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The Cryosphere Discussions

Interactive comment on "Ice loss in High Mountain Asia and the Gulf of Alaska observed by CryoSat-2 swath altimetry between 2010 and 2019" *by* Livia Jakob et al.

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Review on the manuscript entitled "Ice loss in High Mountain Asia and the Gulf of Alaska observed by CryoSat-2 swath altimetry between 2010 and 2019" by L. Jacob et al. submitted to The Cryosphere

General remarks: The study provides estimations of glacier mass balance of two large glacierized regions: High Mountain Asia and the Gulf of Alaska using CryoSat-2 data. Most important is that the authors show the suitability of the radar altimeter to obtain not only information about one period but to obtain information about seasonal height and mass changes for the period 2010-2019. The method is not entirely novel but for

the first time applied over such large areas including smaller mountain glaciers. This is a timely and very relevant work as detailed information about glacier mass changes are important in many aspects. Overall, the manuscript is well written and illustrated. I am not a full expert in processing CryoSat-2 data, but as far as I can judge, the method applied seems sound and also the specific conditions of mountain glaciers are considered to degree possible. However, considering the nature of CryoSat-2 and judging the presented results I am not fully convinced that all results are reliable. As also detailed by the other reviewer there are many error sources and sources of uncertainties which should be better considered. I highlight below first the more general comments and provide then more detailed ones.

General comments:

1. Error/Uncertainty sources:

a. The authors subtract the TanDEM-X 90m DEM from Cryosat-2 swath elevation measurements. The state in their manuscript: "The remaining elevation differences are due to time-dependent elevation change that can be related to glacier thickness change as well as errors in the two data sets, temporal heterogeneity and differences in penetration between the reference DEM and the swath elevation measurements." Both utilised data are microwave data. Although the KU and X-band penetration is lower than the penetration of larger wavelengths such as the often used SRTM-C band data, it is not negligible especially in dry snow which is common in many parts of HMA. Moreover, the TanDEM-X DEM is composed of different acquisitions of different seasons and years.

b. Density conversion and snow accumulation: The author's apply a constant value of 850 kg/m^3. This value is often applied also in other studies but needs to be applied with caution. First of all Huss (2013) states that the conversion factor can be significant different for short periods. This is especially important as height changes of snow which has a much lower density can be large. Hence, the authors needs to consider

these density variation more carefully especially when interpreting short term changes.

c. These and other sources of uncertainties needs to be better acknowledged. I have the feeling that the provided uncertainty ranges of 0.03 and 0.07 m w.e./a are clearly too low. I suggest showing the accuracy of the data and results at few selected test sites with independent data (e.g. the HMA DEM, Shean et al. 2017, ICESat-2 data, detailed comparison to high resolution DEMs or other studies for smaller regions and also in-situ measurements, e.g. as available from WGMS).

2. Small glaciers and data coverage: The authors state that CryoSat-2 are also able to survey (very) small glaciers, but do not clearly state a size threshold what they consider as small. There are many very small glaciers especially in HMA which can have significant impact on the overall and especially specific mass changes. Please define “small” and show the portion of the size classes covered in comparison the glacier inventory. Moreover, be more specific about the data coverage and the representativeness and show a plot of the data coverage in relation the total area.

3. There is no mention of impact of glacier surges and avalanche-fed glaciers which are common in parts of the study regions. The validity of the data for these glacier types should be in an ideal case shown, but at least discussed.

4. The authors exclude the endorheic basis when considering the contribution to sea level rise. Here, the authors need to be more specific: Basins where part of the glacier melt as led to lake level rise (e.g. Neckel et al., 2014) this is suitable, but for the others the water (if not sored in the ground) would end up in the hydrological cycle and ultimately in the oceans.

5. Climatic consideration: The authors explain some of the variation by accumulation type and changes in weather and climate. While I agree that this is in principle true the relation to the weather and climate is too simplified. E.g. there are regions in the Tien Shan which receive more accumulation during summer and winter snowfall is also of high importance for parts central Himalaya. Please be here more specific. I suggest to

consider more references (e.g. Maussion et al. 2014) and consider Sakai and Fujita (2017) more in detail.

6. Sections on main results: Sections 3.2 and 3.3: are important sections as the main results are shown. However, these are really short and lack details. You have much more to show. Highlight here all the important results including the shorter term trends and seasonal variability.

7. Discussion sections: Put more emphasis on possible reasons of mismatches to other studies.

8. HIMAP-regions: In order to be able to better compare the regional results to other studies, I suggest to include also a comparison the subregions presented by the cryosphere chapter of the HIMAP report (Bolch et al. 2019) at least in a figure and table in the supplement. These regions were defined by an international team including local scientists and are often used (e.g. Shean et al. 2020, Rounce et al., 2020). Moreover, these regions capture better the regional variability (e.g. mass balances in East Pamir which are more positive and those in central and west Pamir which are more negative). However, I do not want to force you as I am one of the lead authors of this HIMAP chapter and leave this decision to you/the editor.

Specific comments:

Title: The title does not fully reflect the content. One of the strengths of the study is that is shows not only one period but annual and the seasonal variability.

Abstract: It is good to keep the abstract short. However, it would benefit if the variations of glacier mass/elevation change found are better highlighted (also in quantitative way).

L23: Write consistently “Glaciers and ice caps” and include also the percentage area of the glaciers as the area matters more when considering ice melt.

L26: I suggest to cite Immerzeel et al. (2020) instead of (2010).

L40: Include here also Shean et al. (2020)

L43-45: I suggest to include lines 25f here as it is a repetition apart from the seasonality. And please include also a suitable reference for food security and GLOFs.

L65ff: I suggest to introduce the subheading “Study regions” here.

L66: write “... includes the Himalaya, Tibetan mountain ranges, the Pamir and Tien Shan” (Shan=Mountain).

L67: write “about 95,500 (or 96,000 or even “almost 100,000”) glaciers”. Glacier number is a bit arbitrary and depends on the size threshold used and how you split contiguous ice masses.

L79: not only since the satellite records but also before. You may then cite here Bolch et al. (2012) which summarised the info for the Himalaya. Please also consider a reference for the Tien Shan which was not covered by the HIMAP report.

L83: See my comment on the glacier number above

L97: See my comment on L79. This applies also here.

L167: See my comment above. Be more specific.

L188: Omit “very”. I would not anymore consider ASTER as “very-high resolution”.

L206: The Karakoram anomaly was first introduced by Hewitt (2005) and then confirmed by geodetic measurements by Gardelle et al. (2012). Please cite these two references here.

L207: I am surprised to read about the moderate thinning for Lahaul-Spiti. In line with mass balance measurements and modelling of Chhota Shigri glacier (e.g. Azam et al. 2014), Mukherjee et al. (2018), showed significant mass loss in this region using geodetic data. However maybe mass loss was less after 2010? Please be more specific and quantitative and discuss in the discussion section.

L210ff: Be more specific about the own results here (and double check your results especially considering the uncertainty) and move the critical comparison to other studies to the discussion.

L213ff: Similar in this section: Be more specific about the own results and move the critical comparison to other studies to the discussion.

L224ff.: This section contains several interesting findings which I suggest presenting in the results section (e.g. the variability in mass changes for the different regions) and keep here, but extend the climatic discussion.

L249: This is again an important finding and should be move to the results section and the reasoning discussed here.

L264: Please avoid the term doubling or almost doubling. There is a clear increase of mass loss, but uncertainty ranges in this studies are (realistically calculated) and large. Consider here also the Study by King et al. (2019) who used similar data for the Himalaya but found less increase.

L296: “Widely discussed and predicted...” please provide evidence for that.

L385: Avoid citations in the conclusions (especially when too prominently referring to own work). Move this to the discussion for more details but keep the main statement here.

L305ff: I am not too familiar with all the work from Alaska, but I ask you to be more specific regarding the work you are considering. Some have clear different time periods of analysis. As mentioned above discuss in more detail possible reasons to different results.

Some remarks on figure 2: The overall pattern of mass changes makes sense and fits to the current knowledge. There are, however, certain 100x100 grids where the mass balance does not fit. This is especially the case for the central part of Northern Tien Shan in Kazakhstan. There was a positive balance according to your data,

but both geodetic data and in-situ measurements of Tuyuksu glacier clearly highlight negative mass balance Kapitsa et al. (2020) and WGMS data. As already mentioned above Lahaul-Spiti has also more negative values in other studies. You may compare to Chhota Shigri glacier which was shown to be representative for the region. In contrast Eastern Pamir and Altun Shan seems more negative than suggested by other studies. This highlights again that a more careful uncertainty analysis and comparison to existing data and studies are needed.

References cited in my review but not in the manuscript:

Azam, M.F., Wagnon, P., Vincent, C., Ramanathan, A., Linda, A., Singh, V.B., 2014. Reconstruction of the annual mass balance of Chhota Shigri glacier, Western Himalaya, India, since 1969. Ann. Glaciol. 55 (66), 69–80. https://doi.org/10.3189/2014AoG66A104.

Hewitt, K., 2005. The Karakoram Anomaly? Glacier expansion and the "Elevation Effect" Karakoram Himalaya. Mount. Res. Dev. 25 (4), 332–340.

Immerzeel et al., 2020. Importance and vulnerability of the world's water towers. Nature 577 (7790), 364–369. https://doi.org/10.1038/s41586-019-1822-y.

Kapitsa, V., Shahgedanova, M., Severskiy, I., Kasatkin, N., White, K., Usmanova, Z., 2020. Assessment of Changes in Mass Balance of the Tuyuksu Group of Glaciers, Northern Tien Shan, Between 1958 and 2016 Using Ground-Based Observations and Pléiades Satellite Imagery. Frontiers in Earth Science 8, 259. https://doi.org/10.3389/feart.2020.00259.

King, O., Bhattacharya, A., Bhambri, R., Bolch, T., 2019. Glacial lakes exacerbate Himalayan glacier mass loss. Scientific Reports 9 (1), 18145. https://doi.org/10.1038/s41598-019-53733-x.

Maussion, F., Scherer, D., Mölg, T., Collier, E., Curio, J., Finkelnburg, R., 2014. Precipitation seasonality and variability over the Tibetan Plateau as resolved by the High Asia

Reanalysis. J. Climate 27, 1910–1927. https://doi.org/10.1175/JCLI-D-13-00282.1.

Mukherjee, K., Bhattacharya, A., Pieczonka, T., Ghosh, S., Bolch, T., 2018. Glacier mass budget and climate reanalysis data indicate a climatic shift around 2000 in Lahaul-Spiti, western Himalaya. Climatic Change 148 (1), 219–233. https://doi.org/10.1007/s10584-018-2185-3.

Shean, D.E., Bhushan, S., Montesano, P., Rounce, D.R., Arendt, A., Osmanoglu, B., 2020. A systematic, regional assessment of High Mountain Asia glacier mass balance. Frontiers in Earth Science 7, 363. https://doi.org/10.3389/feart.2019.00363.

Shean, D.E., 2017. High Mountain Asia 8-meter DEM mosaics derived from optical imagery, version 1. https://doi.org/10.5067/KXOVQ9L172S2.

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