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We would like to thank the referees for the careful review of the revised version of the manuscript and their positive comments. We incorporated the suggested changes, as explained below point-by-point. In addition, we implemented a few minor editorial changes (wording, clarifications).

Referee comments are in italics, our responses and explanations on implemented changes are in normal font. The line numbers ("L") in the response refer to the revised version of the manuscript with changes tracked.

Authors Response to Referee 1

The authors should be commended for making this paper more readable for a broader audience. The structure of this revised manuscript is much improved, with clearer separation of the theory and assumptions that underpin the method and the application of it. The paper appears technically correct but would benefit from further clarifications to aid the reader and to ensure reproducibility. It would also be better to try to reduce the number of acronyms (perhaps in addition make a table in the appendix): it is jarring to try to recall what these are (particularly if they are used infrequently) and this interrupts the flow. Specific comments are:

Response: We wish to thank the referee for his useful comments and the suggestions for changes addressing further clarifications and the improvement of readability. We incorporated the proposed changes as explained below. Besides, we checked the number of acronyms and their distribution in the text. We removed a few acronyms. Many acronyms refer to satellites, sensors and satellite products and are quite common. Some of the other acronyms require detailed explanations, better to be provided in the context rather than in a separate table. Therefore we refrain from lining up the acronyms in a table.

Line 60: define SP-InSAR as single pass InSAR.

Response: Defined (L 36).

Line 127. It would really help to add a sentence here indicating why both the TDMgl and tiles are used in this study.

Response: Explained, adding the following information (L132 to L135): "We use the TDMgl DEM for topographic corrections and geocoding because it provides full spatial coverage whereas the DEMs of individual tracks have gaps, depending on the observation geometry. The data from individual tracks are used for studying the impact of particular InSAR configurations on the coherence, backscatter signatures and the penetration bias."

Line 140: 'geocoded rasters of the height error' – is this the Height Error Map from line 150 and comes directly from ITP?

Response: This corresponds to the Height Error Map (reference added L147)

Line 168: It would really help to add a sentence here to indicate IceSat / IceSat2 data are used to assess the temporal stability of the surface elevation and REMA is used to assess elevation bias because of its spatial coverage.

Response: We added the following explanation (L179, 180): "We use the ICESat and ICESat-2 data primarily for assessing the temporal stability of the surface elevation." The information regarding the use of REMA is explained in L196.

Line 214: 'At lower depth equi-temperature metamorphism takes over as dominant process for grain growth'. There are few measurements deeper than 2m and of these the grain type isn't showing strong

evidence for equitemperature metamorphism: these crystal types are also prolific higher in the pack which is dominated by temperature gradient metamorphism.

Response: We skipped this sentence.

Line 215: 'Estimates on accumulation rates are based on stake measurements (Rivera et al., 2014), the ice core between pit 2 and pit 3 (Hoffmann et al., 2020), and the summer melt crust in pit 5'. These accumulation rates and a brief description of how they were derived need to be presented, particularly as the discussion and even the abstract links the elevation bias to accumulation rates.

Response: Sources and numbers for accumulation rates are addressed in a paragraph at the beginning of Section 2. For better visibility we added a separate sub-heading (2.1, L92) referring to surface mass balance (that corresponds to net accumulation in the areas of positive mass balance) and included further information on accumulation in this subsection.

Figure 2: Colors in grain size legend still don't match the colors in the figure. E.g. on my computer smallest grain size in legend is #dde7f3, next smallest is #96b4f8. Lightest grain size in pit 5 is #b5bbde. I have not compared the hardness grayscale but would be hard-pushed to identify which category each would belong to. Would recommend switching hardness to different filling techniques (hatching). Please align the symbols used for crystal types with the International Classification for Seasonal Snow on the Ground – the symbol for 'faceted and rounded particles, closely packed' is not part of the classification and also is mismatched with the figure itself, which shows a closed form. You can download and use the fonts from https://cryosphericsciences.org/publications/snow-classification/.

Response: Fig.2 was revised taking into account these comments.

*Line 247: 'The total snow mass down to 2.04 m amounts to 0.80 m w.e'. From Table 2, mean density for P4 is 403.75 kg / m3. For a depth of 2.04 m, this equates to a water equivalent of 2.04*403.75/1000 m = 0.82 m. Does 0.80 m w.e. refer to just P4 or a mean of more than one pit?*

Response: This information refers to pit 4, as stated in the text. Number 4 of the hard layers (from the top) extends from 168 cm to 197 cm (with hardness R4) and 197 to 200 cm (R5), with rounding depth hoar below. We take the bottom of the hard layer (R5) as reference horizon, resulting in slightly revised numbers (L258): "Down to the depth of 2.0 m the P4 stratification shows four comparatively thick, hard layers with rounding depth hoar below. The total snow mass down to 2.0 m amounts to 0.81 m w.e."

Line 311. Add in Dall 2007 reference again to show equation 11 is derived there not here.

Response: For better explanation we added another equation (in response to a comment of referee 2) and another reference to Dall (2007) (L317 to L325).

Line 338. 'For the elevation bias estimate derived from the volumetric coherence we use the notation hbInv'. Is this the same as hb in equation 11? If so, hbInv should be in equation 11. My understanding is that hbinv is expected to be the same as hb provided that hs is itself error-free. Unfortunately it is not because of potential temporal changes and the correction of REMA to CryoSat2 with its penetration correction and the following sections look at this. It would be helpful to provide some similar indication (or a correction of my understanding) to the reader to set the scene for sections 4.1.2 and 4.1.3.

Response: Eqs. 10 to 12 (L320 to L328) are taken over from Dall (2007). These equations refer to the ideal case. In order to indicate the actual setting, we define in Section 4.1.1 notations for elevation differences derived from different topographic data and/or applying different registration procedures. We added a statement relating H_{bInv} of Eq. 12 and h_b of Eq. 13 (L354).

Line 340 'On these surfaces the raw TDM DEMs show vertical offsets up to a few metres because for these data an absolute height calibration is not performed routinely'. Add reference into section 2.1 as this is presumably the 0.7 to 3m height error on line 158. If it is not, please add in an appropriate reference and explanation.

Response: The values of 0.7 m to 3.0 m in the Raw DEMs are random errors at pixel scale due to phase noise which can be reduced by low-pass filtering (this is explained in L159 to L161). We added the following additional information in L161, L162: "The Height Error Map does neither account for the absolute height error (offset) in respect to a particular geodetic reference system nor for penetration related errors."

Line 377: 'theTDMgl' -> 'the TDMgl'

Response: Corrected, L394.

Line 380: 'Subtracting the TDMgl offset of -6.76 m yields a dh value of -6.67 m due to signal penetration' would benefit from a rephrase: subtraction of a negative gives a positive.

Response: We reformulated this statement (L397): "Subtracting the TDMgl offset of -6.76 m from the mean Δh value (0.09 m) yields an elevation bias (dh) value of -6.67 m due to signal penetration.

Line 407. Supplementary figures are out of order. S4 is referenced before S1.

Response: We deleted here the reference to S4 (L425).

Figure 3 and all relevant figures (especially Figure 5, difference a vs b and c vs d). Please put incidence angles in captions for ease of reading (rather than having to look them up in Table 1. The importance of incidence angle isn't evident before line 411, which is a long way past Table 1.

Response: We added the incidence angle values in the captions of Figure 3, Figure 5, Figure 7; Figure 8; Figure S1:

Line 417. This entire paragraph discusses supplementary figure S2. This is not supplementary and belongs in the same paper. Switching between files interrupts the flow of the paper.

Response: We moved Fig. S2 to the main paper.

Line 429. 'The coherence image of 6 May 2013 (Fig. 4) shows the lowest coherence (... < 0.7) on glacier sections with large signal penetration'. Where is the signal penetration shown? The following two sentences then contradict this. Coherence for BIA (surface reflection, no penetration) is 0.79 whereas the low accumulation areas (penetration) has coherence of 0.85 to 0.9. Please highlight the features of interest much more clearly.

Response: Revised text (L454 to L458: "The coherence image of 6 May 2013 (Fig. 4) shows the lowest coherence on glacier sections with the largest elevation bias located on Driscoll Glacier and near the ALE camp (γ_{tot} = 0.50 to 0.65). In the T2013 and T2014 (T2013/14) TDM images the coherence of the BIA is also comparatively low (mean γ_{tot} = 0.79) because of thermal decorrelation due to the low SNR. In the low accumulation areas surrounding the BIA the σ° values range from ..."

Lines 433-437 discuss the role of incidence angle in interpreting the relationship between coherence and backscatter, supported by Figure S3. Figure S3 belongs in the main body of the paper.

Response: We moved Fig. S3 to the main paper.

Line 438. 'shows the expected variations in dependence of the height of ambiguity and incidence angle'. Please state here what the expected variations are as these are not obvious.

Response: Information added (L465, 466): "..expected trend, i.e. i.e. decrease of γ_{Vol} with increasing baseline (decreasing H_a) at a given incidence angle."

Line 485. 'In order to check effects of the incidence angle the data derived from the T2013/14 and from the T2016/18 scenes are displayed separately' should come earlier, before Figure 5.

Response: ...Moved to Sect. 4.3 (482, L483).

Discussion. This isn't a criticism but merely noticeable that the first three paragraphs of the discussion cover material presented in the Appendix and supplementary material. It's a very interesting discussion but this gives the impression that the interesting material isn't in the main body of the paper. I'm not sure what to suggest here – perhaps no change needed.

Response: Thanks for this positive feedback. In order to improve the visibility of relevant information we moved Figs. S2, S3 and S5 to the main paper.

Line 555 – this paragraph appears without context. The structural anisotropy of Leinss et al is not the same as macroscale anisotropy (e.g. sastrugi) mentioned in the previous paragraph. This paragraph (if included) should be restructured to clarify the intended point.

Response: We shifted this paragraph in order to follow the paragraph reporting on HH and VV properties for intensity, coherence and penetration. We start the paragraph on co-polarized phase difference and coherence with explaining the motivation. In the revision we provide also references on co-pol phase observations in dry polar firn, add some specific numbers for the LGAS and conclude a statement on the optional use of such data (L610 to L634).

Line 574 – 'On the average' -> 'On average'

Response: ..."on average " should be correct

Line 580 – 'Both the dh and hbInv values indicate deeper penetration for T2018 compared to 2016, amounting on the LGA to 0.42 m, respectively 0.68 m' needs rephrasing. Does this mean e.g. 'Both the dh and hbInv values indicate deeper penetration for T2018 compared to 2016, amounting to 0.42 m for dh and 0.68 m for hbInv over the LGA'?

Response: Reworded (L639, L640): 'Both dh and h_{bInv} indicate a larger elevation bias (deeper penetration) for T2018 compared to T2016, amounting on the LGA to dh = -4.80 m for T2018 versus -4.38 m for T2016, respectively h_{bInv} = -5.18 m for T2018 versus -4.40 m for T2016.

Line 689. 'mean penetration depth which is deduced from the mean elevation difference' – how is this deduced? Does this mean that the mean penetration depth is assumed equal to the mean elevation difference?

Response: ...Explained (L753): " ....which is deduced from the mean elevation difference (dh) between the T2013/14 scenes and the REMA assuming that the 2-way penetration depth is equal to the elevation bias."

Line 697. 'smaller gain size' -> 'smaller grain size'.

Response: ..Corrected

Figure A1. Please explain how these simulations were carried out. It is not clear how the backscatter from below a layer is derived from total surface backscatter. How deep was the simulated profile? What was the lower boundary condition? Sufficient information must be given for these simulations to be recreatable.

Response: This was explained in lines 675 – 676. We added further explanation added (L735 to L738): "We use τ as tuning parameter in order to match the average observed and computed total backscatter intensity at the individual sites. ..... The computations were performed down to 20m

depth. Contributions to total backscatter from the layers below are negligible because of the dense medium effect and the attenuation in the layers above."

Figure A1. Not for this paper, but it would be really interesting to try to understand how the relative differences between the uniform volume approach and the layered scattering model approach translates to penetration bias. 3dB (line 704) is a large error. The error for pits 2-5 should be reported here rather than simply stating 'The RT simulations are able to reproduce the observed total backscatter intensity at snow pit sites 2 to 5'.

Response: Further explanation added (L766 to L769): "The RT simulations for $\theta_i = 40°$, using τ as tuning parameter, reproduce exactly the observed mean total backscatter intensity of the corresponding T2013/14 data at snow pit sites 2 to 5. For pit 1 the simulations for $\theta_i = 40°$ yield an underestimation of 3 dB, even when assuming consistently maximum stickiness ($\tau = 0.1$), ..."

(For snow pit sites 2 to 5 (40°) the simulations are matching the observations with 0.1 dB)

Line 707. 'Such an angular difference is typical for density-layered firn' – does this refer to the 1.3 or 5.9 dB?

Response: Explained (L772): "Large incidence angle dependence of backscatter is typical for density-layered firn."

Authors Response to Referee 2

General Comment

The authors thoroughly revised the manuscript. The revision is better balanced and provides a more objective interpretation of the numerous observations and results. The results are valuable for interpretation of radar penetration, coherence and backscatter response over ice sheets and glaciers. While the authors present promising results using the relatively simple Dall model to correct for radar penetration, they also address problems in simulating the backscatter response from stratified snow. Overall, I can recommend publication of the manuscript in TC. Below are some technical corrections which I encourage the authors to consider before submitting the final manuscript

Response: We wish to thank the referee for his useful comments and the suggestions regarding technical corrections or clarifications. We have incorporated implemented the proposed changes as explained below.

Technical corrections:

line 146: What do you mean by "absolute radiometric calibration and terrain-corrected geocoding" ? Do you mean, the backscatter signal was radiometrically terrain corrected? gamma_0 or sigma_0? Or was it only geometrically terrain corrected (orthorectified) as written above ("geocoded rasters of ... SAR amplitude")

Response: For clarification we replaced this sentence by (L153 to L155): "The backscatter intensity images show maps of the normalized radar cross section $\sigma°$. For the computation of $\sigma°$ effects of topography were taken into account for antenna pattern removal and for defining the actual size of the local scattering area." The notation "normalized radar cross section" is a standard term for absolutely calibrated backscatter values referring to the unit surface area.

Figure 2: Comparing the colorbar for grain size with the color-coded column "GS" appears as different color scales were used. Please correct.

Response: The colour scale was revised for better discrimination of the grain size and the hardness classes.

line 306: "Normalizing the coherence by the interferometric phase of the volume surface": Could you provide an equation make this sentence more comprehensible? Do you divide two complex numbers here.

Response: For better explanation we revised the text and added another equation (L317 to L325).

line 310: "As according to this relation [Eq. 10] the coherence phase, <gamma, is uniquely defined by the coherence magnitude": Eq. 10 does not illustrate that the coherence phase is uniquely defined by the coherence magnitude.

Response: See response to comment above. Eq. 11 (added) shows this relation.

line 397: "total normalized coherence": could you provide a reference to an equation which defines the "total normalized coherence"?

Response: "Normalized coherence" is a common term for the magnitude of the complex interferometric correlation coefficient (often simply called "coherence"). The total complex correlation coefficient is specified in equation 1. Revised text (L415, L416):" Fig. 3 shows an image of the magnitude of the complex interferometric correlation coefficient (the total normalized coherence) ..."

line 399: "excluding (...) and slopes smaller than 5° inclination": I guess you mean larger than 5°?

Response: Thanks for noting this. Corrected (L418).

line 422: "Large angular gradients" - See original comment by Referee #2: "angular gradients of the backscatter" Here and other places (...) angular could refer to any direction or angle. Please be specific: I would rephrase that to "incidence angle dependence". Same for "Angular difference" in Fig. S2. I suggest to delete "angular" in the caption as the different incidence angles are already given in parenthesis

Response: We replaced the word "angular gradients" in L442, L572, L772 and deleted the word "angular" in the caption of Fig. S2 (now Fig. 5).