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 (and extended) the description of the interferometric DEM creation and associated uncertainty section to the supplement because those chapters follow closely our previous publications. Instead, we extended the description and discussion of radar signal penetration in the main manuscript (new chapter 2.2 and 2.3) and replaced the temperature-

based correction of autumn elevations on Novaya Zemlya with an analysis of backscatter intensity. We also included further figures (e.g. spatial distribution of different DEM acquisition dates).

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.
***Changed title 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.
***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
***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).”
***Sentence changed accordingly**

L30: What is the CoSSC tile product? Write out the acronym as a minimum.
***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...”
***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.
***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.
***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.
***Yes, extended & clarified the explanation**

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.

***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.

***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.

***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. 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).

***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.

***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.

***We moved the uncertainty section to the supplementary materials and extended the description.**

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.

***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.

***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.

***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).**

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.

***We included a more specific analysis of local radar backscatter intensity and the related differences in measured surface elevation (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.

***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..."

***Rephrased/changed this part of the results section.**

L98: Redundant wording; elevation gains are always positive.
***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.
***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.
***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).
***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?
***To our knowledge there are no recent studies which analysed the Russian High Arctic specifically. We inserted a recent reference of climate analysis in the entire Arctic. The sentence was removed and combined with Line 159.**

L119: What about the comparable Wouters et al. (2019) paper?
***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.
***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.
***Unfortunately, we could not include all available references in the discussion as there is a limit for the number of citations for the brief communications format.**

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

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

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

L138: ...between 2010 and 2017
***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?
***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?
***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.
***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.
***We removed the abbreviation for autumn-winter in the manuscript.**

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.
***The climate data used for the glacier regions was changed to the ERA5 Land product (which provides a better spatial resolution) and added specifications about the extracted area in the caption and directly in the plots.**

Fig. S4: Nice compilation of results. For FJZ, it should be Zheng et al. (2018), not 2019 which is another paper.
***Changed Zheng et al. (2019) to (2018)**

References

Melkonian, A. K., M. J. Willis, M. E. Pritchard, and A. J. Stewart (2016), Recent changes in glacier velocities and thinning at Novaya Zemlya, Remote Sens. Environ., 174, 244-257.

Rastner, P., T. Strozzi, and F. Paul (2017), Fusion of Multi-Source Satellite Data and DEMs to Create a New Glacier Inventory for Novaya Zemlya, Remote Sensing, 9(11).

Braun, M. H., Malz, P., Sommer, C., Farías-Barahona, D., Sauter, T., Casassa, G., Soruco, A., Skvarca, P., and Seehaus, T. C.: Constraining glacier elevation and mass changes in South America, Nat. Clim. Change, 9, 130–136, https://doi.org/10.1038/s41558-018-0375-7, 2019.

Ciracì, E., Velicogna, I., and Swenson, S.: Continuity of the Mass Loss of the World's Glaciers and Ice Caps From the GRACE and GRACE Follow-On Missions, Geophys. Res. Lett., 47, https://doi.org/10.1029/2019GL086926, 2020.

Huss, M.: Density assumptions for converting geodetic glacier volume change to mass change, The Cryosphere, 7, 877–887, https://doi.org/10.5194/tc-7-877-2013, 2013.

Seehaus, T., Malz, P., Sommer, C., Lippl, S., Cochachin, A., and Braun, M.: Changes of the tropical glaciers throughout Peru between 2000 and 2016 – mass balance and area fluctuations, The Cryosphere, 13, 2537–2556, https://doi.org/10.5194/tc-13-2537-2019, 2019.

Zheng, W., Pritchard, M. E., Willis, M. J., Tepes, P., Gourmelen, N., Benham, T. J., and Dowdeswell, J. A.: Accelerating glacier mass loss on Franz Josef Land, Russian Arctic, Remote Sens. Environ., 211, 357–375, https://doi.org/10.1016/j.rse.2018.04.004, 2018.