

The Cryosphere Discuss., referee comment RC2
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Comment on tc-2020-380

Anonymous Referee #2

Referee comment on "Penetration of interferometric radar signals in Antarctic snow" by Helmut Rott et al., The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-380-RC2>, 2021

General Comments:

In the manuscript "Penetration of interferometric radar signals in Antarctic snow" the authors study the relation between radar penetration depth into Antarctic snow and the interferometric coherence obtained from the single-pass InSAR mission TanDEM-X. They apply great effort for vertical alignment of different elevation models. For inversion they apply a model developed by (Dall, 2007) which assumes an uniform scattering efficiency of the snow volume. Based on snow pit measurements and depth-resolved radar backscatter models they conclude that despite a strong vertical variability of the scattering efficiency the depth-integrated backscatter signal represents well the model suggested by (Dall, 2007). They also found that modeling of the backscatter signal with the SMRT model based on grain size and layer thickness cannot explain the strong incidence angle dependence of the observed backscatter signal.

These findings make the manuscript in general suitable for publication in The Cryosphere. However, I have major concerns about the objectiveness of presented results and think that the manuscript requires a major restructuring to present and to focus on the most relevant results listed above. Below I first detail my main concerns on section 4-6, followed by minor comments and technical corrections. I would also suggest the authors to use the common structure of Data - Method - Results - Discussion - Conclusion and to write the manuscript more concise, instead of the currently used sequential form of sub-method-sub-results mixed with interpretation and discussion.

Major comments:

Section 4.1: This section about modeling the backscatter contributions from different layers lacks a thoroughly analysis and results seem to be presented in a selective manner. I understand that modeling the backscatter signal from the complex snow structure is challenging. Therefore, I think results should be presented in a more objective way to allow the reader to draw his own conclusions. Specifically, I have the following comments:

- line 268: Why did the author choose 25 layers and not the 30+ layers shown for the snow pits?

- line 370: When adopting the density profiles from firn cores, how was the layer thickness chosen? Fig. 3 indicates that, despite using the same firn core, different layer thicknesses

were used below 2 meters. Please also mention that half of the simulated backscatter contribution originates from the adopted firn core. Only the upper half of simulated backscatter is based on snow pit data.

- Fig. 3: What are the "spikes" in Fig. 3(a) and (c)?

line 388: "Fig. 3 shows ... simulations for 40° ... of Pit 2 and 4." Why were these two pits chosen for the figure? Why not showing simulations for all snow pits (at least for one incidence angle)?

- line 393: "Both values differ less than 0.3 dB from the mean values of the 2013 and 2014 TDM scenes (40 deg)": This information is meaningless, considering that the standard deviation of the 2013 and 2014 measurements is around 1 dB; further, as stated by the authors, for 22 degree incidence angle, the model deviates 3 to 8 dB from the measurements and requires more tuning for a reasonable agreement.

line 395-397: The modeled two-way penetration depths for Pit 2 is 4.72m and 7.25m for Pit 4. The distance between the snow pits is about 7 km, but modeled results differ by 2.5 meters apparently due to different snow properties. Therefore I think it is meaningless to compare the penetration depth of Pit 2 with similar results from East Antarctica (Rott 1993) except for increasing the self-citation index. Please remove the reference or provide a more traceable comparison.

To present more objective results, I suggest to show a scatter plot presenting modeled vs. simulated backscatter intensity for all(!) 30 or 40 backscatter values listed in Table S3. Then the authors can discuss in a more objective manner what they think what causes the strong discrepancies. Different symbols or color could allow to separate different incidence angles and test sites(e.g., you could use numbers as plot symbols referring to the snow pits).

Section 5.2 is extremely hard to read. The main message of this section is not clear and backscatter results and discussions are mixed with inversion results of the penetration depth. Results from the snow pit locations are mixed with area-wide maps and scatter plots representing the same variables. Please restructure this section thoroughly and present only the most relevant results (estimation of interferometric bias) in a well-structured way. Currently, it's not clear to me why you also discuss and interpret backscatter values in this section. Specific suggestions:

- line 522-540 should go to the methods.

- Figures and tables: Why not showing a full page or full column figure with six rows and two or three columns of subplots (6 TDM scenes x 3 types or scatter plot) where each subplot shows a scatter plot of 1:(γ_{vol} over dh), 2:(h_{binv} over dh) and possibly 3:(σ_0 over dh). For each subplot, you could indicate the datapoint corresponding to a snow pit with a black symbol. This would then provide a solid basis for discussion of the inversion results with different incidence angles and baselines. At the same time you avoid discussion of mean values calculated over the strongly inhomogeneous areas of the LGA.

- I think you could merge Section 5.2 with 5.3.

- following a result-section about penetration estimation, you could - if it's worth - add a section presenting result on backscatter.

Minor comments:

abstract, l.23. "The average depth-dependent... can be approximated.." It's not clear if that's a general statement or a finding of the study. Please indicate.

abstract l.29: "The angular gradients of the backscatter intensity": Unclear what "angular gradients" are. Looking at line 420-427 I think you mean that simulated backscatter data do not match the backscatter measurements at different incidence-angles?

line 42-44: "Backscatter contributions ... within a volume scattering medium, observed under slightly different incidence angles, are causing a spectral wavenumber shift and decorrelation (Gatelli 1994)": I think that two different things have been mixed in this sentence. The observation under slightly different incidence angles (or InSAR nbaselines) causes a phase ramp (flat earth phase) modulated by topography (topographic phase). The sum of these two phases causes the spectral wavenumber shift which can be corrected for by spectral filtering (Section III-A in Gatelli 1994) and which is not caused by volume scattering. In my opinion, another effect is volume decorrelation (Section III-B in Gatelli 1994) which occurs when different scatterers exist within the same resolution cell but at different viewing angles, hence they scatter with different inSAR phases which sum up coherently but random, therefore causing decorrelation.

line 75: "data from optical satellite sensors": which?

line 96: "... bare ice appears on the surface (Fig. 1). The blue ice area (BIA)..." : Could you indicate consistently the location of the BIA on the map? The captions indicate the BIA with "B". Maybe, replace it with "BIA". or/and change "(Fig. 1)" do "(BIA in Fig. 1)". Could you add an arrow to the map indicating the wind direction and the location of its measurement? Possibly, also add the location of the stakes to the map as e.g., little black dots. As you are referring later to the ALE camp, could you also add it's location to the map?

line 119-124: Could you add the transect, and possibly the thickness of the firn layers, to Fig. 1?

line 124 and 137: "raw SAR data". What do you mean with raw SAR data? Level 0 raw data or level 1b CoSSC data?

line 140: "the SAR amplitude, the backscattering coefficient": Are they not identical? Or do the authors mean different normalizations? Did the authors consider a backscatter dependence or normalization with respect to the local topography? If no radiometric terrain correction was applied, please justify and mention the expected error. See [D. Small, "Flattening Gamma: Radiometric Terrain Correction for SAR Imagery," in IEEE Transactions on Geoscience and Remote Sensing, vol. 49, no. 8, pp. 3081-3093, Aug. 2011, doi: <https://doi.org/10.1109/TGRS.2011.2120616>.]

line 190: Even though stated in the introduction, I would repeat the information that snow pit measurements were done in Dec. 2016. This information is relevant for comparison of

the snow pit data with the TanDEM-X data from different years.

line 198 (also 371): "grain size": The next sentence indicates you measured "D_max"? Please specify. Maybe, add a reference, e.g. Mätzler et al (2002) "Relation between grain-size and correlation length of snow": <https://doi.org/10.3189/172756502781831287> or the references to Colbeck 1990 or Armstrong 1993 therein.

line 209: "snow age following from different accumulation rates": Did you estimate accumulation rates or snow age from the snow pit measurements or from the accumulation stakes?

line 221: "accumulation rate near the ALE camp" Do you refer to snow pit P3 or to the accumulation stakes?

line 388: To clarify that refraction has been considered, I suggest to write "backscatter simulations for $\theta_i = 40^\circ$ ". Could you mention in line 338 how you obtained the refraction angle θ_r from snow density and θ_i ?

line 422: "The need for different parameter settings ... is an indication for structural anisotropy": Please rephrase. Neglecting a possibly(!) existing structural anisotropy (please define! see next comment) could be a possible reason, amongst others(!), why the backscatter model does not fit the observations.

line 423-427: What do you mean with "structural anisotropy"? Do you mean the structural anisotropy of the microstructure (e.g. Leinss et al. "Modeling the evolution of the structural anisotropy of snow" *The Cryosphere*, 14, 51–75, 2020 <https://doi.org/10.5194/tc-14-51-2020>) or do you mean that horizontal layers with different density create a structural anisotropy of the snow pack (in the extreme case ice layers)? For such a layered snow pack I would expect a strongly angle-dependent backscatter dependency due to directional reflection at the layer-interfaces. Temperature gradient seems to be relatively low, but (Montagnat et al. (2020) "On the Birth of Structural and Crystallographic Fabric Signals in Polar Snow: A Case Study From the EastGRIP Snowpack". *Front. Earth Sci.* 8:365. doi: <https://doi.org/10.3389/feart.2020.00365>) found for similar snow conditions a strong structural anisotropy of both, the c-axis and the microstructure. given the availability of VV and HH polarized acquisitions you could quickly check the copolar phase difference (Leinss, S., Löwe, et al.: Anisotropy of seasonal snow measured by polarimetric phase differences in radar time series, *The Cryosphere*, 10, 1771–1797, <https://doi.org/10.5194/tc-10-1771-2016>, 2016.) to estimate whether a strong structural anisotropy of the microstructure exists or whether you interpret the model-data discrepancy of the backscatter signal with incidence angle dependence through horizontal density variations as suggested by (Tan et al. 2017). In the latter case, I guess, without knowing the surface-roughness of each layer it seems impossible to model the precise incidence-angle dependent backscatter response of each layer. For discussing this effect,

the work by Oh, Ulaby et al. could help: [Oh, Yisok, Kamal Sarabandi, and Fawwaz T. Ulaby. "An empirical model and an inversion technique for radar scattering from bare soil surfaces." IEEE transactions on Geoscience and Remote Sensing 30.2 (1992): 370-381.]

line 425: "angular gradients of the backscatter": Here and other places (especially also in the abstract) angular could refer to any direction or angle. Please be specific: I would rephrase that to "incidence angle dependence of the backscatter coefficient". Same for "angular difference".

line 484: What is LGA? Please introduce abbreviation (level glacier area).

line 484: Why did the authors exclude the BIA?

line 468: "coherence phase...is uniquely defined by the coherence magnitude" - That is only true for small penetration depth compared to the height of ambiguity which is given in your case, please clarify. See also Fischer et al. "Modeling Multifrequency Pol-InSAR Data From the Percolation Zone of the Greenland Ice Sheet" in IEEE Transactions on Geoscience and Remote Sensing, vol. 57, no. 4, pp. 1963-1976, April 2019, doi: <https://doi.org/10.1109/TGRS.2018.2870301>.

Line 470-480: You describe two models (Dall 2007, Zebker 2000) to estimate the penetration depth from the coherence but both models provide different results. Could you provide reasons why you have chosen the model of Dall 2007 and why you think that this model describes better the relation between coherence and penetration depth?

Line 543-544: Please specify the wavenumber instead of using large / small.

Line 544: "The scene T2018 ... shows the smallest gradient" I see more a point cloud than a clear linear relation in Fig. 6b. Anyway, you write "as expected according to theory." Could you specify which theory you are referring to? Eq. 16?

Line 633-644: These lines reads like a general introduction rather than a discussion of your results. I would remove these lines.

Line 656-663: Strongly shorten this section about the structural anisotropy to max. 1 sentence. You cannot start with "This can be explained.." to finish with "However, such layers are absent... excluding anisotropy as a main explanation"

Section 7: Please split this section into two sections, 7) Discussion and 8) conclusion. Line

689: I think here starts the conclusion.

I think it is worth mentioning in the conclusion that despite its simplicity the approach from Dall 2007 provided reasonable results.

Line 682-688: Please check references with cited content. They might have been flipped.

Line 702-708: I don't see a point of citing the work of Parrella here because the authors have excluded the structural anisotropy as a reason for the observed incidence-angle dependent scattering.

technical corrections:

line 64: "DEM" is already defined in line 33.

line 77/78: "a well equipped field station" I guess, this is the "ALE camp" to which you are referring later. I think this is a better place to introduce the abbreviation "ALE" or "ALE camp". Currently, mentioning Antarctic Logistics & Expeditions sounds a bit like company advertisement, but I guess the name is important to define the location of the ALE camp.

Fig. 2: Please check whether the symbol size of grain shapes (and also fonts) is appropriate for the final layout.

line 286: Here you introduce the abbreviation "IC2" which is sporadically used later. I think it's more consistent to define the abbreviation at the very first location where IceSat-2 is introduced and use it then consistently. Or avoid the abbreviation if you prefer.

Equation 4 (and other equations, variables and mathematical expressions): Only variables should be italic; functions "exp" and descriptive indices like "tot" should be upright. Latex: $P_{\text{tot}}^{\text{exp}}$...

line 334: 37% reads a bit random. I guess you mean "attenuated to e^{-1} ".

line 485: level areas -> horizontal areas.

line 494: "high values indicate large scattering elements": ... or steep slopes / layover / strong topographic variations.

Line 502 - referring to Fig. S2: Could the authors extend the color scale to cover the full range of backscatter differences? Possibly, clip the range at the 1 and 99% percentil to define the colorscale.

Fig. 4: Could the authors add the LGA mask to Fig. 4 for orientation?

line 507: "left of the camp" - do you mean ALE camp? As you are referring to the map, could you also indicate the location of the camp in Fig. 4 and 5?

line 509: "The angular difference... of ...-13.3.. is characteristic for" -> "The strong incidence angle dependence of the backscatter signal ... is characteristic for ... " (The current formulation implies that the specific values of the BIA are characteristic for ...)

line 517: "has an also impact" -> "also has an impact"