<u>Author´s response to 2nd reviews of:</u>

Brief communication: Increased glacier mass loss in the Russian High Arctic (2010-2017)

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Response to the editor:

Editor report

Comments to the Author:

Dear authors,

I now collected two reviews of your revised manuscript.

In general, both reviewers are rather positive about your revisions. They acknowledge your efforts and agree that the manuscript has been improved compared to the initial submission. However, they still suggest some significant improvements are needed before publication. In particular, reviewer#2 challenges some of the corrections and ask to double check some of the hypothesis/equations that you used.

*Thank you very much for the extension which was very helpful to discuss some of the suggested revisions.

Following the suggestions of reviewer 1, we made some structural changes to the methods chapter and extended some parts of the discussion (please see detailed comments).

We also applied the different conversion approach between vertical height differences and surface penetration depths which was suggested by reviewer 2. The new calculation does not change the overall elevation change rate but affects the estimated depth of signal penetration. However, we are not entirely convinced if this approach is an improvement to our previous version because it is based on some assumptions which might be only correct for some areas on Novaya Zemlya. Particularly, it assumes radar penetration into an infinite volume (i.e. no reflection from a previously melted and refrozen surface layer). Therefore, we described the new calculations and results in our reply to reviewer 2 but did not change the respective paragraphs in the revised manuscript yet. We would be interested to hear the reviewer´s opinion on this alternative calculation and our concerns regarding the involved assumptions.

In addition, you could compare your estimate to the recently published Tepes et al., (10.1016/j.rse.2021.112481) using CS-2 for the 2010-2017 time period. A figure comparing the available estimates for the Russian Arctic (as done in Tepes et al.) would be welcome to put your findings into context (in the supplement if you lack space for it). It seems that your mass balance estimates is more negative than all previous estimates for the Russian Arctic, something you may want to comment on.

*Yes, the available mass change results for the Russian Arctic vary between different studies and even measurements of the same sensors (e.g. different GRACE estimates for the Russian Arctic in Fig. S5). The TanDEM-X results (> 2010) are similar to the measurements of e.g. (Ciracì et al., 2020; Zheng et al., 2018; Ciracì et al., 2018) but rather high compared to (Hugonnet et al., 2021; Tepes et al., 2021). Yet, an increase in mass loss during the 21st century is shown by most studies. We also mentioned those differences and similarities in the discussion section but cannot provide a definite explanation for the different results. Regarding the recent CryoSat-2 study, we included Tepes et al. (2021) in section 4 (Discussion of mass change results) of the revised manuscript. Also, there is already figure S5 in the supplement (now also including Tepes et al.) which shows available results from the Russian Arctic and individual archipelagos. Or did you mean a different kind of figure? If so, we could still include it in the supplement.

Note that I am away until 8 August so no rush to submit your revised text (even if the system is pressing you).

We are now looking forward for a revised manuscript together with a point-by-point response.

Best regards,

Etienne Berthier

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Response to reviewer 1:

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

This manuscript has been heavily revised with a new SAR-signal penetration correction based on an empirical relation between seasonal SAR backscatter and measured elevation differences. The correction scheme lacks proper validation (e.g. by satellite altimetry), but the authors provide both corrected and uncorrected results which are not so different that they alter the main findings and conclusions. So all in all, I think they have succeed to address the main issues at an appropriate level, providing robust results of recent glacier changes across the Russian High Arctic at a detailed level not presented before.

The manuscript has also been restructured by moving most of the standard methodology (established in earlier publications) to the supplement instead of the confusing and partly repetitive split-up of the previous manuscript version. This has allowed more details on the penetration issue to be included in the main manuscript. Both aspects are clear improvements, but as indicated in the chronological comments below, I think the manuscript can still benefit from some smaller restructuring and inclusion of essential methodological details such as density conversion factors and the parallel calculation of mass-change rates with and without penetration correction. There are also some unclear sentences and inconsistent terminology, so I recommend the authors to do a careful language read-through and edit for the final manuscript version.

*Thank you very much for the 2nd review. We added some description to the methods section including density conversion and the calculation of mass change rates with and without signal penetration correction. Additionally, we made some changes to the structure of the methods and results chapter. As suggested, we merged the two sections about surface penetration in the methods chapter. Please find our detailed responses below (Line numbers refer to the track-changes version):

Specific comments and edits (line numbers refer to the version with tracked changes):

L21: Clarify: interpolation -> spatial interpolation (or: interpolation to unmeasured areas)

*Changed to "spatial interpolation"

L26: Since SAR penetration has become a large (and important) part of the manuscript, the introduction could be expanded with a paragraph on that topic using some of the material and references that are now in the data&methods section (L58-62, L89-91).

*Agree, we moved some of the more general descriptions of surface penetration and backscatter from the former methods section to the introduction (L.25-35): *"… We use synthetic aperture radar (SAR) DEMs of the TanDEM-X satellites which are independent from cloud cover and provide a high spatial resolution. However, the SAR data derived elevation change rate can be biased by differences in signal penetration depth into the glacier surface between DEM acquisitions of different seasons. The depth of signal penetration is related to the prevailing glacier surface conditions at the acquisition time. In general, SAR penetration is close to zero for melting snow surfaces and bare glacier ice and increases during dry and frozen conditions. X-band penetration depths of several meters have been observed in* *different regions (e.g. Millan et al., 2015; Zhao and Floricioiu, 2017; Abdullahi et al., 2018; Li et al., 2021). Previously, penetration depths have been estimated by a number of studies (e.g. Abdullahi et al., 2018, 2019; Li et al., 2021), using backscatter intensity. SAR backscatter intensity depends on physical properties of the glacier ice, such as grain size and density, roughness and water content (Wessel et al., 2016) and changes between melting and frozen conditions. Thus, we apply a regional correction for relative differences in SAR penetration, based on backscatter intensity, to account for different TanDEM-X acquisition periods of the Novaya Zemlya ice cap."*

Section 2.1: The main methods have now been moved to the supplement and only a brief summary remains here. I think this makes a much clearer distinction although it forces the reader to look up the supplement or the more methodology-oriented previous papers. The important density assumption should still be mentioned here with regards to the conversion from elevation change to mass change. And it would only take another sentence to also mention that you did separate elevation/mass rate calculations for land- and marine-terminating glaciers based on RGI. Also important for the Results/Discussion. And finally, it should be made clear that you make mass-change estimates both with and without the penetration correction (as in Table 1) and the associated terms should be clear and consistent to avoid confusion.

*We extended 2.1 and added explanations regarding the density conversion factor and land-/marine-terminating glaciers separation and corrected/uncorrected mass change calculations (L.119-125): *"Glacier volume changes changes are converted to mass change using two density scenarios. For a) a conversion factor of 850±60 kg m^{-3} (Huss, 2013) is applied and for b) 900±60 kg m^{-3} as an approximation of the density of ice. For Novaya Zemlya, the geodetic mass change rate ($\Delta M/\Delta t$) is calculated using the uncorrected elevation change rate ($\Delta h/\Delta t_{uncorr.}$) as derived from the DEM differencing as well as the surface penetration corrected elevation change ($\Delta h/\Delta t_{corr.}$). Additionally, glacier elevation changes are derived specifically for marine- and land-terminating glaciers (Fig. S3), following the glacier terminus classification of the Randolph Glacier Inventory."*

*Concerning the calculation of mass changes with and without signal penetration correction, we also extended the supplementary methods (L. 87-97) to include the terminology.

Section 2.2: This is a new subsection about X-band penetration. It reads fine, but is somewhat awkward in a data&methods section as it partly discusses SAR/X-band penetration in general (like introductory or discussion material) and partly describes the timing of the data without any further methodological description than "When comparing elevations of different TanDEM-X scenes the relative difference in penetration depths is determined by the acquisition dates and seasons", which really says nothing about how it was done. Instead, the penetration-related methods come in the next Section (2.3) which is narrowly named "SAR backscatter intensity analysis. I think it would be clearer to combine these two sections with a title that fits both, e.g. "Correction of seasonal penetration bias". Actually, since a penetration correction is added to the autumn DEMs before calculating mass change rates, I think it would be even better if this section comes first and describes both the TanDEM-X data and the correction, e.g. "2.1 TanDEM-X elevation data and penetration correction" and "2.2 Glacier elevation- and mass rates".

*Agree, we merged the former sections 2.2 & 2.3 (now section 2.1) and adjusted the internal structure of the methods chapter by moving the data (and penetration correction) description to the start of the chapter. The explanation of the elevation and mass change rate calculation is now section 2.2. In addition, some of the more general statements on surface penetration were moved to the introduction.

*Also, the sentence “When comparing …” was rephrased to: *“When subtracting elevations of SAR DEMs from different seasons, the depth of signal penetration might differ between acquisitions, due to changing surface conditions, and bias the elevation change rate.”*

Section 2.3: Nice add-on, but the topic is really penetration, not backscatter itself, so a revised title or a merge with Sect 2.2, as mentioned above, should be considered.

**See comment above*

Eqs. 1-2: Any references for these equations if they have been used in a similar context?

*Eq. 1 is just a trigonometric function based on the viewing geometry of the radar and the local surface slope. Eq. 2 is similar to the approach of (Abdullahi et al., 2019). However, we used a different version to derive the relative difference in penetration depths instead of absolute surface heights ((Abdullahi et al., 2019) applied a different linear model including SAR coherence). Therefore, we cited the studies in the methods section but not directly at the equation.

L122: smaller as -> smaller than

*Ok, changed

L148: add “average: ...“ to the parentheses to be clear. What is “...confined to a smaller number of glaciers”? I think you mean that strong thinning is confined to a smaller number of glaciers, not the “general less negative” elevation changes.

*Yes, the sentence should express that strong thinning is only observed at a relatively small number of glaciers and not for all glaciers of the regions. Changed sentence (L.195-197) to: *“Regional average elevation changes of glaciers in Franz Josef Land (-0.48±0.04 m a^{-1}) and Severnaya Zemlya (-0.34±0.12 m a^{-1}) are in general less negative and strong thinning is confined to a smaller number of glaciers.”*

L150: “Slight thickening is also observed...”. What does “also” refer to? Delete.

*”also” referred to the description of positive elevation changes in the previous sentence (Vavilov surge). In any case, we removed it.

L152: The term “adjusted mass change” should have been clearly defined beforehand in the methods section, otherwise one has to guess what you mean here. Personally, I think “uncorrrected” and “corrected” would be clearer terms than “measured” and “adjusted”, but either is fine as long as they are clearly defined at an early stage.

*We changed the terminology of the mass change results to “uncorrected mass change” ($\Delta M/\Delta t_{uncorr.}$) and “corrected mass change” ($\Delta M/\Delta t_{corr.}$) in the main manuscript and the supplementary methods. This new terminology is introduced in chapter 2.2 (glacier elevation change calculation)

L159: I understand what you mean, but the sentence is not technically clear. Rephrase.

*Split and rephrased sentence (L.180) to: *"The average vertical difference in surface elevation on the respective overlapping glacier areas (Fig. S2a) is 2.13 m. Also, the elevation change rates derived from all glacier areas which were acquired in September 2016 (Fig. 1b), show an average difference in surface lowering of 0.4 m a^{-1} compared to areas acquired in winter 2016/17."*

L156-168: I suggest to move this text about the penetration issue ahead of the glaciological results, similar to what I suggested for the data&methods section and what is already done in the Discussion. First you build confidence in the data and corrections, then you look at glacier changes.

*Agree, we changed the structure of the results chapter accordingly.

L194: What is "relatively large"? Based on your calculations and relevant studies, you can provide an approximate meter-range here to make it more informative.

*We did not provide a number in this sentence deliberately because our estimate shows only a relative difference in surface penetration between winter and autumn and not an absolute depth of penetration. The latter can not be derived from our datasets as we do not have a height reference for the respective acquisition dates. Thus, it might confuse readers to provide an absolute depth of penetration at this point which cannot inferred from our TanDEM-X data. However, based on other studies which did calculate absolute penetration depths, it is likely that the winter penetration was in the range of several meters. Therefore, we extended the sentence (L.222): *"… was relatively large, i.e. several meters as found by previous studies (Millan et al., 2015; Zhao and Floricioiu, 2017; Abdullahi et al., 2018; Li et al., 2021), …"*

L195: An important reference study, but it is not really a "similar observation" because your relative penetration differences indicate penetration beyond previous summer surface which is not expected to be deeper than 1-2 m in your case. If the summer-surface indeed plays a role here, it can be troublesome because there are large variations from year-to-year in how much melt and refreezing there is in the higher accumulation area. Some years almost no melt and refreezing, other years thick ice layers can form. I miss a brief discussion of this issue in relation to the findings of Rott et al.

*Agree, for the glacier areas with relatively high differences in penetration depth, the scattering surface of the winter acquisitions does probably not refer to the late summer surface of the previous year. For those areas with high penetration values the measured height of the phase center could be either related to volume scattering into the ice body or an even older firn layer. This could be the case, for example, for one of the DEM strips in the northern part of Novaya Zemlya (Fig. S2b) which shows high offset values in the upper accumulation area of the ice cap. However, without knowledge of the glacier ice stratification it is not possible to determine the potential cause. On the other side, there are also glacier areas which show small differences in comparison. For those areas, it is not unlikely that the microwave signal is reflected by the summer surface of the previous year.

We discuss this in the revised manuscript (L.223-230)*: "For TanDEM-X DEMs of the Antarctic Peninsula it was observed that the measured cold-season heights rather referred to the refrozen firn of the previous summer than to the actual glacier surface (Rott et al., 2014). This might be also the case for some of the glacierized areas of Novaya Zemlya where the observed bias between autumn and winter DEMs is relatively small (e.g. < 2 m). However, for glacier areas with higher differences in signal penetration depth, it is more likely that the measurement is biased by penetration beyond the previous late-summer surface, either by an older ice layer of a year with widespread melt and refreezing or volume scattering of the X-band SAR* (Dall et al., 2001)*."*

L221: More negative than what? Franz Josef Land or the other studies. Unclear sentence.

*This referred to the gravimetric measurements mentioned at the beginning of the sentence. Changed to (L.250): *"…, the estimate for Severnaya Zemlya is even more negative than the gravimetric measurements (Ciracì et al., 2020) which might indicate recent acceleration of glacier mass loss also on this archipelago."*

L224: Sudden transition from discussing mass changes to elevation changes. Please reread the Discussion and try to be more consistent with terms and quantities discussed.

*For the glacier change discussion, there are a variety of studies with different regional settings and methodology. We decided to use a structure which begins with the large-scale regional studies (GRACE & Aster), followed by region-specific studies which focus on the individual archipelagos. Thereby, it is not entirely possible to avoid transitions between different datasets and methods. Nevertheless, we rephrased some parts of the discussion and reduced the transitions between different units. In the former L.224, we replaced the elevation change value with the respective mass change value.

L224: Marine-terminating only or land-terminating also? Fig. S3 indicate little difference between the two types at NZ, which I think should have been discussed too. But here you maybe talk about a smaller number of glacier with even larger changes, please specify.

*The strongest negative elevation changes are measured at the termini of some marine-terminating glaciers on Novaya Zemlya (particularly northwestern part of the ice cap). However, there are also a number of marine-terminating glaciers with less negative surface changes which is the reason why the average changes in Fig.S3 are not very different for marine- and land-terminating glaciers. Nevertheless, this statement referred to the (marine-terminating) outlet glaciers of Novaya Zemlya which have the most negative elevation changes. Thus, we rephrased the sentence (L.249): *"The strongest local surface lowering is observed at some of the large marine-terminating outlet glaciers, most notably on Novaya Zemlya (Northwestern Severny Island ice cap)."*

L229: If terms have been well explained earlier, you wouldn't need a long add-on like "...to the signal penetration adjusted mass change rate derived by TanDEM-X".

*Agree, we changed the mass change terminology (see comment 3) and shortened this part.

L230: "also measured by Strozzi". What does "also" refer to? Your results show few indications of accelerated flow on NZ as far as I can see, so it's unclear how your results relate to those of Strozzi et al. Please clarify the relevance in the manuscript.

*This phrase is more related to the previously cited studies (Carr et al., 2014; Melkonian et al., 2016) which describe an increasing retreat of large outlet glaciers and general increase in mass loss on Novaya Zemlya during (approximately) similar observation periods as the TanDEM-X difference of this study. To clarify this, we moved the sentence to Line 255: *"… For those glaciers, an increasing retreat in the early 21st century was attributed to fjord geometries and changes in sea-ice concentrations (Carr et al., 2014). Long-term observations indicate a more rapid thinning during recent years, particularly at the termini of marine-terminating glaciers (Melkonian et al., 2016). An acceleration in flow velocities for* *the major tidewater glaciers in the Russian Arctic was also measured by (Strozzi et al., 2017) over the course of the last decades"*

L233: "does not seem to be related to potential SAR penetration" -> "do not seem to be related to differences in SAR penetration" (there is penetration, but it can vary...)

*Agree, changed sentence accordingly

L236: Or it can be related to a long-term dynamic imbalance (too low ice-flux velocities) with cyclic fast-flow/surging as seen for some glaciers. Your results are not conclusive.

*Yes, it is not possible to conclude this from our rather short observation period. We therefore wrote *"… might be related …"* because the elevation gains cannot be clearly attributed to increases in moisture.

L241-242: This is your conclusion based on a single observation period. The term "increasingly negative" infers a more continuous process (acceleration) which you cannot conclude from your data and is not supported from gravimetry time series either. To be on the safe side, you should limit your conclusion to an increased mass loss in your period versus other studies from the 2000s. Please keep this in mind elsewhere too.

*This sentence was intended to describe the increase in mass loss between our study and measurements between ~2000 – 2010. Also, the most recent gravimetric time series (Ciracì et al., 2020) does indicate an acceleration for FJL & NZ (their Table 1). Nevertheless, we made some small changes to clarify this comparison (L.276-280): "Glaciers in the Russian High Arctic have shown a contribution of 0.06 mm a^{-1} to global sea-level rise between 2010 and 2017 and an increased mass loss compared to the first decade of the 21st century."

L243: As pointed out in the previous review, this sentence is unclear. Please revise according to the explanation in the author response letter which makes sense.

*Extended/changed conclusion (L.278-280): *"This observation is in line with glacier changes of other Arctic regions, showing an increasing contribution to sea-level rise in the last decades. While specific mass change rates of Arctic glaciers are still less negative than those of many glaciers outside the polar regions, the absolute mass loss is higher due to the vast glacierized areas of the Arctic."*

Fig 1: Nice figure, but the data coverage of each panel should be made clear by statements for panel a and b (all data or only overlapping?) and by referring to Fig. S2 for aerial extents. This important aspect can also be made clearer in the manuscript text.

*The data points shown in Fig. 1a refer to a random subset (for improved visibility) of all raster cells of the 2016/17 DEM mosaic. The hypsometric mean elevation changes in Fig 1b were also calculated from all respective glacier areas (not only overlapping). Fig. 1c & d refer to overlapping glacier areas. We extended the caption to clarify the data shown in each panel: Fig. 1a: *"Point icons illustrate a random subset (5000 cells) of the 2016/17 DEM mosaic of Novaya Zemlya."* & Fig. 1b: *"Mean elevation change rates … of all respective glacier areas."*

Fig. 2, caption: Nice figure. To be more precise I would say: "...corrected for differences in seasonal SAR-signal penetration (Fig. S2)."

*Ok, changed caption accordingly

Supp-L55: How was the co-registration done?

*The co-registration includes an initial vertical correction, followed by an iterative horizontal correction (following the algorithm of (Nuth and Kääb, 2011)) and a final vertical correction. The description of the co-registration was extended (L.57-61): *"In both cases, the co-registration is performed as an iterative process to remove vertical and horizontal shifts between the "raw" CoSSC DEMs and a reference surface. Initially, vertical biases are estimated (on stable areas) and corrected. Thereafter, horizontal shifts are minimized using an iterative approach of (Nuth and Kääb, 2011). Eventually, a second vertical correction is applied to reduce remaining offsets."*

Fig. S2b: Penetration-bias corrections look very different from scene to scene, which I suppose is due to different backscattering conditions for the acquisitions. Although this can be a large local issue, it has only a small impact on the regional change rates which is the main scope of this paper. Still, a brief discussion of this matter would be good.

*Yes, the visible differences in the correction field are related to the different acquisition dates of the September TanDEM-X data and reflect the varying states of backscatter intensity and local surface conditions. Due to the limited extent of overlapping data takes, the proposed correction scheme can only account for mean differences between autumn/winter acquisitions. It is therefore possible that on a local scale the applied correction, as mentioned, over- or underestimates the actual signal penetration depth.

*We added this to the discussion in the main manuscript: *"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."*

*… and also to the caption of Fig. S2: "*Vertical differences in the estimated correction field are caused by different local surface conditions and backscatter intensities of each September TanDEM-X acquisition.*"

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Response to reviewer 2:

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.
*Thank you very much for your comments and please find our point-by-point responses below. Line numbers refer to the track-changes version of the revised manuscript.

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

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

where Θ_l is the local incidence angle, n_1 the refractive index of air (1.000293) and n_2 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 (n_2) of 1.4 as the approximate permittivity of ice close to the glacier surface."

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.

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.
*We did not correct for different incidence angles or baselines because the viewing geometries of the used SAR data on Novaya Zemlya are relatively similar: 99% of the autumn glacier areas have been acquired with a difference in incidence angles of less than 2°. More precisely, 68% of the raster cells have an incidence angle of ~39.3° and the remaining 32% of ~41.3°. Regarding the baseline, the mean effective baseline of ~93% of the used data is 91.9 m with a minimum to maximum range between 87.8 and 95.4 m. Only the acquisitions of 2016-09-08 (~7% of total area) have a significantly larger baseline of 238.6 m. In the revised version, the specific incidence angles and baselines of each DEM strip are stated in Table S2 and we added the following lines to the methods section (L. 88-91): *"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)."*

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 σ0 between September (surface melt) and mean σ0 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. σ0 values are lower than the Jan. values (in particular in the 300 m to 600 m elevation zone).
*Agree, a gradual transition between melting and frozen conditions is much more realistic than a sudden transition. However, the extent of overlapping glacier areas (which are used to estimate the vertical offset) is limiting the potential of a more detailed comparison. For example, if we would calculate a specific correction factor for each month (or even acquisition date), the reference areas would be very small. Additionally, another source of uncertainty could be the spatial distribution of reference areas across the Novaya Zemlya ice cap. In the current comparison, the overlapping (reference) areas are almost equally distributed across Novaya Zemlya (Fig. S2). When using a monthly or sub-monthly comparison, the individual reference areas would be no longer represent the entire latitudinal extent of

Novaya Zemlya. Therefore, we think that a correction approach with a higher temporal resolution would rather increase the uncertainty than improve the results. Nevertheless, we agree that this average correction factor can over- or underestimate differences in signal penetration depth on a local (glacier-specific) scale, depending on the exact acquisition times. To discuss this issue, we added a paragraph to the discussion (L.235): *"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."*

Fig. 1a: The used procedure (calibration coefficients) to convert amplitude to σ0 needs to be checked because as far as I see σ0 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 σ0 show large changes with the incidence angle.
*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.
*Concerning the incidence angle, we revised Table S2 following the suggestions (see respective comment below) and extended the methods section (see second comment).

Results (line 92 and below and Fig. 2)

The error bar is decreasing with the decreasing magnitude of Δh/Δt and increasing elevation. Usually, the geodetic error should be independent on Δh/Δt. At higher elevations where Δh/Δt 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 ΔM/Δt. 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) Δh/Δt 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).

*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).

*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 ($\delta_{\Delta h/\Delta t}$) and the mean (glacier) Δh/Δt estimate. While the Δh/Δt estimate is derived on glacierized areas, the Δh/Δt uncertainty ($\delta_{\Delta h/\Delta t}$) 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 $\Delta M/\Delta t$), the uncertainty of $\Delta M/\Delta t$ could still be relatively high if $\delta_{\Delta h/\Delta t}$ (off-ice) is high compared to $\Delta h/\Delta t$ (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 $\delta_{\Delta M/\Delta t}$ would be high compared to $\Delta M/\Delta t$.

Specific comments

Main paper:

Line 39 into the glacier volume.
*Included "volume"

Line 41 increases in dry snow.
*Ok, changed

Line 51 snow and ice properties at the glacier surface can have significant impact on …
*Included "properties"

Line 58 much lower backscatter values then …
*Ok, corrected

Line 64 and 66 replace "surface penetration" by penetration into the volume
*Ok, replaced

Line 86 smaller than on NZ
*Ok, corrected sentence

Line 89 Fig S1a
*Ok, changed

Line 298 Fig. 1a (identical with Fig S1e): Mean backscatter corresponding to 2016-12 is not visible
*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 km²). 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.

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: Active sensor instead of "transmitting", "Strip" should be "Beam" and TSX-1 and TDX-1 (instead of TST and TDT), Relative orbit instead of "Path number".
*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.

Line 122 quadrature sum
*Ok, changed

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