Review of ‘Ice loss in High Mountain Asia and the Gulf of Alaska observed by CryoSat-2 swath altimetry between 2010 and 2019’ by L. Jakob et al.

Summary:

In this paper the authors have used a recent but well-established technique to quantify the glacial ice loss in two relatively large regions; ‘High Mountain Asia’ (HMA), including the Himalayas, and the Gulf of Alaska (GoA). The technique uses ‘swath processing’ of the Doppler beam-sharpened and interferometric altimeter data from CryoSat to measure the ice loss between commissioning (fall 2010) and the end of 2019.

General Comments:

Both areas include small and large mountain glaciers that are particularly challenging for any form of radar altimetry, and the authors are to be commended for both attempting this job and for the credible results they are presenting. Measuring recent mass loss in the HMA area and relating that to the changing weather conditions over the last nine years is important as so many people depend on and are affected by summer run-off.

Also, I think the authors can be commended for the comparison of their results with those of others who have used different techniques to estimate the change in mass balance of the glaciers in these regions. Figure 8 highlights the fact that some of the different approaches produce different results and, when the associated error estimates are considered, there are inconsistencies. I do not expect the authors to reconcile these problems, but I do want them to avoid the trap that some authors must have fallen into, i.e. underestimating their errors. While the random noise component can be estimated often there are other potential bias errors which can creep into the results. As these are hard to quantify, they are often dismissed or ignored. More on this in the detailed comments below.

For some of the areas the modulation in seasonal height change appears to be related to the magnitude of the snow accumulation as well as the summer melt. This is a consequence of the large number of height estimates possible with swath-processing and the temporal sampling in any one area. However, there is little discussion of this apparent capability in the paper, I think this is another area that could be explored further in this paper.

Specific Comments:

L9. ‘… *the largest non-steric contributor*’. I do not think the term ‘non-steric’ is appropriate here. If you mean the largest contributor after ocean thermal expansion, then use this more straight forward wording.

L25. I think reference to Bamber et al., 2018. (Env. Res. Letters) is more appropriate here than the Shepherd et al., 2020 reference. The later focusses on Greenland, not the non-ice sheet glaciers and ice caps while the Bamber reference includes discussion of change in both the large ice sheets, glaciers and ice caps.

L34. This paper will have wide readership and I think some of the technical terms should be explained, the term ‘geodetic remote sensing methods’ is used here and could (should?) be explained.

L39-L41. ICESat-2 (launched 2018) data is being, and will be, used to monitor change in the height of glacial ice worldwide. I think the combination of data from CryoSat-2 (CS-2) and ICESat-2 will lead to a better system than CryoSat alone. (ref. Smith et al. Science 2020 for ICESat-2 monitoring of glacial ice).

L100-L118. In the introduction the two limiting factors for the use of satellite radar altimeters over mountain glaciers are itemized… namely for CryoSat the limited 240 m range window and the closed loop on-board tracking used to position the start of the range window in fast time. But in the Data and Methods section there is no explicit explanation as to how these limitations are addressed or overcome with the methodology used in tis work.

L104-L106. The two sentences… '*SIRAL is a beam-forming… ground. The sensor emits… the beam.*' either need more explanation or could be left out completely. The approximate diameter of the area beneath the satellite from which returns might be expected is ~15 km, and the diameter of the first return footprint (POCA) from a flat Earth is ~ 1.5 km based on just the pulse bandwidth. It would be very rare indeed that a flat Earth model could be used in the areas studied here.

L112. '*Single*' should be 'signal', or 'return' power.

L122. I suggest… 'The distribution of height measurements departs…'

L123. The sentence 'Given the… domain' is poorly worded, try to simplify.

L125. In correcting for the difference between the glacier hypsometry in a 100 x 100 km bin and the height distribution of CS-2 swath measurements your methodology appears to discard legitimate measurements in those elevation bands which are over-populated in relation to the glacier hypsometry (Fig. S4). In summing the height change to get volume change why not simply scale the CS-2 results in the various elevation bands to match the hypsometry. Would this not be simpler and avoid discarding results?

L157. I am not sure that you can claim the temporal variation in CS-2 height change will always match the surface height change in either area. Looking at your seasonal CS-2 height change curves (Fig. 5) for some of the HMA there appears to be significant winter snow accumulation (5 – 10 m in some of your areas!) so I would suspect that conditions (the nature and density of the surface snow, and therefore reflectivity) will change between the winter and after the summer melt, and the 'penetration' or the effective surface seen by the radar altimeter will change with season and possibly elevation. For example, recent work on the high accumulation region in SE Greenland appears to show a seasonal change in the bias between the surface and CS-2 detected 'height' (Gray et al., Front. Earth Sci., https://doi.org/10.3389/feart.2019.00146). However, if you assume that conditions do not change significantly fall-to-fall then the year-to-year volume and mass change can still be estimated. The shape of the seasonal height change may be affected by a varying bias between the surface and the detected CS height. While I think your results are credible, I suspect that a demonstration that CS-2 seasonal height change matches surface height change in these areas will require a comparison of coincident CS-2 and ICESat-2 results.

L167. I would explain the term 'endorheic' or use the phrase 'closed or endorheic basins'.

L183. I am not sure how the percentage coverage of the glaciated regions in the two areas was estimated; the swath processed CS footprint area is ~ 380 m along-track times a figure dependent on the cross-track slope and the waveform smoothing. For example, if the cross-track slope is 0.5 degrees then the footprint in this direction without smoothing is only 27 m. With waveform smoothing and volume backscatter this will be broadened, but it is still a fraction of the typical POCA footprint for flat terrain. Considering the early CryoSat results by Amaury Dehecq over the Himalayas, I have to say that the percentage of the glaciated areas you have covered (~ 50%?) is remarkable. Is this correct?

L185. Spatial coverage and elevation sampling… The relatively poor coverage of the lower reaches of the HMA and GoA glaciers is a concern. These are the normally the areas that are most vulnerable to rising temperatures and which often change the most.

L187. 'Whilst'

L192. '*Spatial coverage and number of points show a different relationship with hypsometry, which is due to the overlap between adjacent CryoSat footprints.'…* I don't understand this sentence, in the along-track direction the footprint is ~ 380 m and the sampling is ~ 300 m while in the cross-track direction there is oversampling if all the waveform points are retained after the waveform smoothing stage. In fact, if the filter smooths over 3 samples (from the Gourmelen reference) then you could use every third sample in the waveform.

L197. You have acknowledged the problem associated with the onboard tracking and the limited 240 m waveform window but the fact that most (?) of the glaciers you are studying will have termini at an elevation beyond the end of the measured waveform remains perhaps the most important limitation of your study. The implication that the small difference between the 'biased' and 'non-biased' estimates of the specific mass losses somehow justifies your results for mass loss is weak. A 'stable result' is not necessarily a precise one.

L209. '*In contrast with other studies (Brun et al., 2017; Shean et al., 2020) we find a heterogenous pattern in the Tibetan Plateau and Eastern Kunlun, with some scattered glaciers displaying higher mass losses.'…* Can you think why this would be the case?

L223. 'Temporal variability'. Looking at Figures S1, S2 and 5, I see the upward trend in elevation change after ~ 2015 for the Karakorum region but I am not so sure about the statement … '*This shift of thinning rates post-2015 is also clearly seen in Bhutan/East Himalaya, Kunlun (West and East), Tien Shan, Pamir Alay/Hissar Alay and Nyainqêntanglha/Hengduan 230 Shan (Figure 5, S1, S2)'.* What is clear is that the seasonal modulation is increasing for many of these areas. I admit that I have not studied glaciologic change in these areas previously, but can you have a 10 m height change for the glaciers of Pamir Allay between the summer of 2017 and the following winter? Even the 'Full region' plot, top left Figs. 5 and S2, shows the increasing seasonal modulation as well as the slow height decrease. Is this significant?

Looking at the elevation change rates in the HMA (Fig. S6) it seems that your study (Fig. S6a) produces a noisier elevation change rate vs normalized elevation than the Brun et al. study (Fig. S6b). Is this assessment fair? And can you rationalize this behavior?

Also, in Fig. 6, the variation in height change rate and the 100 m bin glacier hypsometry are plotted against elevation for the various regions. The elevation change-rates generally become less negative with increasing elevation but some of the curves are quite noisy and have what I would consider as suspicious jumps that are larger than the uncertainty shading. Can you provide some explanation for these variations?

L250 and onward… comparison of mass balance estimates. I will leave detailed comments on the difficulty of reconciling the various mass balance studies to others with a better knowledge of these geographic areas. The problem as I see it is that some of the studies appear to underestimate their potential errors. For the swath processing approach used in this study there are several potential issues which may lead to bias errors. While I acknowledge the careful approach used to try to eliminate the poor height values, I think the quoted errors may still not reflect all the potential problems that could lead to bias errors… For example:

1. As the orbit is essentially north-south for the HMA, is there any possible bias between the height change results dependent on glacier orientation (NS vs EW)? Along-track slopes (> ~ 1°) are not good news for the delay-Doppler or subsequent algorithms.
2. The swath-processed footprint is rarely contiguous, hopefully one area (on the glacier!) dominates the returns so that the phase can be used to geocode the footprint. Remember that with delay-Doppler processing the geocoding is done based on the differential phase which will be corrupted when multiple areas contribute to the range sample. The hope is that one area of the composite footprint dominates the return.
3. The seasonal variation in height change can reflect changing surface conditions as well as accumulation/melt. You cannot assume that the conditions in your areas are comparable to those studied in the papers you reference.
4. With swath-processing, compensation for the low percentage of results from the lower glacier elevations must be difficult. Looking at, e.g. Fig 11, the low elevation height change rate is quite variable, some of the areas show an increase in height loss with elevation when surely one would expect smoothly increasing loss with decreasing elevation?

In summary, while these results are on the one hand both impressive, important and well-illustrated, it is important that all the possible errors and biases are at least acknowledged. The 'uncertainty envelopes' used in the figures reflect the quantifiable errors in the methodology. While some of the potential bias errors are very difficult to quantify that does not excuse ignoring them. I would like the authors to at least acknowledge that there are other potential bias errors that could expand the 'uncertainty envelopes'.