7 July 2021

2nd report for the Brief communication paper: **Increased glacier mass loss in the Russian Arctic (2010-2017)**

The Cryosphere Discuss. https://tc.copernicus.org/preprints/tc-2020-358/

This 2nd report refers to the revised manuscript **tc-2020-358-manuscript-version3.pdf** and **tc-2020-358-supplement-version3.pdf** from 26.05.2021

Comments to the authors

Thank you for responding to my comments and the changes implemented for improving the work. The paper is better structured now the Data and Methods section reveals relevant aspects specific to the used dataset on this particular glaciated region. Although, as suggested in the review of the first version, the analysis of the backscattering coefficients was added, the estimation of the penetration depth of the X-band SAR signal into the glacier volume is based on wrong assumptions.

I have some doubts regarding the correctness of Eq.1 by the following reasons (see also Dall, 2007): (i) The penetration depth (dp) refers to the vertical. (ii) For small relative penetration the elevation bias h_b can be approximated by the two-way power penetration depth: $dp_2 = dp/2 \approx h_b$. (not by the one-way penetration depth). (iii) For given InSAR geometry and propagation conditions (permittivity) dp is related to the oblique radar propagation path multiplied by the cos of the refraction angle in the snow volume.

The penetration bias depends not only on the radar wave propagation properties in the snow volume but also on the interferometric baseline and incidence angle. The impact of these parameters needs to be considered if an observed elevation bias (or penetration value) is applied to another InSAR scene.

Regarding the Fig. 1 and the related text (line 70 and below):

For estimating the penetration-related elevation bias in Eq. 2 the difference in σ^0 between September (surface melt) and mean σ^0 of Oct. to Jan. is used as proxy. This implies an immediate switch for melting state in Sept. to dry snow with deep penetration in Oct. In reality this transition is gradual in time which means using October (and possibly also November) data in the “winter” ensemble causes a bias for estimating the penetration for the winter case. In Fig. 1a the Oct. and Nov. σ^0 values are lower than the Jan. values (in particular in the 300 m to 600 m elevation zone).

Fig. 1a: The used procedure (calibration coefficients) to convert amplitude to σ^0 needs to be checked because as far as I see σ^0 values are down to -30 dB which is far below NESZ. I also miss mentioning in the paper or supplement in which way was the incidence angle dependence of backscatter intensity taken into account. Also, the look angle of the various TanDEM-X acquisitions is not given anywhere (Table S2 gives a list but with some redundant information). In particular for wet snow σ^0 show large changes with the incidence angle.

Results (line 92 and below and Fig. 2)

The error bar is decreasing with the decreasing magnitude of $\Delta h/\Delta t$ and increasing elevation. Usually, the geodetic error should be independent on $\Delta h/\Delta t$. At higher elevations where $\Delta h/\Delta t$ small additional error contributions may be added resulting in larger error bars than at the termini. I recommend therefore to revise the error calculations. Regarding the uncertainty assessment for the mass change (now equation (1) in Supplement in the current version of the manuscript) I also have some doubts (expressed also by reviewer #2). According to this equation the error of the mass change estimate depends on the mass change magnitude $\Delta M/\Delta t$. This would mean a zero mass change estimate would yield a perfect result (no error). But then the first term of the sum would compensate: small (near zero) $\Delta h/\Delta t$ leads to very large error and vice versa (in case of large mass changes). These terms contributing to the error budget should be treated independently to hold for quadrature sum. See also (Nuth & Kääb, 2011).

Specific comments

Main paper:

Line 39 into the glacier <u>volume</u>.

Line 41 increases in dry snow.

Line 51 snow and ice <u>properties at</u> the glacier surface <u>can</u> have significant <u>impact</u> on …

Line 58 much <u>lower</u> backscatter values then …

Line 64 and 66 replace “surface penetration” by <u>penetration into the volume</u>

Line 86 smaller <u>than</u> on NZ

Line 89 Fig S1<u>a</u>

Line 298 Fig. 1a (identical with Fig S1e): Mean backscatter corresponding to 2016-12 is not visible

Supplement

Adding Table S2 is welcome but contains a lot of redundant information and not the important one. One row pro TanDEM-X acquisition (instead of one row pro CoSSC framing of the same datatake) would be enough but some additional information would be useful: Beff, HoA, incidence angle, etc similar to other publications using self-processed TanDEM-X DEMs (e.g. Table 1 in (Malz et al., 2018)). Keep the established acronyms and labels used in the metadata: <u>Active</u> sensor instead of “transmitting”, “Strip” should be “<u>Beam</u>” and <u>TSX-1</u> and <u>TDX-1</u> (instead of TST and TDT), Relative orbit instead of “Path number”.

Line 122 <u>quadrature</u> sum

References:

Dall, J.: InSAR elevation bias caused by penetration into uniform volumes, IEEE Trans. Geosci. Remote Sens., 45, 2319–2324, 2007

Malz, P.; Meier, W.; Casassa, G.; Jaña, R.; Skvarca, P.; Braun, M.H. Elevation and Mass Changes of the Southern Patagonia Icefield Derived from TanDEM-X and SRTM Data. *Remote Sens.* **2018**, *10*, 188. https://doi.org/10.3390/rs10020188

Nuth, C. and Kääb, A.: Co-registration and bias corrections of satellite elevation data sets for quantifying glacier thickness change, The Cryosphere, 5, 271–290, https://doi.org/10.5194/tc-5-271-2011, 2011.