Author´s response to reviews of:

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

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Initially, we would like to thank the reviewers and editor for the detailed and comprehensive comments on our manuscript "Increased glacier mass loss in the Russian High Arctic (2010-2017)". Following the suggestions during the review process, we changed the structure of the paper in the revised version. We moved parts of the methods section (DEM creation & co-registration) to the supplement as this approach has been already described in a number of previous publications. Instead, we extended the discussion and correction of radar signal penetration in the main manuscript. Therefore, we included an analysis of backscatter intensity, as suggested by the reviewers, and estimated the relative difference in surface penetration depth of different TanDEM-X acquisitions based on an empirical relationship between autumn backscatter and observed differences in measured surface elevation. All relevant changes made in the manuscript are summarized on page 2.

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1) List of changes

a) Methods section (chapter 2)
- Moved former chapter 2.1 (DEM creation) & 2.2 (uncertainty analysis) to supplement and extended method description
- Included short methods overview (2.1) and signal penetration properties (2.2) in main manuscript L 29-51
- Rewrote chapter 2.3 and included backscatter analysis L53-90

b) Results section (chapter 3)
- Included observations from backscatter analysis in results section L 104-116

c) Discussion section (chapter 4)
- Rephrased discussion of temporal differences between acquisitions on Novaya Zemlya L 118-141
- Slightly extended discussion and comparison between TanDEM-X derived mass change and previous studies L 142-168

d) Figures (main manuscript)
- Former Figure 1 (elevation change map) was moved to Figure 2
- Included new Figure 1 with results from backscatter analysis of Novaya Zemlya

e) Supplement
- Included extended methods section on datasets, DEM-creation & co-registration (p. 2-4)
- New Table S1 with DEM co-registration statistics on non-glacierized areas (p. 5)
- New Figure S1 with backscatter intensities of each subregion (p. 6)
- New Figure S2 with overview map of DEM acquisitions and surface penetration correction of Novaya Zemlya (p. 7)
- Included Table S2 with metadata of used TanDEM-X data (p. 11-18)

2) Response to the editor

Comments to the Author:

ORIGINALITY / NOVELTY

The mass loss of glaciers in the Russian Arctic has already been estimated using various techniques but this is the first time, to my knowledge, this is done using DEM differencing. This study provides a welcome, novel estimates that helps to constrain the acceleration of the mass loss in this high arctic region.

SCIENTIFIC QUALITY / RIGOR

The methods used here have already been applied and validated elsewhere. I missed a clearer explanation of the seasonal corrections (see below). Also the density conversion value should be better justified.

SIGNIFICANCE / IMPACT

This is a brief and solid study. To increased its impact, the comparison to earlier studies could be improved (same time periods see below) and receive more space in the main text.

PRESENTATION QUALITY

The material is concisely presented. The text is also well-written.

R: Thank you very much for the quick review and constructive comments. Our point-by-point responses are listed below:

Acquisition date correction. It seems to me that two effects need to be distinguished, correct me if I am wrong. First, the fact that measurements are not made at the same of the year and thus, because of the strong elevation/mass balance seasonal cycle, this may lead to overestimation of mass loss (because the changes are computed from an annual elevation maximum in winter to a minimum in Autumn). The second effect is radar penetration. I think it would help/convince the reader/reviewers if these two effects were well separated.

R: The differences in elevation change between the 2016/17 acquisition dates could be in fact related to either "real" physical changes of the surface heights (winter accumulation) or varying depths of radar signal penetration. The largest temporal offset exists for DEMs which were acquired in September 2016 (~35 % of total glacier area on Novaya Zemlya). For those areas, the accumulation of approximately 3 months of winter 2016/17 is not included in the observation period which would cause an overestimation of surface elevation change. Therefore, we included an analysis on surface elevation heights on overlapping glacier areas (i.e. areas which were measured in autumn and winter 2016/17 as well) in the revised manuscript. This comparison (Fig 1b) showed that actually the surface heights measured in September 2016 were at all altitudes higher than those measured in winter 2016/17. This is contradictory to the assumption of "missing" winter accumulation for the period winter 2010/11 to autumn 2016/17 because the presence of widespread melt in the High Arctic after September is very unlikely. Based on this observation, it is likely that the differences are related to varying depths of surface signal penetration between autumn and winter 2016/17.

Thus, we included an analysis on backscatter intensity, as suggested by both reviewers, which can be used to estimate signal penetration depths (e.g. Abdullahi et al., 2019). Using the backscatter

intensity and observed differences in measured surface heights (on all overlapping areas), we applied an empirical relation between September backscatter intensity and relative difference in surface penetration depth to adjust the elevation change rate of all glacier areas on Novaya Zemlya which were measured in September 2016.

To include the backscatter analysis and extended discussion on surface penetration, the former short methods description of the TanDEM-X DEM creation and co-registration was moved (and extended) to the supplement. This was also suggested by one of the reviewers. The new methods section in the main manuscript focuses on the observed differences in measured surface heights and backscatter. Additionally, we rephrased parts of the discussion section (see list of changes) to specifically discuss the observed relation between surface heights and radar backscatter properties.

Comparison to earlier studies. GRACE studies provide a continuous time series. Contacting the first authors of recent studies (Wouters/Ciraci), authors could get their time series and extract the exact same time period as them and make a more convincing comparison. I looked at the Wouters et al. time series (Their Figure 2) and, visually, did not find an obvious signal of acceleration.

R: An acceleration of mass loss in the Russian Artic (-1.2 Gt a^{-1}) over the period 2002-2019 was reported within the recent gravimetric study by (Ciracì et al., 2020). Also, for Novaya Zemlya, a gravimetric mass change measurement (Ciracì et al., 2018) for 2010-2016 was reported which is almost the same observation period as covered by TanDEM-X. We cited the respective publications in the comparison section of the discussion. However, we did not attempt to reconstruct the respective gravimetric mass change for the period 2010-2017 for Franz Josef Land and Severnaya Zemlya because it would be difficult to include this comparison within the page limits of the "Brief Communications" format in addition to the extended analysis on signal penetration.

The 900 kg/m3 density need to be justified. It implies that the authors entirely neglect firn compaction. An assumption not so straightforward in the context of rapid warming of high latitude ice caps.

R: We applied a volume-to-mass conversion factor of 900 kg/m3 in the initial submission to enable a straightforward comparison to some of the existing (gravimetric) regional studies which were conducted for the Russian Arctic archipelagos (e.g. Ciracì et al., 2018; Moholdt et al., 2012; Sánchez-Gámez et al., 2019). In the revised version we included additional mass change results using an average density scenario of 850 kg/m3 (Huss, 2013) and included an indication (Supplement Methods, L 64-65) that we do not consider potential changes in the density scenarios as there are no respective values available for this region.

Figure 1. color blind people may not be able to distinguish the "no coverage" color from the (rare) positive dh/dt values.

R: We changed the color scheme for the "no coverage" areas.

Maybe for Severnaya Zemlya (panel e) it would be good to separate the signal from the surging glacier in Vavilov from the rest of the region. So that Severnaya Zemlya can be compared to the other regions. Otherwise the signature of the Vavilov collapse is really strong in the dh/dt curve with elevation.

R: We removed the surging glacier part of Vavilov ice cap (RGI60-09.00971) from the hypsometric distribution of elevation changes of Severnaya Zemlya and adjusted the figure (and caption) accordingly.

Table 1. For Franz Josef land and Severnaya (and not Severnaja as mis-written), I do not understand why the error bars are changing between the columns. Can the authors clarify this?

R: The error bars between the dh/dt columns are different as we intended to provide different measures of the vertical uncertainty of the provided dh/dt. The numbers of the 1st column represented the "raw" vertical offset on all areas outside glacier areas (slope-weighted standard deviations, $\sigma_{\Delta h/\Delta t\ AW}$) while the 2nd column showed the "final" dh/dt uncertainty ($\delta_{\Delta h/\Delta t}$) as described in the methods section (e.g. including interpolation, spatial auto correlation, …). To avoid confusion, we moved the uncertainty values of dh/dt column 1 from Table 1 to a new table with off-ice accuracy statistics in the supplement (Table S1).

Figure 2. The rational for separating marine/land terminating glaciers for this section (penetration effect) was not clear to me. Can the authors also clarify this point in the text? Why such a separation is needed.

R: In the original version we separated between marine- and land-terminating glaciers as we assumed potential differences in the dynamics of those glacier types (e.g. higher flow velocities of marine-term. Glaciers). However, the magnitude of signal penetration is in fact independent from the terminus type of the respective glacier. In the revised version we estimated differences in surface signal penetration by using observed elevation differences on overlapping glacier areas (i.e. areas which were measured in autumn and winter 2016/17). For this analysis we did not separate marine- and land-terminating glaciers.

3) Response to reviewer 1

Paper
Brief communication: Accelerated glacier mass loss in the Russian Arctic (2010-2017)

The Cryosphere Discuss. https://tc.copernicus.org/preprints/tc-2020-358/
Comments to the authors

1) Summary and general comments

The presented work estimates the mass balance of three glaciated archipelagos of the Arctic Ocean in Northern Russia, namely Novaya Zemlya (NZ), Severnaya Zemlya (SZ) and Franz Josef Land (FJL). The three groups of islands are largely glaciated and were subject of several investigations related to their ice mass loss in the recent years using gravimetry and altimetry data. This study is based on elevation data from bistatic SAR satellite mission TanDEM-X and applies the meanwhile well-established method of calculating the ice surface elevation difference between DEMs acquired during the mission at different dates (here the winters 2010/2011 and 2016/2017). The methodology is one of the most precise for estimating spatially distributed, high resolution surface elevation change rates.
However, since NZ's mass loss is the largest of the three archipelagos (50% of the total) and because 70% of the TanDEM-X data in the winter 2016/2017 mosaic were acquired earlier, namely in September/October 2016, while the other two smaller archipelagos have each about a quarter contribution to the mass loss and the TanDEM-X data processed here were acquired in the same season a particular attention has to be given to the processing and analysis of NZ. Two problems arise here regarding the measured elevation changes:

1. the glaciological cycle is not fully covered missing parts of the accumulation period in the elevation change rate dh/dt.
2. the different reference surfaces of the InSAR DEMs from late summer/early autumn vs. winter introduce apparent changes in surface elevation due to differences in radar signal penetration.

Although these issues are addressed along the paper they are not clearly separated and the effects on the results are confusing. The uncertainty assessment of the mass change rate presented in this paper consists of three terms, the vertical coregistration being explained in detail. This term includes also the effect of SAR signal penetration as the factor S_{pen} (eq 2) resulted from the winter-autumn (WA) and winter-winter (WW) elevation changes of NZ. The authors apply a bulk correction which is a questionable approach because the surfaces of NZ glaciers extend over an elevation range of 1500 m and thus include ice/snow volumes of very different penetration properties, from close to zero for glaciers ice to several meters in the upper sections of the accumulation area (if dry). The elevation range implies a typical temperature difference of about 10 °C, so that – in particular in late summer and autumn – surface melt all over is rather unlikely. This indicates the need for localised penetration corrections, e.g. using backscatter coefficients in SAR amplitude images for assessing the melting state. The SAR backscattering coefficient on each archipelago is a more precise indicator than one mean monthly value of skin temperature (see specific comments).

R: Initially we would like to thank the reviewer for the detailed and comprehensive comments. We agree that the correction of elevation differences (likely related to signal penetration) based on monthly average temperatures cannot fully explain local variations in surface conditions. Therefore, we replaced it in the revised version of the manuscript by a more detailed analysis of backscatter intensity and measured elevation change on glacier areas acquired in different months.

Concerning the main comments regarding the elevation change measurement on Novaya Zemlya:

1) **As mentioned by the reviewer, the accumulation of Novaya Zemlya of the last year of the observation period is not entirely covered due to a temporal offset between the TanDEM-X acquisitions of 2010/11 and 2016/17. While the observation periods of most pixels start in winter 2010/11 (December & January), for some pixels the end date is in autumn 2016. Due to this shift, the observation period does not cover 2-3 months of winter accumulation for some glacier areas. Therefore, it is expected that the derived elevation change measurements would overestimate the actual surface lowering. Yet, the comparison of winter-to-winter and winter-to-autumn elevation change measurements indicate that the surface heights measured by TanDEM-X during winter 2016/17 were lower than the surface measured in autumn 2016 at all altitudes. It is not likely that this offset is primarily caused by physical changes of the surface heights between the acquisitions because widespread surface melt at all altitudes after late summer/autumn is not very likely in the Arctic. Also, the average vertical offset (> 2 m) between autumn and winter elevations (chapter 2.3, 3 & 4) is high compared to the average elevation change rate over the entire observation period. An analysis of backscatter (see point 2) on glacier areas, which were acquired in both seasons, indicates that the observed differences are rather related to differences in signal penetration depth (e.g. by days with melt in September) than to physical changes of the surface heights. We therefore provided a correction estimate which is based on the local backscatter intensity (see comments below).**

2) **We extracted backscatter intensities of all DEM data of Novaya Zemlya (2016/17) to analyze potential differences in surface conditions (and thereby differences in signal penetration) as suggested. By comparing the change of backscatter intensity versus elevation for different acquisition dates (Fig. 1), the acquisitions of September 2016 showed significant differences while the backscatter values of the September-January (2016/17) DEMs are relatively similar at all altitudes. The respective backscatter values are also displayed in Fig. 1a and Fig. S1 in the manuscript and supplement. While September backscatter intensity is relatively similar for altitudes below ~400 m a.s.l. and intensity values ~ -20 db, it diverges at higher altitudes. Thus, we applied the revised offset correction only for glacier areas which were acquired in September 2016 because the backscatter indicates the largest change in surface conditions for those areas.**

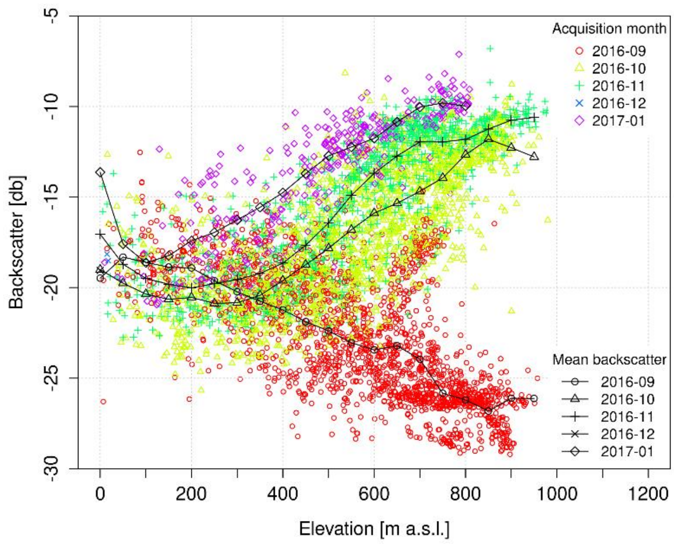

Fig. 1 Backscatter intensity versus elevation of TanDEM-X acquisitions on Novaya Zemlya. Black lines indicate the mean DEM backscatter of each acquisition month.

To account for the observed differences in backscatter between the acquisition dates we also revised the correction approach: We removed the bulk estimate of the mean elevation difference between September and winter DEMs. Instead, we applied a regression based on local backscatter intensity and altitude. To fit the correction model, we extracted the elevation difference and backscatter on all overlapping glacier areas (i.e. areas which were acquired in autumn and winter 2016/17). The mean backscatter of the overlapping areas diverges at altitudes above 400 m a.s.l. and backscatter intensities of approximately -20 db, which is similar as the altitudinal distribution of backscatter intensity of all acquisitions on Novaya Zemlya (Fig. 1). We then transferred the model to all glacier areas which were acquired only in September 2016 (Fig. 2a) and used the respective backscatter values to estimate a vertical correction value for each pixel (Fig. 2b). Fig. 2a (Fig. S2a in the supplement) shows the reference (overlap) areas as well as those areas which were eventually corrected. The vertical correction values are provided in Fig. 2b (Fig. S2b).
The analysis of the overlapping glacier areas and the transfer of the correction model to all other autumn DEMs is described in chapter 2.3 & 3 in the revised manuscript.

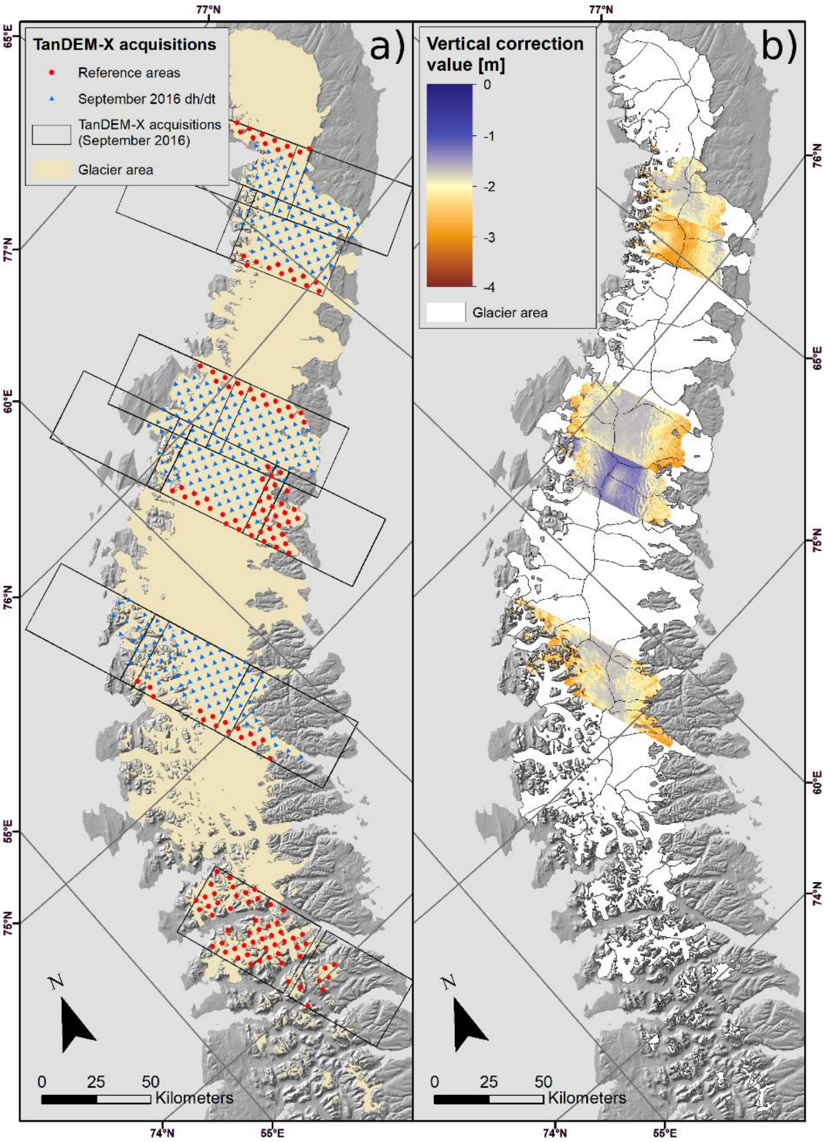

Fig. 2 a) Overview of glacier areas which were acquired during September and winter 2016/17 (red dots) and areas which were measured only in September 2016 (blue triangles). b) Estimated vertical offset between September 2016 and winter 2016/17 TanDEM-X acquisitions.

Eventually, we made some changes of the manuscript structure to focus on the adjustments applied to the elevation change rate of Novaya Zemlya:

***The methods sections describing the interferometric DEM creation and co-registration were moved (and extended) to the supplement (pages 2-4) because those sections closely follow previous publications.**
***The description and discussion of the penetration and elevation change on Novaya Zemlya in the main manuscript was extended.**
***Chapter 2.2 and 2.3 now describe the analysis and correction of different backscatter.**
***We changed the order of figures and extended Fig. 1 (former Fig. 2) with two additional panels on backscatter and estimated difference in signal penetration.**

Please find our point-by-point responses below:

Specific comments, minor comments & typos

Line 55: …$\times \sigma^2_{\Delta h/\Delta t\ AW}$… (in case this equation comes from the spherical variogram model)
R: Equation corrected

Line 57: correct subscripts (Scor, SG)
R: Subscripts corrected

Line 73: delete **/17** after autumn 2016. The explanations of the polynomial correction are insufficient and the results over NZ aren't traceable. See comments below.
R: We changed the correction estimate for Novaya Zemlya and extended the respective methods section. The original polynomial correction was replaced by a linear regression based on backscatter intensity and altitude (see general responses above and chapter 2.3, Line 71-83).

Line 75 ff. melting/ penetration: the presence of melt should be assessed by checking the backscatter coefficients. In case differences in sigma0 between the data used for retrieving the elevation change are indicating differences in signal penetration, these should be corrected.
R: Included analysis of backscatter intensity and respective correction (chapter 2.2 & 2.3). The DEM acquisitions of September 2016 showed large differences in backscatter and were accordingly adjusted.

Lines 77 and 78: replace "images" with "TanDEM-X data" or "SAR data"
R: Replaced "images"

Line 94 to 98 and Fig. 2a: According to this analysis the period December-April 2010/11 to November-January 2016/17 (WW) shows higher average rates of elevation loss (dh/dt) than the period December-April 2010/11 to September-October 2016 (WA). This is contrary to the expected behaviour if the annual mass balance cycle is taken into account (as I mentioned at point 1. the winter accumulation is partly missing). A possible explanation could be a bias in the penetration correction. Fig. 2b: Novaya Zemlya extends from 71 N to 77 N and 0 m to 1500 m a.s.l. A single mean monthly mean skin temperature is not a useful indicator for estimating the melting state as major spatial and temporal differences have to be expected.
R: As suggested, we replaced the mean monthly skintemperature by the backscatter intensity as indicator for changing surface conditions. The revised correction method uses local backscatter and altitude. This approach accounts for different altitudes and different states of the glacier surface across the Novaya Zemlya ice cap (see comments above and revised manuscript).
In addition, the discussion on potential effects of missing winter accumulation and differences in signal penetration was extended (chapter 4, Line 115-138).

Line 100 ff.: Which "differences in the SAR derived elevation change rates" exactly? Cross reference Fig 2a if you are referring to the dh/dt of NZ for WW and WA periods. Or is a general statement?

R: Yes, this refers to autumn/winter DEMs on NZ. Included reference to Fig 2a

Line 104: If accumulation is only partly included in the 2016/17 elevation this would lead (without applying a correction) to an overestimation of the surface elevation loss.
R: We replaced "overestimation of surface elevation change" with "overestimation of surface elevation loss" to clarify this sentence.

Lines 105: … elevation change of the WA period is less negative than of the WW period at all altitudes, …
R: Corrected and rephrased sentence.

Line 107 replace "decreases during melting conditions" with "is close to zero for melting snow surfaces and for bare glacier ice in general".
R: Ok

Line 117: I see in Fig S3a reddish areas (warmer temperatures) on the ocean and on the northern islands (FJL, SZ) not on the southern islands (NZ). The skin temperature does not show any temporal trend for the glaciers on NZ, supporting the comment above that the temperatures shown in Fig. 2b are not representative for the main glacier areas. The skin temperature increase is most pronounced on the ocean, due to the decrease in sea ice coverage.
R: We agree that the warming trend in skin temperature is most pronounced on the northern archipelagos and ocean areas. However, the ERA5 dataset indicates a general increase in temperature for the entire region and the interior of Novaya Zemlya (<= +1 C°/dec). In Fig. S1 (monthly mean temperatures of TanDEM-X acquisition months) we also replaced the ERA5 data with the ERA5 Land dataset which provides a better spatial resolution and does not include ocean areas.

Line 133: "indicate"
R: Ok

Line 254: Caption of Table 1: Overview of glacier elevation and mass change in the Russian Arctic between 2016 and 2017. This is probably a typo and should mean "2010/2011 to 2016/2017".
R: Yes, replaced wrong year numbers

Line 275: The **a)** and **b)** notations on Fig 2 are missing.
R: Extended figure and added missing notations.

Supplement

Fig. S1: Total precipitation units cannot be meter/day, but mm/day. Please add reference/data source for the ERA5 temperatures and precipitation and give the locations of the measurements.
R: We changed Fig. S1 and removed the precipitation plots. Instead backscatter intensity and average temperatures are compared for the acquisition months. Concerning the temperature values, we added the respective reference and locations of the used reanalysis data in the caption and in the figure. We also changed the reanalysis data and used the ERA5 Land product for the revised version instead of the ERA5 product. The ERA5 Land product does not include ocean areas and provides a better spatial resolution for land areas.

Fig S3 the **a)** and **b)** notations are missing. But references to climate data related studies of the same period are more convincing in demonstrating the long term trends than the trends shown in these figures.
R: We added the notations to Fig S4 (former Fig S3) and included a reference to a recent study about Arctic climate trend (Jansen et al., 2020) **in the discussion section of the main manuscript.**

Please add a table with the specifications for the TanDEM-X database used in the study.

R: We added Table S2 to the supplement which includes metadata of the used TanDEM-X acquisitions.

4) Response to reviewer 2

Interactive comment on "Brief communication: Accelerated glacier mass loss in the Russian Arctic (2010–2017)" by Christian Sommer et al.

Anonymous Referee #2

Received and published: 4 March 2021

This manuscript provides new geodetic estimates of glacier mass balance for the three main Russian Arctic archipelagos (Novaya Zemlya, Severnaya Zemlya, Franz Josef Land) and briefly discusses the results. The two most novel aspects of the study are that near-complete coverage of glacier elevation changes is obtained, and that the results indicate an increase in mass loss compared to earlier periods and studies.
The authors use digital elevation models (DEMs) derived from SAR interferometry of the TanDEM-X mission. This has the advantage of providing near-complete repeat coverage of glacier areas (93%), but can suffer from variable X-band radar signal penetration in snow/ice between satellite acquisitions. This is one of the main discussion points of the paper, and a correction-scheme is proposed for Novaya Zemlya where seasonal acquisition times were most different. Meteorological reanalysis data and supplementary DEM analyses are presented to support the approach, and results are provided both with and without penetration correction, as well as for two different density assumptions in the conversion between volume and mass change.
The main results appear plausible and relatively robust overall, but the differences related to acquisition times on Novaya Zemlya are puzzling and do not give a strong justification for the applied correction scheme. The potential magnitude and mechanisms of seasonal penetration differences are not well described or discussed, and the relevant parts of the manuscript (mainly Section 2.3) brings more confusion than clarity. For example, the paper does not say anything about the spatial coverage of the autumn and winter data of 2016/17 (Do they cover areas of potential different glacier change? Is there any overlap so that the two periods can be compared directly?) or if winter snow is partly accounted for in the co-registration process over land areas, which would limit the need for seasonal correction. See the specific comments below for further details on this issue.
The manuscript is written in a Brief Communication format, which is probably related with the authors' previous publications with similar methodology in other glacier regions, but I think that the present version suffers from too short/unclear methodology and very limited discussions. I think a lot of this can be fixed with improved writing and referencing, and perhaps by moving parts or all of Sections 2.2 (uncertainty assessment) and 2.3 (Dem Acquisition date correction) to the Supplement as these two sections are not satisfactory in the present form (see specific comments below). Alternatively, the manuscript could be expanded to a normal paper by making more complete data/methods sections and expanding the discussion of observed glacier changes which is now very brief. In any case, some major revisions are needed regarding these aspects.

Initially we would like to thank the reviewer for the detailed and comprehensive comments.

Concerning the "Brief communications" format, we decided to use this short type of manuscript because the presented method and datasets have been described in a number of previous publications and the only significant changes are related to the temporal offsets between DEM acquisitions on Novaya Zemlya. However, we agree that the description of the workflow suffered from the short format. Therefore, we moved the description of the interferometric DEM creation and associated uncertainty section, as suggested, to the supplement because those chapters follow

closely our previous publications. We also extended those method sections and inserted additional references which are related to the processing workflow. Within the main manuscript, we extended the description and discussion of radar signal penetration (new chapter 2.2 and 2.3) and included further figures (Fig. 1a/b, Fig. S2).

As suggested, we also revised the correction for temporal offsets of the DEM acquisitions on Novaya Zemlya:

- **We included an analysis of differences between backscatter intensities during different acquisition months. The observed hypsometric distribution of backscatter intensity (revised manuscript: Fig. 1a & Fig. S1) indicates significant changes in surface conditions for the September 2016 acquisitions while backscatter intensity of the other acquisition months is relatively similar (Fig. 1).**
- **To estimate the vertical difference between September and winter acquisitions, we derived differences in surface elevations and respective backscatter from glacier areas which were acquired in September and winter 2016/17 (Fig. S2, ~3000 km²). The extracted vertical difference and respective September backscatter intensities are used to fit a linear regression model (chapter 2.2 & 2.3).**
- **Thereafter, the model is transferred to all glacier areas which were only acquired in September 2016 and the elevation change rate is adjusted accordingly. The overlapping (reference) glacier areas and those areas which were only acquired in September 2016 are indicated in Fig. 2a (revised manuscript: Fig. S2a). Fig. 2b shows the applied vertical correction values (Fig. S2b).**

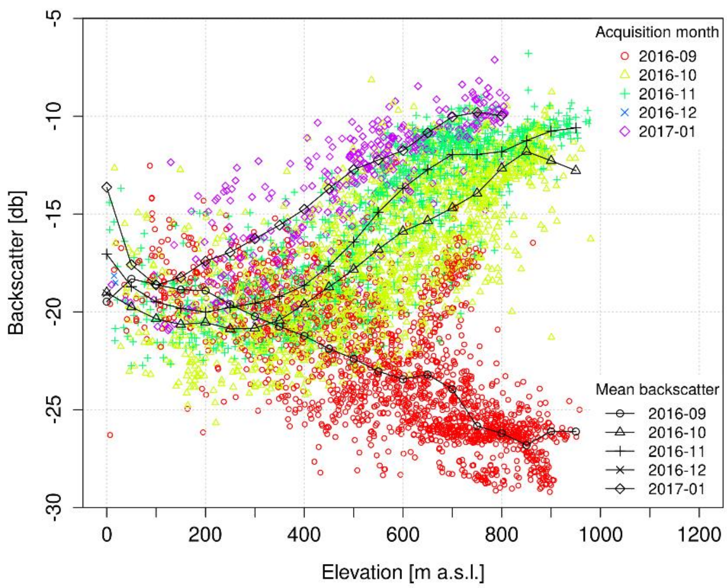

Fig. 1 Backscatter intensity versus elevation of TanDEM-X acquisitions on Novaya Zemlya (2016/17). Black lines indicate the average backscatter intensity of each acquisition month.

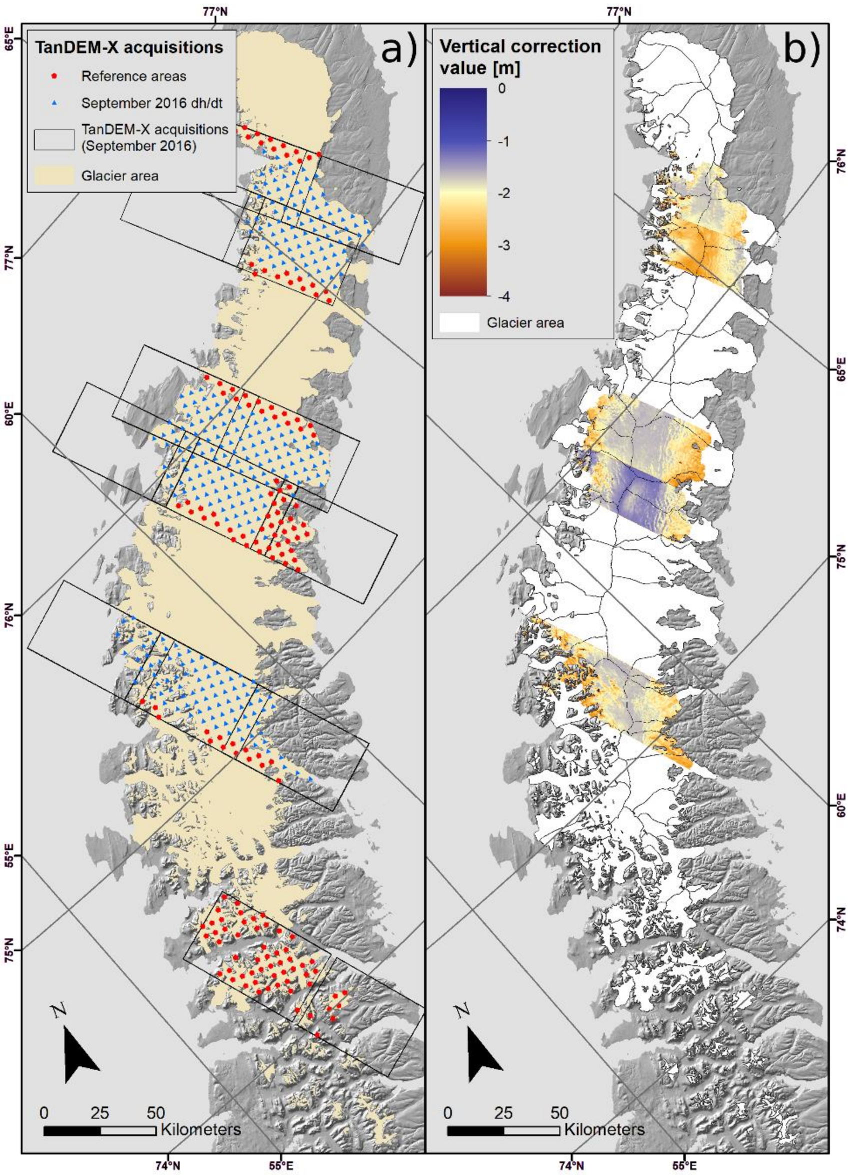

Fig. 2 a) Overview of glacier areas which were acquired by TanDEM-X during September and winter 2016/17 (red dots) and areas which were measured only in September 2016 (blue triangles). b) Estimated vertical offset between DEM acquisitions of September and winter 2016/17.

The applied correction and further details of the extracted elevation and backscatter values are described in the revised chapter 2.2 and 2.3 and Fig. 1.
Please find our point-by-point responses below:

Specific comments and edits:

Title: Since parts of Siberia is often considered to be in the Russian Arctic and there are areas with small mountain glaciers there, it would be more precise to say "Russian High Arctic" or "Russian Arctic archipelagos" in the title and elsewhere in the manuscript. Also, I think that "increased" is a more correct term than "accelerated" considering your results in relation to other studies.
R: The term "Russian Arctic" in the manuscript refers to the regional subdivision of the Randolph Glacier Inventory which comprises the archipelagos of Franz Josef Land, Severnaya Zemlya and Novaya Zemlya as "Russian Arctic". But we agree that the term might be confusing and replaced it

with "Russian High Arctic" in the title and abstract. Additionally, the title was changed to: "Increased glacier mass loss in the Russian High Arctic (2010-2017)"

L7: I assume you mean "atmospheric warming" or "surface warming", not the thermal state of the glaciers.
R: Yes, included "atmospheric"

L15: This reference only considers one region. Please provide a few other similar refs or a more general one covering multiple regions. Russian Arctic
R: We included some other studies which focus on (increasing) glacier mass loss during recent years (~ >2010): (Zheng et al., 2018; Ciracì et al., 2020)

L21: Or more broadly: "...and various corrections related to surrounding oceans, surface hydrology and glacial isostatic adjustment (GIA)."
R: Sentence changed accordingly

L30: What is the CoSSC tile product? Write out the acronym as a minimum.
R: Included: "…Coregistered Single look Slant range Complex (CoSSC)…" which is the product specification by the data provider of TanDEM-X.

L30: "Compared with..." – what do you actually mean? "Unlike..." or "Similar to..."
R: In previous studies on glacierized regions outside the Arctic we used the SRTM DEM as reference surface while in the Arctic we applied the TanDEM-X Global DEM because SRTM was not acquired beyond 60°N. We therefore changed the beginning of the sentence to "Unlike previous studies (), …"

L34: Did you cross-check this coastline against the glacier inventory to make sure no glacier areas were excluded? Please specify in the text to make this clear.
R: Yes, the OpenStreetMap coastline was visually inspected and adjusted in areas where it did greatly differ from glacier areas of the Randolph Glacier inventory. Most changes were related to the glacier tongues of marine-terminating glaciers which also changed since the acquisition of the Randolph glacier inventory (see comment L40). Also, a small inverse buffer was applied to the coastline to account for an insufficient separation between land (stable ground for co-registration) and ocean/sea ice on some of the smaller islands of Franz Josef Land and Severnaya Zemlya. We added a respective explanation in the methods section of the supplement.

L35: This relates to the sentence at L30. Please combine similar content at one place.
R: Combined content with first sentence of chapter 2.1

L36: Somewhat unclear. After a few reads I understand it as .... 2010/11 co-registered to Global DEM and mosaiced ... then 2016/17 co-registered to the 2010/11 mosaic to make a 2016/17 mosaic. Please clarify the text.
R: Yes, extended & clarified the explanation. The DEM-creation methods section was moved to the supplement and extended.

L37: I understand this as dividing by decimal numbers of years according to the dates of the source tiles. But that's confusing since you are differencing DEM mosaics. Does that mean you also made a mosaic layer of time differences? Or did you divide by an integer number of years (6) everywhere which would make more sense in a climatic mass balance perspective? Either approach could be justified, but this not discussed at all although it could have a significant impact on the results.
R: Yes, a mosaic layer of time differences is created alongside the 2010/11 and 2016/17 DEM mosaics. This layer provides for each raster cell the exact time difference (as decimal number of years) between the acquisitions. We use this to calculate an individual elevation change rate (m/a)

for each elevation change value with the respective start and end date. We included this in the extended supplement methods.

L39: Would be good to refer Fig. 1 here since the altitude dh/dt function is shown there.
R: Included reference to Fig. 1

L40: Isn't the inventory applied earlier than this, e.g. for the void filling? Also, the inventory is somewhat outdated, so what was done (or not) for glaciers that have undergone major changes such as the advancing Vavilov ice cap. The altitude-dependency of dh/dt in Fig S2 indicates that the Vavilov advance has been accounted for, whereas the less negative dh/dt of the lowermost altitudes of land-terminating glaciers in NZ indicate an impact from retreat which shouldn't influence overall mass rates (Gt/y), but could impact the area-specific rates (m/y). A brief discussion of these matters would be good to have somewhere in the manuscript. Note that there is a newer inventory for Novaya Zemlya (Rastner et al., 2017) which could be relevant for context or comparison.

R: The Randolph Glacier Inventory of the Russian Arctic archipelagos was created from optical images between 2000 and 2010 but there is no specific timestamp provided within this period for a number of glaciers. We made some manual adjustments as the retreat of some of the major (marine-terminating) outlet glaciers and of course the surge of the Vavilov ice cap were not covered by the original inventory.
A comparison with the recent inventory for Novaya Zemlya (Rastner et al., 2017) also indicated that most changes in glacier outlines are related to the retreat of outlet glaciers along the coastlines. The total glacier areas of Novaya Zemlya provided by the Randolph inventory (~22,128 km²) and Rastner et al. 2017 (~22,379±246 km²) are very similar.
Unfortunately, there are no other recent inventories which cover the remaining glacier areas of Severnaya Zemlya and Franz Josef Land. Therefore, we decided to use the (modified) Randolph inventory as it provides a homogeneous glacier area dataset for the entire region.
The less negative elevation change rates of the lowermost elevation bins are related to glacier retreat during the observation period and the temporal offset between outlines and DEM (we also included this in the caption of Fig. S3). It is not possible to update the entire inventory due to a lack of cloud-free images in this region.
We included a small section in the supplement methods to describe the applied glacier inventory.

L42-43: It's not the scenarios that change, but the firn pack. Rewrite sentence to make it clear what you actually mean here. Also, do you consider this issue to be within the error estimates you provide or as something that comes on top of that (i.e. not considered).
R: Changed sentence to "Possible changes in the glacier ice density (e.g. firn compaction) …" (supplement methods, Line 64-65).
The suggested uncertainty of ±60 kg m-3 (Huss, 2013)**, which is included in our uncertainty estimate, is recommended for observation periods of more than 5 years, the presence of firn and volume changes different from zero. However, the mentioned study reported that this mean conversion factor can significantly vary under different conditions. As there are no observations of glacier density in the Russian High Arctic, we cannot quantify a region-specific uncertainty value for the volume to mass conversion.**

L44: Unclear and not strictly correct. It does include frontal melt/calving when that balances the ice outflux, but it does not include subaqueous glacier volume changes related to advance or retreat. This should be made clear, and also its potential relevance for the overall glacier mass balance and sea-level contribution, here or in the discussion.
R: Rewrote sentence (supplement methods, Line 66-67).

L45: The uncertainty section is not understandable by itself and needs to be rewritten. There are parameters that are not fully explained, units are unclear, and it is hard to follow the logic unless a lot of time is spent with Table S1 and given references.
R: We moved the uncertainty section to the supplementary materials and extended the description of the applied workflow and equations.

Eq. 1: Is this equation from previous work or is it unique for this study? It appears like mass rate uncertainty is a factor of the mass rate itself which does not make sense to me if the mass rate turn out to be near zero.
R: Equation 1 is from previous studies, e.g. (Braun et al., 2019; Seehaus et al., 2019) and was only slightly modified for this study because we added an estimate of the signal surface penetration (-> winter to autumn acquisitions) directly to the elevation change uncertainty (and thereby also to the mass change uncertainty). In previous studies, surface penetration was estimated as a "bias volume" and thus only included in the volume/mass change uncertainty.

L56: Is Sg ever larger than Scor here? If not, then it's confusing to include this equation. I understand it as you are calculating errors per region, not per glacier.
R: Yes, this part is for the large ice bodies of the Russian High Arctic not relevant. Still, we would like to keep the entire equation in the methods section because of consistency with previous publications of the presented uncertainty calculation.

L60: How was this number found? Not clear from Section 2.3. It is also unclear if the approximate 2 m penetration difference (Spen) is applied only to the NZ autumn data or to all data in all regions which would make most sense.
R: This number was the originally determined offset value between autumn and winter acquisitions on Novaya Zemlya. We changed the respective analysis and descriptions in the text (and in the supplement methods). The revised vertical offset value for Novaya Zemlya (~ 2 m) is derived by an analysis of backscatter intensity on overlapping glacier areas (chapter 2.2 & 2.3 in revised manuscript). For the elevation change uncertainty, we applied this value to Franz Josef Land and Novaya Zemlya but weighted it with the respective autumn area because the difference in surface penetration is expected to be small or zero for acquisitions from the same season. For Severnaya Zemlya we used an estimate of average penetration depth because all DEMs were acquired in the same season (see supplement methods section).

L64-79: I like the comparative elevation differencing from winter 2010/11 to autumn (WA) and winter (WW) 2016/17, respectively, and I agree it might be the best way to try to account for errors related to signal penetration, but the logic is too simplified. Is it just melting or non-melting surface condition that is relevant? Widespread melting conditions are unlikely after mid-September, and ERA5 is too coarse to capture topographic temperature variations. In that context, I would consider differences in SAR backscatter to be relevant. And how deep can the X-band signal penetrate? There is no mention or references regarding that. For example, is the last summer-surface a dominant reflection horizon during winter or can it penetrate even deeper. In the latter case, the meteorological conditions of previous years might also matter.
R: The depth of surface penetration of the X-band radar strongly depends on the prevailing surface conditions during the DEM acquisition. In general, penetration is low for melting conditions and high for dry and frozen surfaces. For the DEM-differencing in this study, the relative difference between the penetration depths of the acquisitions at the beginning and end of the observation period is relevant. It is likely that this difference is small or zero for acquisitions of similar seasons or dates but increases when comparing DEMs of different seasons. Therefore, we included a more specific analysis of local radar backscatter intensity and the related differences in measured surface elevation (revised chapter 2.2 & 2.3) to account for the temporal offsets between acquisitions of

autumn and winter 2016/17 (see response to general comments). Additionally, chapter 2.2 includes now a general description of signal penetration and respective references.

L84: Fig. S2 shows altitude dependency, not whether a glacier is small or large. Rephrase or refer to Fig. 1 instead where it does seem like the largest glacier fronts thin the most.
R: Changed figure reference to Fig. 2 (former Fig. 1).

L94: Unclear. Rather something like this: "Relations between acquisition times, monthly temperatures and derived elevation change rates for NZ are shown..."
R: Rephrased/changed this part of the results section.

L98: Redundant wording; elevation gains are always positive.
R: Removed "positive"

L102-104: True if no penetration, whereas if fresh cold snow is transparent then it can be considered as autumn 2010 to autumn 2016 changes, with no seasonal snow bias.
R: The part about signal penetration in the discussion section was rewritten and extended. The discussion of potential offsets in measured winter accumulation or signal penetration differences has been extended. We also included the radar backscatter as indicator of changing surface conditions between September 2016 and winter 2016/17.

L112: This is also what I speculated (see previous comment), but then dh/dt from the WA and WW periods should have been more or less similar, which is not the case.
R: please see comment above

L113-115: I don't understand the logic here. Are you suggesting penetration into the firn/ice during winters and near-surface reflection during autumn? If so, you are in practice measuring a "delayed mass balance" (shifted backwards in time).
R: Yes, it is likely that the penetration in winter 2010/11 and 2016/17 was higher (but similar in both cases) while in September 2016 the measured elevations were closer to the actual glacier surface (less penetration). We extended and rewrote this part of the discussion.

L117: The figure indicates largest warming for the northern islands (FJL and SZ) and smallest for the southern ones (NZ), which is opposite of what you say. But warming might still have a larger impact in the south since climate is in general warmer and closer to the melting point. The most relevant aspect for this paper would be how 2010-17 stands out from the longer-term climate, especially during the summer melt season. Any relevant references that have studied climate change in this region in more detail?
R: To our knowledge there are no recent studies which analysed the Russian High Arctic specifically. We inserted a recent reference of climate trend analysis in the entire Arctic (Jansen et al., 2020)**. In the revised manuscript, Line 117 was removed and combined with Line 159.**

L119: What about the comparable Wouters et al. (2019) paper?
R: Added Wouters et al. 2019

L121: How much of your mass loss is related to the surge of Vavilov ice cap? Would there be a substantial remaining mass loss if dynamic areas of Vavilov and Academy of Sciences ice caps were excluded? I miss such aspects of the discussion.
R: The Glacier elevation change of the Severnaya Zemlya archipelago would be approximately half as negative without the outlet glaciers of the Vavilov and Academy of Sciences ice caps. We included this in Line 144-146.

L123-l30: The study of Melkonian et al. (2016) is also very relevant for this discussion, considering both long-term elevation changes and ice dynamics.
R: Included Melkonian et al. (2016), Line 150: "(Carr et al., 2014). Long-term observations also indicate a more rapid thinning during recent years, particularly at the termini of marine-terminating glaciers (Melkonian et al., 2016)"

L129: are not always related to -> does not seem to be related to
R: Ok, changed

L132: Zhang et al. (2018) is also very relevant here (only referenced in the Supplement)
R: Included (Zheng et al., 2018)

L137: showed -> has shown
R: Ok, changed

L138: ...between 2010 and 2017
R: Included

L139: Unclear. Do you mean that Arctic glacier mass losses are increasing more than non-polar ones? If so, in total or specific rates?
R: This sentence refers to the sea level rise contribution of different glacierized regions during the last decades. At the end of the 20th and beginning of 21st century, many Arctic glaciers showed small elevation changes or even balanced conditions. Their contribution to sea level rise was therefore rather small compared to glacier outside the Arctic which showed much higher melt rates. Various studies indicate that this pattern is changing in recent years and increasing melt rates are also measured in the polar region. While specific change rates of Arctic glaciers are still less negative than those of mountain glaciers outside the polar regions, the total mass loss (and therefore also the contribution to sea level) is higher due to the very large glacier areas.

L140: You are basically listing all regions except Svalbard. Is this sentence needed?
R: Removed sentence

Fig. 1: Nice figure. Is it possible to also show the autumn (A) versus winter (W) coverage of DEMs in the 2016/17 seasons? Or in the supplement to keep this figure clean.
R: We included another map of Novaya Zemlya in the supplement which shows glacier areas covered in autumn and winter 2016/17 (Fig. S2).

Table S1: You seem to use AW here as an abbreviation for area-weighted, which is confusing because you use AW as an abbreviation for autumn-winter elsewhere in the manuscript. And at L51 you write slope-weighted instead of area-weighted.
R: We removed the abbreviation for autumn-winter in the manuscript because the correction was changed to September areas only.

Fig. S1: Are the climatological data extracted for the entire regions or specifically for the glacier areas? I don't think that is mentioned anywhere in the manuscript.
R: The climate data used for the glacier regions was changed to the ERA5 Land product (which provides a better spatial resolution) in the revised version. We also added specifications about the extracted area in the caption and directly in the plots. The regional data was extracted with a bounding box with the extent of the glacier inventory of each archipelago. Ocean areas are not included.

Fig. S4: Nice compilation of results. For FJZ, it should be Zheng et al. (2018), not 2019 which is another paper.
R: Thank you very much, changed Zheng et al. (2019) to (2018).

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