

The Cryosphere Discuss., author comment AC1
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Comment on tc-2020-380

Helmut Rott et al.

Author 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-AC1>, 2021

Manuscript tc-2020-380, Author comment AC1

Response to Referee 1

We wish to thank the Referee for the valuable comments and suggestions which are very helpful for improving the manuscript. We address the comments below and explain how these will be taken into account in the revised manuscript.

Referee comments are in italics, our responses are in normal font.

General Comment: This paper concerns a new application of existing theory to estimate bias in InSAR-derived elevation measurements of ice sheets and glaciers caused by below-surface radar returns. The method is based on the relationship with volume scattering coherence, which can be determined from total coherence, knowledge of the signal to noise ratio and assumptions of coherence loss from smaller order terms. The estimated bias from volume scattering coherence measurement is compared with bias calculated from the difference between InSAR elevation measurements and REMA.

There is no question as to the importance of this study. Observation error of cryosphere changes from InSAR is essential, particularly given the high magnitudes observed (mean bias of around 5m) and potential for seasonal variation due to changes in the snow grain size and density. The consistency between the simulation results and observed bias is encouraging. This is a new, practical application of the theoretical work of Dall 2007.

Response: We appreciate the positive feedback on the scope of our work. This comment very well reflects the main objectives of the paper.

Main Comment 1: My main concern about this study is that the Dall 2007 model is not applicable where there is significant surface scattering. While this may be valid considering the snow surface, there are a number of ice lenses and crusts within the snowpack (Figure 2) that act in a similar way. It was not clear from the snow backscatter modeling (section 4.1) how these were simulated but possible these were explicitly taken into account (e.g. ice layer with 2mm air bubbles). Discussion of the limitations of the Dall 2007 model should be included as well as the limitations of the methodology of the snow backscattering modeling. The impact of the use a subjective grain size and assumed stickiness on the retrieved bias should be discussed.

Response: The Dall model is based on an exponential loss function. The validity of this model can be assessed by comparing the inversion of this model in terms of the InSAR elevation bias with the elevation bias resulting from the difference between vertically co-registered InSAR and optical elevation data (dh , as defined in Eq. 3). The assessment of the performance of the InSAR elevation bias derived from volumetric coherence for polar firn is actually the main objective of this project. The comparison of the elevation bias from the inverted Dall model with the dh -values shows on the average good performance, confirming the general applicability of this model for dry polar firn with different structural properties (Table 3, Fig. 7). However, there is some over-, respectively under-estimation of the retrieved elevation bias for sites with specific structural properties. Performance and limitations of the Dall model are discussed Section 7.

The inversion of the Dall model is not based on the multi-layer radiative transfer model that is applied in Section 4.1 for backscatter simulations. The inversion of such a model is not possible with the very limited set of input parameters. The main objective of the reported backscatter forward modelling activity is to assess the suitability of the exponential loss function for describing the vertical backscatter contributions of a layered polar snow/firn medium (line 403 to 407).

By now there is no backscatter model available that specifically accounts for the complex layered structure of polar firn. We are well aware of the shortcomings of the current model. Constraints of the backscatter model and of the input parameters for characterizing the specific properties of the individual layers are addressed in the sections 4.1 and 7. Nevertheless, the selected model allows demonstrating the impact of differences in scattering of individual layers, parametrized by effective grain size and stickiness, in order to check the vertical backscatter distribution. The simulations show some deviations from the exponential loss function but not any fundamental difference. This is in accordance with the good performance of the Dall model.

Main Comment 2: Overall the paper is detailed and would probably be better suited to a remote sensing journal in its current form. For broader applicability as a publication in The Cryosphere it needs to be more succinct. I would encourage the authors to look again at the balance of what must be provided for reproducibility, what is required for basic understanding and what information may be already available for those who really need to know the detail. For example, equations 11-15 may be better kept in the Dall 2007 reference as the jump from equation 10 to 16 will be easier to read for most. This may also allow some of the figures in the supplementary material to be brought into the main paper.

Response: Our motivation for submitting the paper to *The Cryosphere* is the fact that many papers using TanDEM-X data for measuring glacier surface elevation change were published in this journal and that the correction of the penetration related elevation bias was addressed as a critical issue. We agree that some of the technical descriptions are too detailed and that a better focus on the main issues should be provided (including issues explained in our response to Main Comment 1). We will thoroughly check and revise the manuscript in this respect. We will also check options to shift some of the technical information to an appendix or to the supplement, for example the backscatter forward modelling (because this is not used directly for the inversion).

Main Comment 3: There is a lot of detail early in the paper on ICESat / ICESat-2 (section 2.2 – nearly a page) yet these are not actually used for the estimation of InSAR elevation bias, despite them being the observation with the lowest error. There is an indication in Table S1 for the ice-free slope and blue ice area, and for the area around pit P4 in Table S2 but not over the larger area. In table S2 the standard deviation is much higher than the actual measurement. This, and the positive height biases shown in Figure 1 should be discussed – what does it mean when the TDM DEM elevation is higher than the optical-

derived DEM elevation?

Response: Regarding these issues, we want at first refer to Section 3, line 253 to 276, where the differences between the penetration-related InSAR elevation bias (Eq. 1), the height difference between non co-registered DEMs (Eq. 2) and the height difference between optical and InSAR elevation data, co-registered at surface scattering targets, (Eq. 3) is explained. Figure 1 shows the height difference between the global TanDEM-X DEM (TDMgl) and ICESat elevation. In lines 132 to 136 it is stated that for the TDMgl DEM over Antarctica bulk penetration corrections are applied. This explains the positive height bias which refers to areas where the actual penetration bias is smaller than the bulk penetration correction. Because the TDMgl DEM is widely used, we show in Figure 1 the height difference (elevation bias) for the original TDMgl DEM (rather than applying a specific adjustment for the Union Glacier area).

We will shorten Section 2.2, omitting general information on features of ICESat provided in the cited references. Regarding the ICESat data used in the study, these are essential for checking the temporal variability of surface height which is of relevance for estimating the InSAR elevation bias using non-coincident optical data. For the spatially detailed analysis we use REMA because it provides full spatial coverage. The standard deviation of the height difference between ICESat and REMA on the blue ice area, used for vertical co-registration, refers to very high resolution data (8 x 8 m pixels) (line 314). Table S2 shows the mean height difference between the original TDMgl DEM and ICESat for 25 footprints of each track and the standard deviation for the 25 points. This is stated in the caption of Table S2.

Main Comment 4: Please could the authors check for consistency throughout the paper. For example, TDM is defined as the TanDEM-X mission, then TanDEM-X is used interchangeably with TDM in lines 60-70. DEM is defined twice. There are two separate definitions of Δh and dh , with opposite signs. SMRT is suggested as the backscatter model used, but then the rest of the text refers to DMRT. If DMRT then the version used needs to be stated. Line 553 refers to equation 4, but this is not the correct equation. These are all minor defects, but unfortunately make the paper difficult to follow.

Response: Many thanks for pointing this out. We will thoroughly check and correct these points. Regarding Dh and dh , these are actually two different quantities referring to sequential steps in the procedure for deriving the elevation bias, as explained in Section 3, line 253 to 276 (see also the response to Main Comment 3). We prefer to keep these two parameters separated in order to stress the importance of vertical co-registration on surface scattering targets and to provide full traceability.

SMRT is a model framework, comprising different models for describing electromagnetic wave propagation and the snow microstructure (stated in line 348 to line 350). Out of this framework, two suitable models were selected for this study (line 350 to 352). Details on the SMRT and the different models are provided by Picard et al. (2018). DMRT is the general term for dense medium radiative transfer models. The backscatter simulations in this paper are based on the Improved Born Approximation for a dense medium.

Equation 4 in line 553 is the correct reference. This equation describes the vertical backscatter distribution of a volume with uniform scattering and extinction properties.

Specific comments (SC):

SC1: The abstract attributes angular gradients of backscatter intensity to anisotropy in the snow structure. This is misleading. Even if the summer observations at one angle can be compared to the winter observations at another angle (and I'm not convinced they can), this is a stratigraphic effect rather than anisotropy.

Response: Layered media show structural anisotropy causing directional differences in signal propagation and scattering. The statement on anisotropy in the abstract (line 30) refers to the snow/firn volume (as stated there) and not to individual snow grains. Regarding differences in scattering properties between images acquired in winter and summer, we analyzed several scenes from different seasons showing that these are negligible. Between late 2016 and 2018 many scenes with similar observation geometry were acquired all year round confirming the stability. Stable backscatter properties for different seasons conform with the fact that the observed signal is made up by volume backscatter contributions down to several metres depth and seasonal differences would affect only a thin surface layer.

SC2: The Section 2.1. Product reference should already contain the majority of this detail. Only additional processing steps done for this study need to be included.

Response: TanDEM-X has about 100 different operation and acquisition modes. Therefore it is important to provide specifications for the products used in this study.

SC3: Line 134 – please show the location of the 11 blocks (or was this part of a different study?)

Response: The blocks are distributed all over Antarctica (at large distances), shown in Rizzoli et al., 2017 (cited in line 133).

SC4: Figure 2. Snow grain size legend is different in colour to the main plots. Please could you increase the snow grain type font size and/or resolution.

Response: We will increase the snow grain type font size.

SC5: Section 3.2 Perhaps the processing steps would be better placed in the supplementary material. There is a lot of detail on the accuracy of REMA. It would be better to state the vertically registered DEM is treated as the truth, the errors briefly discussed as a limitation of the study and the reader referred to the supplement for more information.

Response: The vertical coregistration and potential errors is very important for deriving height differences between different topographic data sets. Therefore we prefer retaining this section within the main paper. Besides, Section 3.2 is short (15 lines).

SC6: Line 377. Stickiness of 0 breaks theoretical limits. The minimum stickiness is bounded by equation 35 in Löwe and Picard (2015).

Response: According to the theory of electromagnetic wave propagation in dense random media stickiness zero corresponds to infinite stickiness (Tsang et al., 2013). The maximum stickiness value for snow (reported in the literature) is 0.1. This is mentioned in the paper. We will skip the reference to the (theoretical) zero value.

SC7: Line 517. The two observations were taken 2.5 years apart. What microstructural changes could reasonably be expected during this time period, and what would the impact be on the backscatter / elevation bias estimate? The difference has been attributed to incidence angle, but other factors have not been discussed.

Response: The backscatter signatures, as well as the surface elevation, are remarkably stable over years (quite common in the dry snow zone of Antarctica). This was one of the reasons for selecting this area for the penetration study. See also the comment to SC1. The mean differences in sigma-0 between the 2016 and 2018 data on the snow pit sites (Table S3) and the main glacier area (Table 3) are within the absolute radiometric

uncertainty for the difference (0.85 dB). The elevation bias estimate is based on coherence. The backscatter intensity is only used for computing the signal-to-noise ratio which is a minor factor for deriving the volumetric coherence from the total coherence.

Besides, significant deviations of the angular gradient in σ_0 from isotropic scattering are prevalent for density-stratified polar firn. Ground-based scatterometer measurements in the dry snow zone of Dronning Maud Land, East Antarctica, show for X-band co-polarized σ_0 differences of 5 dB to 6 dB between 20 and 40 degree incidence angles (Rott et al., 1993), similar to the Pit 3 and Pit 5 sites on Union Glacier.

SC8: Line 590 h_{binv} is mentioned but not defined – presumably this is from rearrangement of equation 16? It is not clear why equation 17 been included in this paper – I think this is used to calculate the volume coherence from the exponential fit to the SMRT / DMRT backscatter curves for retrieval bias but it would help the reader to state clearly the steps taken.

Response: h_{binv} is defined in the caption of Table 3 (line 448). We will repeat the definition in the text and mention that it is computed by means of Eq. 16. The volumetric coherence is computed with Eq. 17 which is based on the uniform volume approach. We will revise Section 4.2 and rearrange the sequence of the equations in order facilitate the understanding and improve the traceability.