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The Cryosphere Discussions

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

Anonymous Referee #2

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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.

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.

L7: I assume you mean “atmospheric warming” or “surface warming”, not the thermal state of the glaciers.

L15: This reference only considers one region. Please provide a few other similar refs or a more general one covering multiple regions. Russian Arctic

L21: Or more broadly: “...and various corrections related to surrounding oceans, surface hydrology and glacial isostatic adjustment (GIA).”

L30: What is the CoSSC tile product? Write out the acronym as a minimum.

L30: “Compared with...” – what do you actually mean? “Unlike...” or “Similar to...”

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.

L35: This relates to the sentence at L30. Please combine similar content at one place.

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.

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.

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

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.

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).

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.

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.

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.

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.

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.

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.

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.

L94: Unclear. Rather something like this: “Relations between acquisition times, monthly temperatures and derived elevation change rates for NZ are shown...”

L98: Redundant wording; elevation gains are always 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.

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.

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).

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?

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

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.

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.

L129: are not always related to -> does not seem to be related to

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

L137: showed -> has shown

L138: ...between 2010 and 2017

L139: Unclear. Do you mean that Arctic glacier mass losses are increasing more than non-polar ones? If so, in total or specific rates?

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

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.

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.

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.

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

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).

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