Response to comments of reviewer 1 to:

Spatially and temporally resolved ice loss in High Mountain Asia and the Gulf of Alaska observed by CryoSat-2 swath altimetry between 2010 and 2019

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.

Main 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

Answer: This will be answered in the specific 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.

Answer: Thank you for this valuable input. We have expanded this aspect in the following sections (for more details refer to the specific comments below):

Time series of surface elevation changes:

“Although time-series are generally reflecting the actual change in surface elevation, there are a number of limitations that are important to keep in mind when interpreting the results from radar altimetry. For the reasons stated above, scattering properties can induce elevation biases at seasonal time-scale (Gray et al., 2019). In addition, integrating changes over very large regions can lead to spatial heterogeneity in the successive time steps, in particular when the data volume becomes too low. These limitations may explain some of the observed patterns, and in particular the few cases where seasonal variability is larger than what is expected from our knowledge of SMB in the regions.”

Mass balance and contribution to sea level rise:
“To obtain volume changes we use the glacierised area of the Randolph Glacier Inventory (RGI 6.0) (RGI Consortium, 2017). We assume the standard bulk density of 850 kg/m^3 (Huss, 2013) to convert volume changes to equivalent mass changes. This assumption is considered appropriate for a wide range of conditions and longer-term trends, however, this factor can differ significantly for shorter term periods (<3 years) (Huss, 2013).”

Conclusion:
“This is the first study to demonstrate the ability of interferometric radar altimetry to monitor large-scale change in thickness, mass and sea-level contribution of glaciers across regions of extreme topography. This, along with recent work in the Arctic and Patagonia demonstrates the potential of such a system to monitor trends in ice mass on a global scale and with increased temporal resolution. **It also demonstrates the ability to monitor monthly change and paves the way to an observation-based quantification of seasonal accumulation and melting processes, a task that will likely require combination with regional climate models, and with other sensors such as IceSat-2 and high-resolution DEMs.**”

Main 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.

Answer: We have rephrased accordingly:
“Glaciers are currently **the largest contributor to sea level rise after ocean thermal expansion**, contributing ~30% to sea level budget.”

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.

Answer: We cited Shepherd et al. (2020) to highlight that glaciers (Gardner et al., 2013; Wouters et al., 2019; Zemp et al., 2019) contribute more than Greenland (Shepherd et al., 2020). We rephrase this section as follows:

“Glaciers store less than 1% of the mass (Farinotti et al., 2019) and occupy just over 4% of the area (RGI Consortium, 2017) of global land ice, however their rapid rate of mass loss accounts for almost a third of the global sea level rise, the largest sea level rise (SLR) contribution from land-ice (Bamber et al., 2018; Gardner et al., 2013; Slater et al., 2020; Wouters et al., 2019; Zemp et al., 2019).”

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.

Answer: We revised accordingly:
“The traditional approach (glaciological method) extrapolates in situ observations (Bolch et al., 2012; Cogley, 2011; Yao et al., 2012; Zemp et al., 2019), however measurements are sparse and possibly biased towards better accessible glaciers located at lower altitudes (Fujita and Nuimura, 2011; Gardner et al., 2013; Wagnon et al., 2013). **In contrast, geodetic remote sensing methods rely on comparisons of topographic data or gravity fields to determine glacier changes. Recent geodetic remote sensing methods include** (1) Digital Elevation Model (DEM) differencing (Berthier et al., 2010; Brun et al., 2017; Gardelle et al., 2013; Maurer et al., 2019; Shean et al., 2020), (2) satellite laser altimetry (Kääb et al., 2012, 2015; Neckel et al., 2014; Treichler et al., 2019) and (3) Gravity Recovery and Climate experiment (GRACE) satellite gravimetry (Ciracì et al., 2020; Gardner et al., 2013; Jacob et al., 2012; Luthcke et al., 2008; Wouters et al., 2019).”

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).

Answer: We fully agree with the reviewer.

In the conclusion section:
“This, along with recent work in the Arctic and Patagonia demonstrates the potential of such a system to monitor trends in ice mass on a global scale and with increased temporal resolution. **It also demonstrates the ability to monitor monthly change and paves the way to an observation-based quantification of seasonal accumulation and melting processes, a task that will likely require combination with regional climate models, and with other sensors such as IceSat-2 and high-resolution DEMS.”**

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 this work.

Answer:

The short answer is that they are not. If the onboard tracking positions the range window outside the range of elevation at which glaciers are found by more than ~half the range window size, there will be no data to exploit over glaciers in any of the recorded echoes. However the wording of this section in the introduction is ambiguous, in addition, Swath will help sample a larger range of elevation within the 240m range window and so we edited the text as follows:

In the introduction:
"**Over regions of more extreme surface topography however, such as those found in mountain glacier areas, the use of radar altimetry has been prohibited by the large pulse-limited footprint, a limited range window (240 m for CryoSat), and closed-loop onboard tracking used to position the altimeter's range window (Dehecq et al., 2013). Despite these limitations, CryoSat's sharper footprint and interferometric capabilities have led to promising studies over mountain glaciers (Dehecq et al., 2013; Foresta et al., 2018; Trantow and Herzfeld, 2016)**"

In the data and method:
" In contrast, swath altimetry exploits the full radar waveform to map a dense swath (~5 km wide) of elevation measurements across the satellite ground track beyond POCA (Foresta et al., 2016, 2018; Gourmelen et al., 2018; Gray et al., 2013; Hawley et al., 2009) providing one to two orders of magnitude more elevation measurements compared with POCA **and improving the sampling of topographic lows (Foresta at al., 2016)**. This makes the CryoSat-2 sensor at present the only radar altimeter able to survey small glaciers at high resolution."

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.

Answer: We revised accordingly:

"SIRAL is a beam-forming active microwave radar altimeter with a maximum imaging range of ~15 km on the ground. The sensor emits time-limited Ku-band pulses aimed at reducing the footprint to ~1.6 km within the beam. Over land-ice, the sensor operates in synthetic aperture interferometric (SARIn) mode, which allows delay-Doppler processing to generate an along-track footprint of ~380 m, while cross-track interferometry is used to extract key information about the position of the footprint centre. In practice however, footprint size will vary depending on properties such as surface slopes, scattering properties, and distance from the Point-of-Closest-Approach (POCA)."

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

Answer: We have changed this to "received power".

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

Answer: We have changed this to: "**The distribution of elevation measurements with altitude** departs somewhat from the glaciers' hypsometry".

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

Answer: We have revised accordingly:
"**Hypsometric representativeness of measurements within spatial units is a key requirement for robust glacier trend estimates. A bias in the altitudinal distribution of observations can lead to a bias in the total rate of thinning when integrated over a larger domain, as rate of thickness change is often strongly correlated with altitude**."

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?

Answer: We have initially tested the scaling approach too, and concluded that we achieve better results with the discard method we currently use. We found that the scaling approach can give high weighting to very few measurement points in sparse areas, which can lead to a reduction of the quality of results. It is also important to note that our current method rates the quality of each measurement and discards the lowest quality elevation measurements first, which can increase the stability of results in dense areas.

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.

We agree with the reviewer, our wording was ambiguous. We rephrase as follow:

"It is a well known observation that microwave pulses scatter from the surface as well as the subsurface, which can lead to elevation change bias in regions of historically anomalous melt event (Nilsson et al., 2015); **or at seasonal time scale (Gray et al., 2019)**. Over most regions however, it has been shown that surface elevation change from CryoSat **over**

annual and pluri-annual time scale are consistent with in-situ, airborne, and meteorological observations (Gourmelen et al., 2018; Gray et al., 2015**, 2019**; McMillan et al., 2014a; Zheng et al., 2018)"

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

Answer: We have changed 'endorheic' to 'closed or endorheic'

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?

Answer: To calculate coverage, we assume an area the size of the pulse limited footprint at POCA for flat terrain i.e. ~380m by 1.5km. However the reviewer is correct that the real footprint will be dependent on many factors, several of which are not fully controlled. As the reviewer rightly points out, data filtering which will tend to coarsen across track resolution, surface slope will decrease across track footprint size, scattering process, e.g. a certain amount of volume scattering, will increase the footprint size for a given surface slope, the footprint size will also vary with distance from POCA. We have rephrased this section as follows:

"Using the **theoretical** pulse-limited footprint size of CryoSat-2, we **derive** a total spatial coverage of glaciated regions of 55% in the GoA and 32% in HMA respectively."

We cover 32% of the glaciated area, 50% is just an estimate of the potential assuming that onboard tracking was not an issue. We rephrase that sentence as follows:

"Given that it is estimated that 40% of HMA glaciers are not sampled due to onboard tracking limitations (Dehecq et al., 2013), we estimate that with an appropriate onboard tracking system, the rate coverage for HMA would be as high as 50%."

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.

Answer: A comparison of the glacier hypsometry and the spatial coverage of our data (Figure S4) shows that we still achieve good coverage at low elevations in both regions. In addition, we interpolate missing data based on the relationship between elevation and elevation changes. We therefore still capture the changes in the lower reaches of the HMA and GoA glaciers.

We have revised part of the spatial coverage and elevation sampling section accordingly:

“We observe a bias of the total number of swath measurements towards higher altitudes (e.g. Figure S5), which can be attributed to the onboard tracking tending to favour elevations closest to the satellite. However, a comparison of the glacier hypsometry and the spatial coverage of our data (Figure S4) shows that we still achieve good coverage at low elevations in both regions. In addition, we interpolate missing data based on the relationship between elevation and elevation changes and therefore still capture the changes in the lower reaches of the HMA and GoA glaciers.”

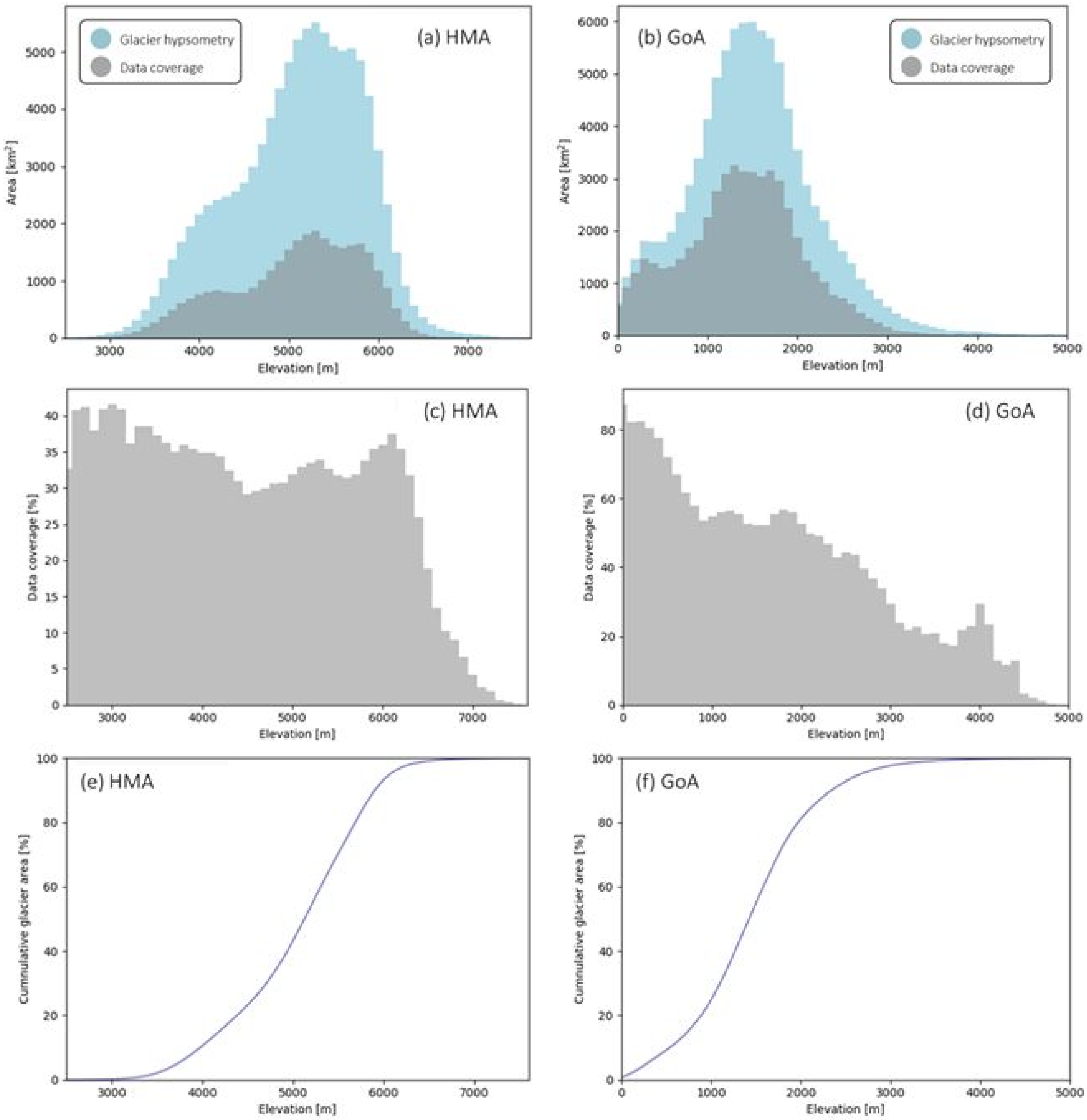

Figure S4: Figure (a) and (b) display glacier hypsometry (light blue) and swath elevation data coverage (grey) for the two regions High Mountain Asia (a) and the Gulf of Alaska (b). Figure (c) and (d) display data coverage as a percentage of the glacier hypsometry and figure (e) and (f) display the cumulative glacier area.

L187. ‘Whilst’

Answer: We have changed this.

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.

Answer: We agree that in theory, successive samples should be independent, but for the reason given above e.g. in particular with respect to volume scattering, and our approach to modeling the coverage i.e. using true sample location and fixed footprint size, this leads to overlap in our modelled footprints.

We rephrased as follows:

“Using the theoretical pulse-limited footprint size of CryoSat-2, we derive a total spatial coverage of glaciated regions of 55% in the GoA and 32% in HMA respectively. These values are the combined result of the absence of recorded returns due to orbit separation and onboard-tracking limitation (Dehecq et al., 2013), and of data quality. Given that it is estimated that 40% of HMA glaciers are not sampled due to onboard tracking limitations (Dehecq et al., 2013) we estimate that with an appropriate onboard tracking system, the rate coverage for HMA would be as high as 50%. These values are within the high-end of the range of observational methods (Zemp et al., 2019), whilst generally lower than the coverage provided by high resolution sensors (Brun et al., 2017; Shean et al., 2020). As expected from the relatively large footprint of radar altimeters, we do observe a positive correlation between spatial coverage and glacier size, we do however observe coverage over all glacier sizes (Figure S6). **We observe a bias of the total number of swath measurements towards higher altitudes (e.g. Figure S5), which can be attributed to the onboard tracking tending to favour elevations closest to the satellite. However, a comparison of the glacier hypsometry and the spatial coverage of our data (Figure S4) shows that we still achieve good coverage at low elevations in both regions.”**

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.

Answer: As discussed above, we have measurements covering all elevation ranges on glaciers in both regions, including glacier termini (Figure S4). Also refer to answer of L185.

We have revised part of the spatial coverage and elevation sampling section accordingly:

“**We observe a bias of the total number of swath measurements towards higher altitudes (e.g. Figure S5), which can be attributed to the onboard tracking tending to favour elevations closest to the satellite. However, a comparison of the glacier hypsometry and the spatial coverage of our data (Figure S4) shows that we still achieve good coverage at low elevations in both regions. In addition, we interpolate missing data based on the relationship between elevation and elevation changes and therefore still capture the changes in the lower reaches of the HMA and GoA glaciers.**”

L209. ‘In contrast with other studies (Brun et al., 2017; Shean et al., 2020) we find a heterogeneous 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?

We have made adaptations in the elevation change rate calculation, integrating the stability of our regression results to identify results that are particularly sensitive to data sampling, data distribution and data weighting. This method adaptation is described in more detail in the response to reviewer #2.
The updated results display a more homogeneous pattern in the Tibetan Plateau, Eastern Kunlun and in Nyainqêntanglha/Hengduan Shan, which is more comparable to other studies (Brun et al., 2017, Shean et al., 2020). We have therefore removed this statement.

L223. ‘Temporal variability’. Looking at Figures S1, S2 and 5, I see the upward trend in elevation change after ~ 2015 for the Karakoram 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?

Answer: The reviewer is correct that changes described above are subtle and seasonal variability relatively large. We therefore removed mention of these later regions. It is not clear to us why seasonal amplitude increases for some regions over time, it is not necessarily systematic however, neither at the scale of the HMA regions, nor when looking at the GoA region. As far as the overall HMA time-series is concerned, apart in 2018, the seasonality is relatively constant. We tend to observe a lower data density when these larger values occur and so these could be the result of a decrease in quality of spatial sampling during specific time-periods.

We have added content in the time-series sub-section of the method section to mention sources of uncertainty in the current time-series calculation:
“Although time-series are generally reflecting the actual change in surface elevation, there are a number of limitations that are important to keep in mind when interpreting the results from radar altimetry. For the reasons stated above, scattering properties can induce elevation biases at seasonal time-scale (Gray et al., 2019). In addition, integrating changes over very large regions can lead to spatial heterogeneity in the

successive time steps, in particular when the data volume becomes too low. These limitations may explain some of the observed patterns, and in particular the few cases where seasonal variability is larger than what is expected from our knowledge of SMB in the regions.”

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?

Answer: The study Brun et al. (2017) – and other DEM differencing studies such as Shean et al. (2020) – resolve elevation changes at much higher spatial resolution than we are currently able to generate with CryoSat-2, leading to smoother curves. However, the plot demonstrates that despite the challenges we face with radar altimetry in the complex terrain of High Mountain Asia we are still able to resolve the relationship between hypsometry and elevation changes which is comparable to results of other studies.

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?

Answer: We do observe a general correlation between uncertainty and variability of the hypsometric curves, although it is true that the magnitude of the envelope is often narrower - this touches upon other such comments in the review about uncertainty. What we do observe is that these regions of larger variability occur for sectors with lower glacier area. We make a note of this in the manuscript:

“**While some variability exists along the profiles, in particular over regions and elevation ranges containing fewer glaciers that can reflect a less robust solution and or spatial variability in glacier response, trends between elevation and ice thickness change are clearly visible.**”

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 delayDoppler 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?

Answer:

We agree in general with the reviewer, we respect to the specific made:

1.The reviewer is correct about the importance of aspect and slopes. In the three graphs below we attempt to quantify the impact of both on data accuracy and distribution. The first plot (Figure S10) shows the comparison between swath elevation and the reference DEM. We do indeed observe an increase in the dispersion of the elevation difference with increasing along and across track slopes, while the bias remains relatively similar. Note that the dispersion will be accounted for in our current error method. The other impact of high along track slopes will be in the ability of the onboard tracker to keep track of the surface, with elevated along-track slopes the onboard tracker will be more likely to “lose lock” and this will be reflected in the data loss discussed in Dehecq et al., (2013) - this is a strong reason to develop a more suited onboard tracker for future radar altimetry missions. We added a mention of this in the manuscript, in the new error section discussed at the end of this specific response. We do not observe significant differences between the distribution of swath elevation versus aspect and the distribution of glacier aspect as seen in the last graph below (Figure S11).

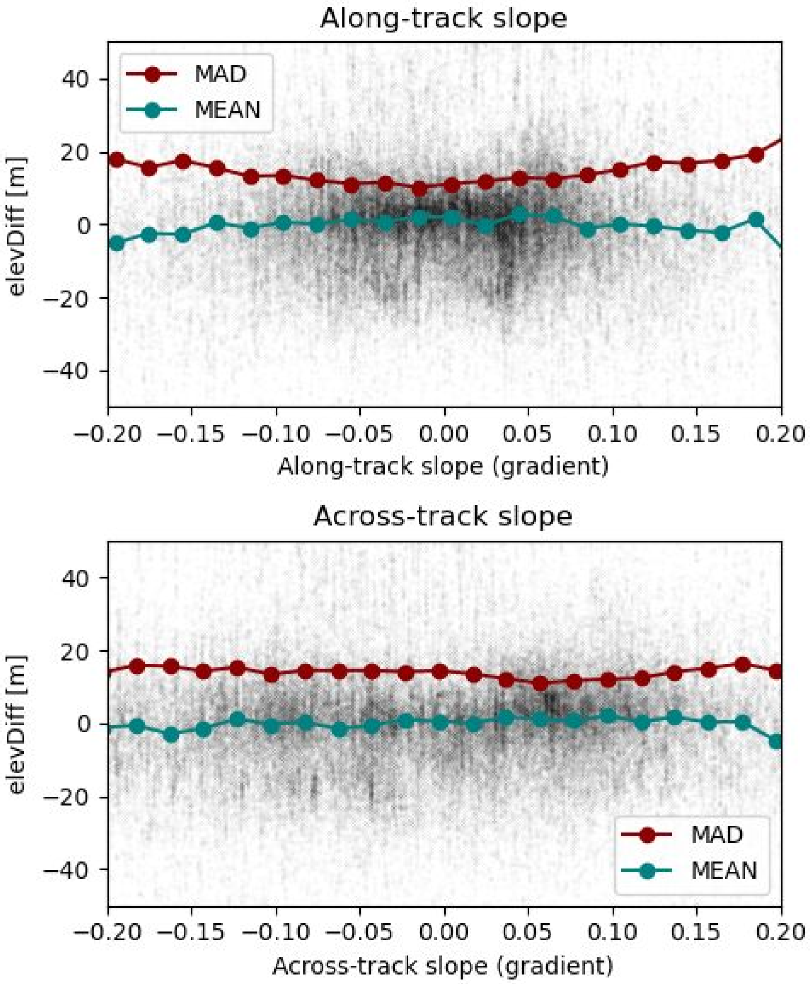

Figure S10: Differences between the TanDEM-X 90m DEM (German Aerospace Center [DLR], 2018) and the swath elevation measurements in a study area in High Mountain Asia (HMA) for different along-track and across-track slopes, including median average deviation (MAD) and mean (MEAN) of the elevation differences (referred to as *elevDiff*).

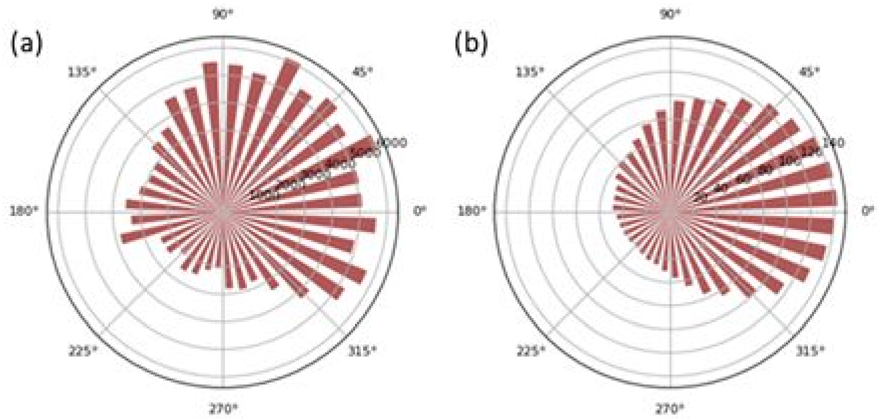

Figure S11: Distribution of aspect in our swath elevation measurements (a) and the distribution of glacier aspects (b) in a study region in High Mountain Asia (HMA).

3. The reviewer is correct, we have made this clear now in the method sections related to rates of elevation change and in the time-series section.

4. Other studies of the Gulf of Alaska have reported a similar relationship to this study (e.g. refer to Berthier et al. (2010)), and attributed this to the impact of debris cover. In addition the use of the static RGI glacier masks can also impact the elevation change profile, by including areas in the profile where glaciers have retreated since the date of the masks.

We mention this in section 4.2.2:
"In the Western Chugach Mountains, Alaska Range and Alaska Peninsula we observe a decrease of thinning rates towards the lowest elevations of these sub-regions, which can be attributed to the effect of debris cover and the temporal evolution of glacier extent during the study period, one of the limitations when using static glacier masks. This characteristic has been observed, although more pronounced and across all sub-regions, by Berthier et al. (2010) and Arendt et al. (2002)."

To cover the various points made above, we added the following paragraph in the method section:

"While our uncertainty methods follow existing approaches and our error bounds are similar in magnitude to Brun et al. (2017), Kääb et al. (2012) and Shean et al. (2020) but lower than GRACE-based estimates, several additional potential sources of errors could impact the results, and methods to assess them, not currently available, should be developed. Radar altimetry has been shown to be sensitive to surface slopes, and in particular to slope in the direction of the satellite's flight path, in regions like HMA and GoA this impact will also be seen in the performance of the onboard tracker as for large slopes the system is expected to "lose lock". It is a well-known observation that microwave pulses scatter from the surface as well as the subsurface, which can lead to elevation change bias in regions of historically anomalous melt events (Nilsson et al., 2015); or at seasonal time-scale (Gray et al., 2019). Over most regions however, it has been shown that surface elevation change from CryoSat over annual and pluri-annual time scale are consistent with in-situ, airborne, and meteorological observations (Gourmelen et al., 2018; Gray et al., 2015, 2019; McMillan et al., 2014a; Zheng et al., 2018). Using static glacier masks can also lead to errors in regions of rapid dynamic changes. In general these limitations are known and efforts are currently underway in the community to improve uncertainty analysis, and develop new glaciers outlines products."

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'.

We appreciate the feedback and agree with the reviewer that more should be done as a community to understand, quantify, and reconcile the several estimates available today. Several community initiatives are underway. While the purpose of this study was not to resolve these issues, we hope that the additional context provided here and in the updated manuscript help make it clear that we do share the reviewer's thoughts.