The Cryosphere Discussions

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Comment on tc-2020-380

Anonymous Referee #1

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-RC1, 2021

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.

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.

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.

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?

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.

Specific comments:

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.

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.

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

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.

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.

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

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.

Line 590 hbinv 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.