Calving event size measurements and statistics of Eqip Sermia, Greenland, from terrestrial radar interferometry

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Reply to reviewer's comments

General reply

We thank both reviewers, Surui Xie and Pierre Marie Lefeuvre, for reading through the manuscript and their critical comments and helpful ideas, and suggestions. We appreciate the invested time for the feedback. The reviewer's major concerns mainly referred to the issue of uncertainty in TRI derived elevation models, the ice flux budget estimation and the statistical analysis. We are thankful for these comments and we think we should be able to address those comments in our revisions and that they will considerably improve the manuscript. Here we reply to the more substantial concerns raised by the reviewers and present our ideas for revisions. The main changes we plan are

- a detailed error analysis of the stable terrain between the TRI derived DEMs
- to compare the ArcticDEM with the TRI derived DEMs.
- and a more detailed analysis of the ice flux as comparison to the calculated calving volumes.

Most of the minor comments consider language or detailed content or are specific examples of the major concerns. All these more minor corrections will be addressed in the revised manuscript. We therefore mostly refer to the major comments here. We appreciate the opinion of the editor on the suggested revisions.

Reply to major concerns Referee 1

R1: Uncertainty in TRI derived elevation models needs better assessment. The authors randomly choose 30 DEMs and computed the variability (its definition needs to be provided in the manuscript) as a measure of the precision.
Although the mean variability is 1 m, but the maximum variability is 5 m. Therefore, the DEMs are likely to have an uncertainty of ~1 m to several meters. Although a threshold of 5m elevation decrease between adjacent DEMs is used to determine calving events, but note that even among only 30 DEMs there is a variability of 5 m between two DEMs. The calving statistics of this manuscript come from hundreds of DEMs, several large random errors (2σ or above) or outliers can significantly change the results. I suggest the authors to provide more details on error analysis.

We agree that the uncertainty analysis was presented too vaguely. We will add a detailed section on error analysis, where we will present the variabilities on stable terrain over space and time. The test area on stable terrain will be indicated in Figure 1. This more detailed error analysis does however not substantially change our results and main findings stated in the paper. However, it will strengthen our methods and findings.

R1: Based on the calving data derived from TRI elevations, the authors concluded that surface calving is more frequent in the shallow water sectors, and the sizes are generally larger. This seems apparent if just looking at Figure 5c. However, due to lack of rigorous uncertainty analysis, I think this conclusion is hasty and may be flawed. In general, noise in TRI measurements increases with distance, and can increase rapidly at a distance of 4-6 km. Glacier front on the northwestern section (shallow water sectors in this manuscript) is further from the radar than the southeastern section (deep water sector), thus radar data on the northwestern section of the glacier should be noisier if all other conditions are similar. Besides, the northwestern section of the glacier front is crevassed heavier than the southeastern section (Figure 1), and elevation changes rapidly (inclined at a slope of 50 degrees according to the authors), both are more likely to induce phase unwrapping errors than a flat and less crevassed surface. These (i.e., increased noise with distance, phase unwrapping problems) could be some of the reasons why the identified calving volumes are more variable and the cumulative calving volumes are larger along the SL/SM/SR/M sectors. In Figure 5c, timing and sizes of calving events at different distances look random, but considering the characteristics of radar noise, it is important to examine if the observed pattern is due to noise or unwrapping errors. Here I suggest one possible method to test how much noise affected the distribution pattern in calving events: using the same analysis approach as presented in the manuscript, but apart from calculating calving volume based on pixels whose elevation decreased by >5 m, the authors can also calculate "increased volumes" by pixels whose elevations increased by >5 m. If a similar distribution pattern as in Figure 5c is seen, then the derived "calving volumes" are likely disturbed. The authors can probably add a plot of such "increased volumes" to the negative side of y-axis in Figure 5d (can used light blue color if the authors don't want it be distracting). A comparison figure of "detected increasing volume" similar as Figure 5c can also add important information to the manuscript, and it can go to the supplement if the authors would like to save space in the main manuscript.

We realized that in our paper the correction process to minimize errors with distance is not described. However, we used a correction factor to correct for systematic error sources. Those error sources can be caused by errors in the reference heights and instrumental geometry, baseline errors and errors caused by a not perfectly vertical mounting of the three antennas (Strozzi et al., 2012). We did that by comparing the calculated DEMs with the Arctic DEM and choosing control points on stable terrain at different distances from the radar. With the used correction factor we can minimize uncertainty in the height estimates. We will add details on computing the correction factor in the revised manuscript.
Considering the geometry of the calving front we agree that the shallow sector is more likely to induce errors than the deep part with the less steep front geometry. We will have a closer look at this by also investigating the uncertainty due to slope differences on stable terrain. For this we include an example of the analysis calculated on stable terrain with different slopes. We thank referee 2 for his suggestion of doing the whole analysis for positive height changes. We will have a look at that and include such a figure in our revised manuscript (most likely in appendix).

R1: According to the authors, there was very little surface calving observed by TRI at sector D (the deep water sector), and mass loss due to subaqueous calving is dominant (50% or more, depends on the rate of oceanic melt) here. Limited evidence of subaqueous calving was shown in the manuscript. Even if substantial subaqueous calving events occurred and contributed significantly to the mass loss at the deep water sector, the manuscript failed to explain where the mass goes. I also think it is not adequate to simply assume that subaqueous calving is independent of TRI observed surface calving. If subaqueous calving would not cause surface elevation change by following the authors' logic, then what was there to fill the space left by the "subaqueous calving"? Besides, if TRI observed little calving at sector D, then glacier front at this section should advance, especially at the high velocity area. Speed in the middle of this sector is 16 m/day, in ~7.65 days, ice front here can advance over 100 meters, much larger than the resolution of either Landsat/Sentinel satellites or TRI images so should be detectable. However, Supplementary Figure S1 rejected this.

We agree that the explanation of the processes happening at the deep sector was not complete. We cannot see subaqueous calving events with the TRI data but we can add an image of a subaqueous calving event from the time-lapse camera installed in 2018. Unfortunately, no time-lapse camera was installed in 2016. As the flow field and surface slope further upstream is homogenous (smooth across-flow profile velocity profile) and thereby does not indicate substantial differences in ice thickness we assume that the ice thickness downstream (towards the front) is similar for the shallow and the deep sector also at the calving front. We argue that if this would not be the case, we would see it in the surface structure of the glacier by specific crevasses or flow velocity variations. Thus, if the ice thickness is similar, at the deep sector about 2/3 of the ice area are below the waterline. The assumption that at least 66 % of the mass loss are underwater is therefore justified. The remaining 10 % might be calved off above the water line through small calving events, which were filtered out during the analysis. Those small events can be caused by undercutting of the calving front due to ocean melt and calving below the waterline. Of course these are only relatively rough estimates which we will clarify in the text. We will rewrite this section and explain this in more detail.

R1: The manuscript did not explain the method they used to choose the study area for calving detection well. Although on page 6 the authors mentioned that they applied a mask with ~150 m wide across the glacier front, however, the glacier front was constantly moving, so a Lagrangian frame should be used. Whatever the reference frame was, according to the methods presented by the authors, areas with calving event detected (Figure 4) over the center of sector D should have the largest along-flow direction width. This is because glacier front at this location should advance (also see comment above), thus the test area should move. Whereas Figure 4 shows a different pattern.

In the revised manuscript we will add a figure showing the used front position mask as well as the front position over the observation period. The front position advanced and retreated only marginally but was always well within the mask. Thus we decided to use a simple constant mask. The fact that the centre of sector D does not have the largest along-flow direction width we explain with many small events, which might be triggered by undercutting of the calving front due to underwater calving and ocean melt. We will rewrite the corresponding section in the revised manuscript.

Reply to major concerns Referee 2

R2: DEM derivation of the glacier front from TRI
The critical part of the paper is the derivation of digital elevation models of the glacier front from terrestrial radar interferometer as developed by Strozzi et al. (2012). However, this method is known to be uncertain, although the large glacier size should help having a greater signal to noise ratio. I think that it is important to extend the paragraph on the error analysis and dedicate a specific figure with a map of the derived DEM(s), statistical distribution of the error in the discussed stable terrain. Assess the uncertainty of the glacier part with the UAV derived DEM too by replacing Figure 3 as the velocity comparison is done in Rohner et al, 2019. I would like to have a Figure showing a study case of the detection and watershed

algorithm to assess issues with signal to noise ratio and uncertainty in radar geometry or cartesian coordinates (in the main text or supplementary material).

We agree with the concern of referee 2 that the error analysis needs more elaboration. We will add a section, where we will investigate the error on stable terrain in more detail (see comments referee 1). We will compare a stacked TRI DEM with the Arctic DEM and investigate the variations between the TRI DEMs itself on the stable terrain over space and time.
A comparison with the UAV derived DEM to investigate the uncertainty of the glacier part is currently not possible as we do not have a georeferenced UAV DEM. To do this comparison we would need to georeference it and do a detailed error analysis, which is not part of this paper. Additionally, as we do not have ground control points on the glacier the UAV DEM might be warped in the centre of the glacier.
Figure 3 is not meant as a velocity comparison, but the UAV velocity is rather a completion of the TRI-derived velocity data to illustrate the flow field. However, we will rephrase the section to make this clearer.
We will add a figure showing a study case of the calving event detection before section 4.1. With this we will show how we can reduce errors by stacking and by using thresholds. An example of the detection of a calving event will be shown in the main text while an example of the stable terrain will be included in the supplementary material.

R2: Issues in determining best fit models for calving distribution
As seen on Figure 6c and 6d, it is not possible to distinguish between a power law and a log normal models as indicated by a low loglikelihood ratio R between the two distributions and a poor significance value, $p>0.1$. The only evaluation possible of the power law is a comparison with other heavy-tailed distributions. The conclusion is that the shallow and deep part does not exhibit a transition in distribution from power law to log normal as they cannot be statistically differentiated from each other. Discuss instead whether the distribution over such a short period is representative.

We agree that the significance value is poor and thus it is not really possible to distinguish between a power law and a log normal model. We will rephrase the section to make it clearer that the distinction is not significant but that there might be a difference. We also agree that the period is rather short to have a representative distribution and we will add a discussion section about that. Since we observed a big number of calving events we are convinced that a statistical analysis is meaningful and legitimate.

R2: Ice flux budget: bed topography and missing component
The paper bases its analysis on the depth of the fjord but no bed data is provided to support this description (just observations of surfacing rocks). Please use the BedMachine v3 to at least provide an idea of the fjord depth in front of the glacier to the reader. The shallow part may be constituted of two bed pinning points beside a deep valley. Furthermore, simplify the subdivision of the shallow part to only the shallow part regrouping SL, SM and SR.
The simple ice flux calculation holds some caveats when identifying a missing volume due to the uncertain fluxgates and filtering of small events. The distribution of these small events may be related to calving mechanisms and ocean melting (undercutting). A more realistic flux can be derived by integrating the ice flux with your surface elevation and velocity data. See my minor comments to improve the understanding of section 5.1.

We will add the data of BedMachine V3 to the supplement of the revised manuscript to give an idea about the fjord topography. However, as no direct measurements are available directly

at the calving front, the data of BedMachine V3 at the calving front should be considered with care. At the calving front and close to it the velocity is influenced by additional processes and the estimation of the ice thickness by inferring the surface elevation and velocity data cannot reproduce the bed topography correctly.
We will add a more detailed analysis of the ice flux to section 5.1. Therefore, we calculate the potential ice flux per bin in Figure 5d with our data on velocity and surface height per bin. We will plot it as additional bars in Figure 5d. To include the calving below waterline and the ocean melt we will assume a total ice thickness of 150 m and calculate the flux with this value, the surface height and the velocity per bin. This analysis will strengthen our findings regarding the importance of ocean melt and calving below the water line at the shallow sector. However, note that these flux estimates are only rough estimates to analyse the rough shares of different calving processes and not exact values of different calving fluxes.
We will keep our subdivision of the calving front as we think it is needed for the interpretation. The three parts of the shallow sector show different characteristics likely due to the bed topography and thus they cannot be seen as one homogenous sector.

R2: Better integration of calving wave dataset
The paper should integrate better the ocean wave data as an alternative dataset of calving events (including subaqueous ones?), explain this discrepancy and discuss other potential sources such as iceberg rolling. This better integration of the wave amplitude dataset with the TRI detected calving events would strengthen the discussion and conclusion of the paper.

We will add a peak detection analysis of the calving wave dataset for a comparison with the TRI detected calving events. However, in this paper we want to focus on the TRI dataset and the established methods. A more detailed analysis of the calving wave dataset is the topic of a follow-up paper.

Reply to minor comments

Page 6, lines 15-17:

R2: Is the elevation difference just a shift in absolute elevation explained by a difference in geoid or geo-referencing problems of the Arctic DEM or your DEM? Also co-registering the two DEMs before differencing is useful to assess systematic errors outside obvious artefacts.

R1: Are these differences RMS difference, Mean difference, or other types? These can be quite different because the difference between TRI-DEM and Arctic DEM can be systematically and/or randomly. Please clarify.

We will add a more complete comparison in the revised manuscript on the comparison of the Arctic DEM and our DEMs investigating the variabilities on stable terrain in more detail. The two DEMs were co-registered before comparing them. The elevation difference is likely not just a shift, it also depends on the slope of the terrain. We will outline the test area in Figure 1 or in an additional Figure and better explain the values in the extended error analysis section.

Page 6, lines18-19:

R2: Please provide a sentence about precision change over time on stable terrain and also ice. You could plot this variability on stable terrain and some upper part of the glacier for the single DEMs and the stacked ones to appreciate the effect of atmospheric disturbances and the improvements from stacking. You could use this variation to provide first order error bars for your volume estimates on Figure 6. In the discussion could you compare your precision to what other studies found.

We thank referee 2 for the suggestion, we will add a section on the variability on stable terrain and use it as error estimates in the further analysis. However, as the glacier area measured with the TRI is highly crevassed and fast flowing, we do not think that an analysis of the variability there is meaningful.

Page 6, line 24:

R2: "When applying [...] are removed". The noise observed on stable terrain is not removed, but the signal to noise ratio is higher for the filtered events. Moreover, quantify the number or area of excluded events or give a percentage.

We will implement this suggestion and quantify the number and percentage of excluded events during the filtering process to assess also the uncertainty of the used calving detection method.

Page 6, lines 20-24:

R2: How do you deal with the zero elevation or water elevation when calving occurs along the entire ice column (i.e. column collapse)? Parts of the DEM covering the sea may have Not A Number values or problems with icebergs?

The water elevation is set to 0 also where there is Not A Number values. Thus, it does not influence the calculation if calving occurs along the entire column. The calving at Eqip Sermia happens mostly through ice avalanches, not resulting in big icebergs. Bigger icebergs remove the ice melange in front of the glacier, which results in a loss of coherence. Due to this loss of coherence the area including the iceberg is not included in the analysis. Icebergs further away are not included anymore as they are not within the glacier front mask.

Page 6, lines 23-24:

R1: Does the "10 pixels" mean "10-adjacent pixels"? I feel the two numbers "10 pixels" and "3 pixels" are confusing: Does noise needs to fulfill both "area<10 pixels" AND "width<3 pixels"? If so, how about a block with 3×3 (9 pixels, each pixel shown by an "O" below) shape, or a 2×8 shape? Are these considered calving events if all pixels have elevation decreases more than 5 m?
Because many of the identified calving events are quite small, and shapes of these blocks may not be regular, I think it is important to clarify these settings here.

We agree with referee 1 that the settings for the thresholds need to be clarified more to avoid confusion. We will add some examples in the supplement showing the used criteria's. Calving events need to fulfil both conditions, a size of 10 pixels and a width of 3 pixels. For the width it is enough if only one line within the calving event has 3 pixels. All the pixels need to have an elevation decrease of more than 5 m, except if some pixels with a lower decrease are surrounded by pixels with a decrease of more than 5 m. Then those pixels are included in the calving event.

Page 7, lines 26-28:

R1: I found that the number of total identified calving events is smaller than the sum of identified calving events in the shallow sector and the deep sector (1681 < 1403+289). Did I miss anything? Please also check numbers in Table 1, many of them are not consistent.

The number of total identified calving events is smaller than the sum of identified calving events in the shallow and the deep sectors, because 11 events happened to be located on the boarder of the two sectors. Those events were counted only once for the total number but for both the number of the shallow and the number of the deep sector.

Page 7, line 29:

R2: Add the number of filtered/removed events for each sector as the filter may affect the number of calving events. I have a hunch that the deeper sector may have more small events, likely filtered out, due to the effect of higher submarine frontal ablation.

We will add the number and percentage of filtered events to see if the deeper sector has more small events. However, we will not change the filter threshold as for smaller thresholds it becomes hard to distinguish between noise and calving events.

Page 8, line 2:

R1: Please ensure the minimum size of identified calving block fulfill the threshold defined for calving events (based on lines 21-24, page 6, I calculated a minimum volume 5×10×3.75×3.75=703 m3. Or did I misunderstand the “resolution”? — I picked it from line 13, page 6. Using the radar pixel specified in line 6 page 8 the minimum volume of identified block is even larger, i.e., 1500 m3). If this paper aims to do statistics of calving event sizes, please ensure that the statistics are correct.

R2: I do no understand how you get a minimum volume of 160 m3. If you take a minimum area of 10 pixels with 30 m2 per pixel, you get a height of 0.53 m. This does not match your vertical change threshold of 5 m. So, I guess the comma is misplaced, it must be 1.6 10ˆ3 m and you were correct with “three orders of magnitude”.
Volume / (10x Pixel Area) = Height or 160/(10x3.75x 8) = 0.53.

We thank the referees for spotting this. We realized that some of the events were considered as calving events even if they are collapsing seracs upstream the glacier. They were included as they were located exactly on the border of our glacier front mask. Those events will be excluded in the revised manuscript. The removing of those small events might influence or calving size distribution but all the other results will not be effected.
The algorithm takes also events into account where pixels of less than 5 m decrease are surrounded by pixels of more than 5 m decrease. Thus the events can be smaller than the calculated minimum size of a calving event.

Page 8:

R1: I think TRI-derived DEMs in this paper are very important data, however, there was no figure showing the DEM, neither in the main paper nor in the supplement. Please consider to include a TRI-derived elevation map in the manuscript. Maybe add a panel or two in Figure 3?

R2: The key result of the paper is the TRI-derived DEMs but the velocity (4.1) is presented first instead.

R2: Add text and figure(s) specifically on the generated DEMs and signal improvement by stacking before section 4.2 on calving detection results or a comparison with the UAV DEM.

We will add a section before 4.1 to show a generated DEMs as well as the signal improvement due to stacking and using thresholds (see comments above).

Page 10, Figure 5:

R1: I like this figure! But I also have a few questions and suggestions. First, color changes from dark blue to light blue in Figure 5c is distracting, I suggest to mask out periods without calving using white or grey. In this way, calving characteristics will be more accessible. Consequently, the minimum value of the color bar can be changed to the smallest volume detected based on the settings (lines 20-25 on page 6).

R2: Panel c: Please use a linear colour scale. Your current palette highlights mostly areas with no calving volume change and the yellow parts of your spectrum (7000 m3) and a bit the red ones. Choose a linear colour scale from light yellow to red or blue. Write “data gap” between 22 and 23 Aug.

We agree that the used color changes can be distracting. In the revised manuscript we will change the colors and use grey for the periods without calving while the data gap will be white.

R1: Second, I am confused by the right axis of Figure 5d. Can the authors elaborate on this?

R2: Panel d: Delete the red dashed line representing 50% and 100% of the mean calving volume in the shallow sectors. I don’t understand which message it is supposed to convey. Do you mean that 50% is equal to ~0.4 106 m3 of calving volume?

We will better explain what the red dashed line means in the revised manuscript. The right axis shows the percentage of detected volume if the mean total calving volume of all bins in the shallow sector is assumed to be 100 %.

R1: Third, I could hardly read the superscripts in y-axis label of Figure 5d due to low resolution. Based on the manuscript I guess it was “106 m3” in the bracket, is this correct? Please increase the figure resolution. Also, maybe add “Cumulative” to the y-axis label of Figure 5d to distinguish from the color bar label in Figure 5c. Fourth, I found that the further analysis separates these sectors, so it is necessary to show the exact along-distance ranges of different sectors. Maybe use vertical bars to mark the boundaries of different sectors? These bars can go between the annotations in Figure 5c, such as “ | SL | SM | SR | M | D |”. Last, would it be possible to add a narrow column on the right of Figure 5c and show total calving volume along the entire calving front in color? Such a plot may provide useful information on calving volume changes with time.

We will increase the figure resolution and add “cumulative” to the y-axis label of Figure 5d. We will not reduce the number of sectors (see comments above) but will use vertical bars to mark their boundaries. We will try to add such a column on the right of Figure 5c but we are not sure if the addition of more information in this already dense plot will not be distracting. Also this information is already provided in Figure 9.

Page 11, line 15-16:

R1: The “observable cluster of calving” is hard to tell from Figure 5c. Yes there are some big events, but since this manuscript does statics, in the sense of statics, do these relatively big events really clustered? Need more elaboration.

We think the observation time is too short to do a more statistical elaboration on that. We will delete this sentence.

Page 11, lines 19-21:

R1: More detail of deriving the 25% needs to be provided. Is it an appropriate assumption of a constant front position? Here ice can move up to 16 m/day (Figure 3). And the assumption of a constant mass flux over the front also needs to be justified.

R2: Develop the detail of your computation (numbers?) and how you obtain 25% in the text and on Figure 5d.

We will explain the deriving of the 25% better in the text and also in Figure 5d. The right axis shows the percentage of detected volume if the mean total calving volume of all bins in the shallow sector is assumed to be 100 %. If we calculate the mean total calving volume of all bins in the deep sector it results in less than 25% of the calculated mean total calving volume of the bins in the shallow sector.
We will also add a figure showing the used glacier front mask and the variation of the front over time to show that the changes are only marginally which makes it justifiable to assume a constant front position at least during our observation period. We agree that the assumption of a constant mass flux over the front is just an estimation. However, the velocity of the ice further upstream the glacier is homogenous. We argue that if the flux would not be constant anymore over the whole front we should be able to see signs of that in the surface structure of the glacier (crevasses). In the revised manuscript we will reformulate that section to make it clearer that this is an assumption and we do not have measurements to prove this assumption. However, due to bathymetric data we know that the fjord is deeper at the deep section (see chapter 2.1), which confirms that there the ice area below the waterline is larger.

Page 13, Figure 7:

R1: This figure is nice. I have a suggestion of another figure: plot one similar figure as Figure 7, but use the entire period (similar as Figure S2), and then plot calving volume of the entire glacier front (y-axis) versus time (x-axis) for comparison. This would help further discussion of potential relationship between calving and water level variation.

We will add a panel to figure 9 with the water level variation and the detected peaks as comparison to the TRI detected calving events.

Page 13, line 18:

R1: I think some of these values can be calculated from the data. If the authors use values derived from real data, the further analysis would sound more reasonable. Also, maybe use “a front thickness” instead of “a front height” to avoid confusion?

R2: Your simplification to compute the ice flux is fair but neglects variations in ice thickness and important processes such as submarine melt. It is thus not convincing that the total calving volume matches the computed flux. Try to obtain the ice flux by integrating the glacier velocity, height for aerial calving (ice thickness would be better assuming a certain bed topography) and the glacier discrete width (for each space unit) maybe even upstream of the front assuming constant front position. Also, rephrase this part by first stating what are your assumptions and the missing elements.

We agree that the computation of the ice flux neglects some important parameters. We will therefore recalculate the ice flux with the available data of velocity and height (see major concerns referee 2).

Page 14, lines 1-6, 9:

R2: I am confused here as you seem to compute the aerial ice flux using the ice height above water (150 m) and thus the missing aerial volume in the deep sector cannot be directly caused by oceanic melting, but indirect effect of the undercutting and thereby lower stress threshold for breakoff. Compare the volume you detect with the aerial ice flux in the deep sector with an ice height of 50 m. The estimated volume from the ice flux is then threefold lower than your previous estimate and is closer to your TRI calving volume estimate.

R1: Why assuming an ice thickness of 100 m below the water line? Too many assumptions could result in significant bias. Since the authors have data of surface elevations (shown in Figure 5b), and have assumed a front thickness of 150 m (line 18 on page13, although I suggested to use real data instead of assumption), ice thickness below water line can be estimated.

We agree that for a comparison of the TRI calving volume and an estimated flux at the deep sector we need to do the calculations also with an ice height of 50 m. We will include that in the revised manuscript (see major concerns referee 2). If we assume the same ice front thickness for the deep part than we see for the shallow part using an ice thickness of 150 m is reasonable. This is the ice thickness of the shallow front where the water depth is about 0-20 m. In the revised manuscript we will calculate the flux above the waterline with the available velocity and surface height data and we will compute the total flux using the velocity, the surface height data and assuming an ice thickness of 150 m.

Page 14, lines 16-18:

R1: If subaqueous calving cannot be detected with the TRI, how could it be detected by visual observations and time-lapse imagery? More details are needed. Maybe show some examples of images taken by the time-lapse camera.

With the presented method of the TRI-DEM differencing it is only possible to detect height changes at the surface. In 2018 a time-lapse camera took images every 10 s. With this high temporal resolution it becomes possible to see icebergs coming up from under water. We will add a picture of such an event in the supplement.

Page 14, lines 25-26:

R1: If ice cliff at the shallow sector can have larger but stable height, then why do calving events occur so frequently? Although ice here is thicker but calving should be less frequent or no calving at all because ice cliff can be stable (lines 25-26). May the authors were referring to the potential of calving so"a thick cliff CAN release larger ice volumes", but please note that here calving is quite frequent (also related to how "stable" was defined in this manuscript), while previous figures (e.g., Figure 5c) show that the shallow sectors calved more frequently at the surface. Even add subaquatic calving to the deep water sector, calving at the shallow sectors will still be more frequent than the deep water sector because the authors assumed the overall mass loss are similar in different sectors, plus the deep water sector has lost more mass due to melt.

We agree that this section leads to some confusion. We will rewrite it to make it clearer that calving is more frequent at the shallow sector than at the deep sector. However, the calving events are generally larger at the shallow sector as compared to the deep sector, which can be explained with the inclined front.

Page 15, lines 4-7:

R1: Would enhanced submarine melt cause surface elevation decrease because ice becomes thinner? If it won't lead to surface elevation decrease, then what was there to support the upper part of the glacier? Would the empty chambers cause instability and calving? On the other hand, if it will lead to surface elevation decrease, then the TRI might be able to see the decrease. Please clarify.

With the TRI we do not see a surface elevation decrease. Due to undercutting at the deep sector caused by mass loss below the waterline small calving events, which were filtered out, might be triggered.

Page 15, lines 12-13:

R2: All cited studies estimate aerial calving and neglects subaqueous events. The main reason for a too steep power law curve may be that the study period is too short and there were too many (or not enough) calving events larger than 10ˆ5 m3 (or between 10ˆ4 and 10ˆ5 m3). This would also explain the misfit on Figure 6c.

We agree that this could also be a possible explanation and we will add it to the revised manuscript.

Page 15, line 30:

R1: Please explain what is “big up-floating icebergs”. If the icebergs are big, they might appear on the TRI amplitude images. An example image would be helpful.

Those icebergs are visible on the TRI amplitude images, however it is not possible to see the upcoming of the icebergs on the image as the temporal resolution is not high enough. We will add a time-lapse picture as example.

Page 16, lines 4-6:

R1: Yes I agree that pressure sensor observations could be used to derive calving events, but challenges remain. One challenge is that subglacial hydrological events may cause similar signal as what has described as subaqueous calving in this manuscript. Need justification.

R2: Discuss whether you can identify waves of rolling iceberg from those of calving events. Subaqueous events can often be confused with icebergs rolling close to the glacier front as we can often only identify them from the produced waves.

We agree that a more detailed study of the pressure sensor data is needed. Also turning over of icebergs can generate waves. However, in this study we focus on one method using the TRI dataset and the pressure sensor data will be investigated accurately in another study.

R2: In order to find the missing component presented in the discussion, it would really help to derive a rough calving catalogue based on a peak detector or even manual picking and neglecting other sources of wave oscillations such as iceberg rotation.

In the revised manuscript we will add a calving catalogue based on peak detection for comparison with the TRI-derived calving events.

R2: The air humidity is not present here (section 2.3) and would be useful to assess potential precipitation periods and atmospheric disturbance on the radar signal. Precipitation tends to mess up surface melt and tidal signals seen in calving event occurrence.

We thank referee 2 for this interesting suggestion. We will have a look at the air humidity but only add a subplot if we see that it is informative. Otherwise we will discuss it in the text.

R2: The temporal resolution used in Figure 9 for your calving events may be too high to find a relation that often occurs on hourly time scale. On Figure 9d-e, resample your calving data and apply a cutoff for the two-three largest events around 0.2 in 9d and 10 in 9e. The legend can be plotted once in between the two panels.

We thank referee 2 for this suggestion. We will have a look at this but only change it if we see that it is important. Additionally, we agree that a cut-off for the largest events might help to increase the readability of the graph.

Figure 9:

R2: Stretch the vertical axis for all panels to highlight the variations of your parameters. Shift the shortwave data so that we see that the curve goes to zero during the night (there must still be some light at this latitude mid-august?). Use the same temporal resolution for the Volume and number of events than the two first panel for instance hourly. Cutoff the extreme value on the 26 Aug. evening and write its value on the figure with an arrow pointing to the top. (also see comments page 16)

We thank referee 2 for this suggestions, we will adapt figure 9 by stretching the vertical axis and cut off the extreme value on the 26 August. However, we do not see why the volume and number of events should be plotted hourly, as we would rather loose information by doing this.