# Response to *3rd 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 3rd report refers to the revised manuscript tc-2020-358-manuscript-version4.pdf and tc-2020-358-supplement-version4.pdf from 21.09.2021

Green: Response of the authors to reviewer 2

Blue: Report #3

**Bold black: Author responses to report #3 of reviewer 2**

### Comments to the authors

Thank you for responding one more time to my comments and the changes implemented for improving the work. I address here the critical issues remaining to be clarified.

- **Thank you very much the comments and please find our point-by-point responses below. Regarding the conversion between observed vertical elevation differences and signal penetration length, we suggest to include the original version as well as the two-way power penetration approach in the supplement (new sections 3.1 & 3.2) because both approaches are based on assumptions (e.g. volume scattering or the presence of a scattering late-summer firn layer) which might be correct for some but not all glacier areas of Novaya Zemlya. Also, both methods produce almost the same vertical correction fields. By this means, we also save some space in the methods section of the main manuscript.**

*As suggested, we applied the approach using the two-way power penetration to estimate the surface penetration depth instead of the trigonometric function. To estimate the refraction angle into the glacier surface, we referred to a reference study on in-situ experiments in Antarctica (see below). Using this approach, the following paragraphs would replace the former Eq. 1 (L.70) in the revised manuscript):

"The vertical differences between heights of autumn and winter DEM acquisitions are converted into depths of signal penetration into the glacier volume using Eq. 1 following (Dall, 2007):

$$l = \frac{d_p}{\cos(\Theta_v)} \; ; \; d_p = 2 \times h_b \qquad \text{Eq. 1}$$

where l is the penetration length and Θv the refraction angle into the volume. dp is the two-way power penetration depth and can be approximated by two times the vertical elevation bias hb (Dall, 2007). To derive the refraction angle (Θv), Eq. 2 (Snell´s law) is applied:

$$\sin(\Theta_v) = n_1 \times \frac{\sin(\Theta_l)}{n_2} \qquad \text{Eq. 2}$$

where Θl is the local incidence angle, n1 the refractive index of air (1.000293) and n2 the refractive index of glacier ice. For the permittivity of ice, various values have been reported in literature (Rasmussen, 1986; Dowdeswell and Evans, 2004). In general, the refractive index of ice increases with depths due to changes in density. Therefore, we refer to a detailed in-situ study on refraction measurements from the ice surface down to depths of 150m in Antarctica (Kravchenko et al., 2004). For glacier ice close to the surface (0 to -40 m depth), they found values between ~1.3 and ~1.5 as index of refraction. Thus, we apply a refractive index of ice (n2) of 1.4 as the approximate permittivity of ice close to the glacier surface."

I would have preferred a revision in the manuscript directly to avoid confusion. The change of Eq.1 after (Dall, 2007) is welcome. In this paper the vertical penetration bias (hb) is approximately the two-way power penetration depth (dp2) in the case of a small penetration compared to the height of ambiguity (eq. 13 in (Dall, 2007)). With dp the one-way power penetration depth is denoted.

- **Agree, there was a typo in the equation. We changed it to:**

$$l_p = \frac{d_{p2} \times 2}{\cos(\Theta_v)} \; ; \; d_{p2} \approx h_b \qquad \textbf{Eq. 2}$$

But since you already have an observed height difference I suggest to derive the correction directly.

- **The linear correction could be derived directly from the observed height differences without the conversion into signal penetration length. Also, the overall results of the correction with and without the conversion are relatively similar. However, in our understanding, by removing the conversion (based on the local incidence and surface slope) this approach would neglect the local topography as the glacier surface is not a flat area. Thus, we think that the conversion between observed vertical differences and signal penetration lengths should be included.**

In the manuscript dp is once defined as depth of penetration into the volume (line 61) and below (line 68) as penetration bias. These are not the same. In this case I guess EQ2 refers to the penetration related elevation bias hb.

- **Unfortunately, there was some confusion between the linear regression equation (EQ2 in the paper) and the response letter. EQ2 is applied to estimate the penetration length into the glacier volume (not vertical elevation difference). We decided to use this variable because we wanted to account for the surface topography as the local surface slope and incidence angle is known for all glacier areas. In the original EQ1 of the previous manuscript version *dp* was not defined as the vertical difference (in contrast to** (Dall, 2007)**). Therefore, in the new version *dp* in Eq.2 was replaced with *lp* which is the penetration length into the volume instead of the vertical difference.**

In Author's response Eq.2 right: sin $\Theta_1$

But you don't need to apply Snell's law if the penetration is defined as vertical. If yes, please explain.

- **The estimated penetration in the regression model is not defined as vertical (see comment above). Therefore, Snell´s law has to be applied, in our understanding, to account for the slight change in direction of the X-Band signal at the interface (glacier surface) between atmosphere and glacier ice.**

Besides Kravchenko et al. report on measurements of the dielectric permittivity on the South Pole, with a density profile of cold polar firn, very different from that of the percolation zone of Arctic glaciers. The real part of permittivity of dry snow and firn can be computed from the density with a slightly non-linear relation (Ulaby and Long, 2014). Typical density profiles from the percolation zone of Artic glaciers have been reported in several papers (e.g. for Svalbard by Marchenko et al., 2017).

- **For the dielectric permittivity of the glacier ice close to the surface, we had to refer to values from the literature as there are no density profiles available from the Russian Arctic archipelagos (to our knowledge). The values reported by (Kravchenko et al., 2004) are in fact from measurements in Antarctica but they also refer to a low density (~400 kg $m^{-3}$) for the surface layers which is similar to the surface part of the profiles shown by (Marchenko et al., 2017) (their figure 4). Therefore, we assume that the used value is a reasonable approximation for our study region.**

We also recalculated the signal penetration corrected elevation & mass change for Novaya Zemlya with this approach but the results are almost exactly the same as in the original version (original Δh/Δt = - 0.643 m/a and new Δh/Δt = -0.644 m/a). The only significant change would be the estimated average vertical offset (3.5 m instead of 2.13 m).

However, we are not sure if this approach improves the accuracy of the estimate. The model presented by (Dall, 2007) assumes an infinite volume, i.e. the microwave signal is not scattered by any layer below the surface (volume scattering). While this might be the case for some glacier areas on Novaya Zemlya with rather high vertical offsets, it is not unlikely that there is a scattering layer below the actual surface (e.g. melt/refrezzing of a previous summer) for glacier areas with smaller vertical differences. For those areas, the new approach could overestimate the penetration depth and produce a rather high average penetration depth for all September acquisitions.

For those reasons, we did not include the two-way power penetration estimate into the revised manuscript yet, but we would be very interested to hear the reviewer´s opinion regarding those concerns.

EQ2 (line 70) should refer to the ΔhW-A.(or hb) A plot of its altitude dependence over the overlapping areas (red dots in Figure S2a) would be crucial to understand how this regression was derived. The regressions shown in Fig 1c and Fig 1d are not explained at all. Hard to understand how they contribute to EQ2.

- **EQ2 is used to estimate the penetration length which is then converted back to the vertical elevation bias using the respective equations (also see comments above). There was unfortunately some confusion because *dp* was defined vertical in the response letter but not in the original manuscript.**
- **Agree, we added another panel to Figure S2 which shows the average observed vertical difference of the indicated overlapping glacier areas.**
- **The linear regressions shown in Fig 1c & 1d do not contribute directly to the correction function. We included those regression lines in the figures to better illustrate the correlations between backscatter intensity, elevation and differences in signal penetration depth. We mentioned those connections in section 2.1 and extended the caption of Figure 1*: "The linear correlations of mean September backscatter intensity and elevation (1c) and mean difference in signal penetration depth and September backscatter intensity (1d) are indicated as black solid lines."***

"We did not adjust for differences in incidence angle or effective baseline because the viewing geometries of the majority of the used SAR acquisitions are rather similar (Table S2). For 99% of the glacierized area of Novaya Zemlya, the difference in incidence angles is not larger than 2° (39.3° - 41.3°) while for 93% of area the average baseline is 91.9 m (87.8 m – 95.4 m)."

Accepted.

"It is noteworthy, that the applied regional correction scheme can introduce a larger uncertainty at a local glacier scale caused by different surface and backscatter conditions between the specific TanDEM-X acquisitions (Fig. S2b). However, due to the limited extent of overlapping glacier areas (Fig. S2a), it is not possible to derive a date-specific intensity correction for each DEM strip. Thus, the applied linear model does rather represent an average difference in surface penetration depth between autumn and winter SAR data."

Accepted.

*The intensity images are created using the Gamma remote sensing software environment (Werner et al., 2000). The radiometric calibration of the amplitude to σ0 values is automatically performed by the conversion algorithm from the CoSSC to the Gamma data format (using the metadata of the CoSSC data product). The respective algorithms are part of the interferometry (ISP) module and described in the Interferometric SAR Processor – ISP user´s guide (GAMMA Interferometric SAR Processor (ISP), 2021) in section 2.2.7 (TerraSAR-X & TanDEM-X data read algorithms) and 2.4.5 (radiometric calibration procedure). A link to this user guide is provided in the reference list.

Still the backscattering values used for the signal penetration (Fig 1c and Fig 1d) are going down to -27 dB. The noise level (NESZ) for the beams around 37-41 deg incidence is around -24 dB. It is annotated in each CoSSC product. This questions additionally EQ2.

- **There are some studies which showed similar radar measurements below the noise level, e.g. (Meng et al., 2017) showed values of -30 db and less for low-scattering (ocean) areas. We assume that the fraction of September acquisitions with relatively low backscatter values are also such low-scattering areas and not caused by an error in the DEM creation. Nevertheless, the linear regression would be almost identical if we would include some of the very low backscatter values (see Fig. 1d).**

*Concerning the incidence angle, we revised Table S2 following the suggestions (see respective comment below) and extended the methods section (see second comment).

Accepted.

*The hypsometric bars shown in Fig. 2 refer to the normalized median absolute deviation of Δh/Δt measurements on glacier areas within each elevation bin. Therefore, the bars are largest a low elevations because the spread of measured Δh/Δt values is large due to the presence of strong thinning glacier termini. At high altitudes, the range of measured Δh/Δt values is in general much smaller (see also Δh/Δt maps of Fig.2) and thereby also the bar. We extended the caption of Fig. 2 because the description of the shown bars was missing. Regarding the geodetic error, we did not calculate a mass change error for each elevation bin but for the entire region (based on the mean regional elevation change and respective uncertainty).

The errors in dh/dt [m/yr] related to vertical co-registration do not depend on the magnitude of retrieved dh/dt, but on the uncertainty in co-registration, independent of the magnitude. Even zero dh/dt has the same error in respect to vertical co-registration. For the higher elevation zones (firn areas) where penetration-related errors are added, the error in the elevation change rate dh/dt [m/yr] should by higher in the firn areas than in ice areas of glaciers. Fig. 2 d to f: Please explain the term “normalized median absolute deviation of elevation change measurements of each elevation bin”. An equation would be helpful.

- **Agree, the magnitude of the specific dh/dt error of an elevation bin is related to the respective accuracy of the co-registration and DEM data. In most cases, this error increases at high altitudes with steep slopes and a rugged topography. However, the “error bars” shown in Fig. 2 do not refer to the actual elevation change error but indicate the range of different glacier elevation change values within each elevation bin.**
- **The size of the bars in Fig. 2d-f was simply derived by calculating the normalized median absolute deviation (NMAD) of glacier dh/dt values of each elevation bin. Thereafter the NMAD was subtracted/added to the mean glacier dh/dt value of the respective elevation bin. We selected this statistical measure to indicate the spread of elevation change values of each elevation bin which contributed to the mean change value. We used the NMAD instead of showing the minimum and maximum elevation change value of each bin because otherwise the size of bars would be very large at some elevations which would decrease the visibility of the overall hypsometric distribution of the mean change values.**

*Regarding to Eq. 1 (Supplement), unfortunately we do not quite understand the question referring to the mass change and elevation change uncertainty: The first term of the sum is the ratio between the Δh/Δt uncertainty (δΔh/Δt) and the mean (glacier) Δh/Δt estimate. While the Δh/Δt estimate is derived on glacierized areas, the Δh/Δt uncertainty (δΔh/Δt) mainly indicates the potentially remaining offsets on non- glacier areas after the co-registration (and also other sources of uncertainty, Supplement Eq.2). If Δh/Δt would be very small (and thereby also ΔM/Δt), the uncertainty of ΔM/Δt could still be relatively high if δΔh/Δt (off-ice) is high compared to Δh/Δt (on-ice). For example, the measured elevation change rate is rather small but there are a lot of artificial elevation offsets remaining after the co-registration. In this case δΔM/Δt would be high compared to ΔM/Δt.

Accepted. For Eq. 1 (Supplement) there was some misunderstanding because of the same symbol (delta) used for relative error (right hand side) and absolute error (left hand side). The error estimate refers to the total change of mass over an extended area (though not defined; should be explained). There is one (rather unlikely case), when the equation is wrong: if the retrieved Δh/Δt is exactly zero. According to Eq. 1 this yields zero error for the mass change.

*Fig. 1a shows a subset of backscatter values (5000 random samples) because otherwise the figure would be too busy. The acquisitions of December 2016 cover only a very small fraction of the Novaya Zemlya ice cap (~10 $km^2$). For this reason, there are only very few December datapoints visible and the mean backscatter was only calculated and plotted for the lowest elevation bin. The mean value is almost the same as for October 2016 (triangle) and therefore difficult to identify in the figure.

Now I can see it. Thanks for the explanation!

*We changed Table S2 and included only one row per TanDEM-X acquisition instead of each CoSSC frame. The terminology was adjusted and the new columns include now, as suggested, acquisition date, acquisition start time, active satellite, orbit direction, relative orbit, strip length (number of CoSSC frames), effective baseline, height of ambiguity and incidence angle.

Accepted.

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