the survey area. This would give the reader a much better impression of where the Devon ice cap is located.
Location	Figure 1
Response	We added an overview map to Figure 1 that indicates the location of Devon Ice Cap.

Comment	I also would suggest finding a better solution with the contour lines and their elevation labels. They appear very chaotic at the ice caps margins, which is rather confusing than helpful information. The same applies to all other figures (also in the supplement; S4) in which the contour lines are shown. Maybe only displaying contour lines only above 600 m would make the plot less overloaded.
Location	Figure 1
Response	We cleaned up the contour lines at the ice cap margins by only including contour lines above 600 m elevation for all figures where applicable.

Comment	It would also be good to state what kind of satellite image you are using as a background image.
Location	Figure 1
Response	This was a Landsat image. We included this information in the caption.

Comment	Please explain the symbols in the table caption (e.g., that range resolution is z_0, etc.). In addition, but very minor: a hline between the two systems would be nice to immediately see which z0 belongs to which system.
Location	Table 1
Response	We redefined the symbols in the caption and added the hline in the table.

Comment	What about the following idea: To give the reader a better understanding of the different depth resolution of the radar systems and which parts of the firn column are affected, one idea would be to somehow draw or indicate the depths that HiCARS & MARFA and MCoRDS3 resolve in Figure 2b.
Location	Figure 2 (initial) / Figure 4 (revised)
Response	Resolution depths for HiCARS2 were initially included but removed from the plot, because they relied on assumptions made about the firn column, such as firn permittivity/density and layer thickness. This was a motivation for including Table 1 in the main text. However, we agree that having this drawn on the figure could be very useful for visualizing the resolution depth. We added resolution depths for HiCARS2/MARFA and MCoRDS3, and indicated in the caption the assumptions used to calculate that depth.

Comment	I think the figure could be better arranged if, for example, (a) and (b) were in a row and (c) below. Then the subfigures would be bigger and the whole figure would take probably less space in the document at the same time. The same could be done with Figure 4.
Location	Figures 3 and 4 (initial) / Figure 2 and 3 (revised)
Response	We rearranged the subplots for these two figures as suggested.

Comment	Shouldn't the label of the colorbar be "dB" instead of "db"?
Location	Figure 3 (initial) / Figure 2 (revised)
Response	We changed “db” to “dB”.

Comment	I would suggest a different color scale, preferably linear rather than divergent. This is because in the HiCARS display, for example, the transition from -10 to -15 dB is shown as a weak color change, while from -20 to -25 dB there is a strong color change (yellow to blue). Therefore, I would suggest a linear graded color scale to better interpret the changes in dB based on a color scale across the different data sets.
Location	Figure 3 (initial) / Figure 2 (revised)
Response	We changed the color scale to a linear graded scale across the surveys.

Comment	Caption: define again that interquartile ranges is IQR and P_c surface coherent power (as in Fig. 3).
Location	Figure 4 (initial) / Figure 3 (revised)
Response	We redefined these in the caption.

Comment	I have a question regarding the ice slab thicknesses in Zone II. In Line 336 you state that the HiCARS/MARFA system captures the entire thickness of the ice slabs. Maybe I have missed it, but why is that the case and how do you know that the ice slabs along these radar profiles are not thicker than the range resolution of the system? My next question is very similar and refers to the average ice slab thicknesses. You calculated a mean ice slab thickness based on the range resolution of the two different radar (groups). Wouldn't it be rather a minimum average ice slab thickness? Because since you are only analyzing the surface return within the limits of the range resolution of the radar system, you cannot estimate if the ice slab continues with depth and is thicker, right? For me it seems that based on the surface GPR data it is assumed that the ice slabs in this region are not thicker as what is for example shown in Figure 2b. However, it might nevertheless be possible that thicker ice slabs might be present along the airborne radar profiles where no surface radar data exists. I think this should be clarified and also mentioned in the uncertainty section.
Location	Discussion
Response	On Devon Ice Cap, the Zone II/III boundary represents the transition from a region with firn to one without firn, which is validated by the Landsat imagery (Fig. 5). This spatial boundary also represents where the maximum ice slab thickness is obtained over Devon Ice Cap, because ice slabs grow in thickness from higher to lower elevations (e.g., MacFerrin et al., 2019) but shouldn't exist beyond the Zone II/III boundary due to the lack of firn. In other words, the Zone II/III boundary constrains the maximum ice slab thickness on Devon Ice Cap. In addition, we believe that our derived ice slab thickness represents an average range of values for Devon Ice Cap. If ice slabs are thicker than the range resolution of HiCARS2/MARFA in Zone II, we would expect a change in the pattern of Pc, particularly near the Zone II/III boundary. However, it remains fairly consistent throughout and thus also consistent with the interpretation that HiCARS2/MARFA observes a 4-layer medium in Zone II. However, we do acknowledge

	in other regions, particularly in Greenland, ice slabs can certainly be thicker than the range resolution of both radar systems. In that case, the average ice slab thickness derived via this method could represent a minimum average, depending on the location of the firn line and how Pc behaves near this boundary. We added some text to discuss the uncertainties/limitations of the approach, as suggested, in a new Section 4.2 within the Discussion section.

Comment	Here now appears a reference to the background satellite image, but the coordinates are missing. Again, I would prefer to get rid of the contour lines and labels below a certain depth.
Location	Figure 5
Response	We added coordinates and removed contour lines below 600 m elevation.

Comment	Figure S4: Please mention once more in the caption that P_c is coherent specular and P_n incoherent/scattered. I'm sure many readers don't, but I often have the problem that I forget the abbreviations in the text while reading and then have to look for them again in the text when they appear in a figure.
Location	Figure S4 (initial) / Figure S2 (revised)
Response	We defined Pc and Pn in the caption.

Comment	I think that Operation Ice Bridge should be mentioned here as well in addition to the University of Kansas. Moreover, I would suggest using the acronym MCoRDS3 instead of just MCoRDS throughout the document.
Location	Line 84-86 (initial) / Line 94-96 (revised)
Response	We added Operation Ice Bridge and changed MCoRDS to MCoRDS3 throughout the manuscript. We also changed HiCARS to HiCARS2.

Comment	With respect to the factors affecting permittivity, I think that temperature and the anisotropy due to the orientation of the ice crystal fabric should also be mentioned (although COF may not be so important in the firn column). In that sense you could additionally cite for example Fujita et al. (2000)
Location	Line 99-100 (initial) / Line 110-111 (revised)
Response	We included temperature and ice crystal fabric as factors affecting permittivity, citing Fujita et al., 2000.

Comment	You mention that “[...] surface roughness is not the main contributor to surface scattering over DIC (Rutishauser et al., 2016).”. It would be interesting to mention in one sentence why this is not the case. Especially because this assumption is important for the interpretation of the results.
Location	Line 128, 177-178 (initial) / Line 137-146 (revised)
Response	We agree that this is an important assumption for interpreting the results. Rutishauser et al., 2016 showed that the incoherent power is mainly governed by volume scattering from the ice layers as opposed to surface roughness. Looking at Figure 3 of Rutishauser et al., 2016, the laser-derived roughness values in Zone II are concentrated at σ_h = 0.09 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 0.22 dB to the coherent power (Pc). On the other hand, the effects of r^2 vary on the order of tens of dB (Figures 2 and S3). 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 0.22 dB surface roughness contribution to Pc already represents a highly conservative value. We added this calculation and explanation into the manuscript.

Comment	Here you state that: “Previous applications of the RSR method have empirically shown that an aircraft roll of 2 to 3° allows for a stable coherent radar return.” Is there a reference for this?
Location	Line 137-139 (initial) / Line 158 (revised)
Response	We included the justification for this and its reference.

Comment	The airborne radar data in your study is also "ground-penetrating". From what I understood you refer to land-based or surface radar in this section. Therefore I would suggest making clear that all radar surveys are ground penetrating and some are airborne and this one is land-based/surface radar data.
Location	Line 141 (initial) / Line 161 (revised)
Response	We clarified the terminology to distinguish between surface-based radar and airborne ice-penetrating radar through the manuscript.

Comment	I am not sure if I missed it, but is the difference between the old and refined Zones shown somewhere? If not, I think it should be (maybe in the Supplement). I guess the old Zones are those displayed in Rutishauser et al. (2016) in Figures 1a and 2?
Location	Line 248-252 (initial) / Line 234-238 (revised)
Response	That is correct. The old zone boundaries are those in Figure 2 of Rutishauser et al., 2016. We included the old and new zone boundaries in the Supplement section, Figure S2.

Comment	Here you refer to the Discussion Section but I think it would be also good to refer to Figure 5.
Location	Line 252-254 (initial) / Line 238-240 (revised)
Response	We updated this to point to Figure 5.

Response to comments from Reviewer 2:

Comment	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.
Location	General
Response	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 reorganized section 3 by moving Section 3.1 (of the initial manuscript) to the last part of the Results section. We also renamed initial 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 the Results 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 added new Section 4.2, to discuss the advantages of a dual-frequency system compared to a mono-frequency system, as mentioned above, and a roadmap under the general Results heading as suggested. We also clarified the limitations of this method for future applications to other regions of interest in Section 4.2. We believe that these edits would hopefully make clear the purpose of the Results subsections and the overall goals of this work.

Comment	I would suggest removing the IPR acronym. These are all ice-penetrating radars, and the terminology is unnecessarily confusing.
Location	Line 52 (initial)
Response	We opted to keep the IPR acronym but clarified throughout the manuscript whether we are referring to surface-based radar or airborne ice-penetrating radar. We also removed the text in parentheses.

Comment	What such methods? The low frequency ones?
Location	Line 57 (initial) / Line 63 (revised)
Response	Yes, this is referring to low frequency methods for near-surface characterization. We clarified this in the text.

Comment	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.
Location	Line 58 (initial) / Line 62 (revised)
Response	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. We clarified this in the text.

Comment	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.
Location	Line 69 (initial) / Line 72 (revised)
Response	We changed the terminology here and throughout the manuscript.

Comment	What does dual phase mean?
Location	Line 82 (initial) / Line 92 (revised)
Response	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 HiCARS2 and MARFA systems. However, this does not change the interpretation and analysis of the data for this work.

Comment	There are plenty of homogeneous media for which the arguments in line 100 apply—perhaps just delete this sentence.
Location	Line 100 (initial)
Response	We removed this sentence.

Comment	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 z0 for firn and for ice. Also should specify that this is not a universal firn number—it assumes 410 kg/m3 or something similar.
Location	Table 1
Response	We reformatted this table to make it easier for the reader and specified the corresponding density value that is representative of the chosen permittivity for firn in this particular case.

Comment	It would be helpful to have a half sentence about why the bin size (in spatial terms) is different for the different systems.
Location	Line 115 (initial) / Line 123-125 (revised)
Response	We added this information.

Comment	RMS height of what? I guess this should be surface elevation.
Location	Line 126 (initial)
Response	It is the standard deviation of the surface topography measured along a profile (see Shepard et al., 2001).

Comment	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.
Location	Line 129 (initial) / Line 137-146 (revised)
Response	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 the laser-derived roughness values in Zone II are concentrated at σ_h = 0.09 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 0.22 dB to the coherent power (Pc). On the other hand, the effects of r^2 vary on the order of tens of dB (Figures 2 and S3). 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 0.22 dB surface roughness contribution to Pc already represents a highly conservative value. We added this calculation and explanation into the manuscript.

Comment	Is this a typo? Why exclude rock based on aircraft elevation rather than imagery, etc.
Location	Line 139 (initial) / Line 159 (revised)
Response	We clarified that this refers to surface elevation and not aircraft elevation.

Comment	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
Location	Line 143 (initial) / Line 163-164 (revised)
Response	We added some relevant details of the GPR system.

Comment	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.
Location	Line 155 (initial)
Response	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 as meltwater/brine. We removed the parentheses and text contained within, because this sentence should be agnostic to any specific assumptions of the near-surface in this part of the section.

Comment	"potentially insightful"
Location	Line 326 (initial) / Line 313 (revised)
Response	We made this edit in the text.

Comment	Rephrase slightly to clarify that the ambiguity is due to tradeoffs between density and layer thickness
Location	Line 391 (initial) / Line 426-427 (revised)
Response	We reworded this sentence and incorporated it into new Section 4.2.

Comment	Caution against?
Location	Line 395 (initial)/ Line 427 (revised)
Response	We changed this in the text.

Comment	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.
Location	Line 411-420 (initial) / Line 444-450 (revised)
Response	We moved this to new Section 4.2 in the Discussion.