Reviewer: Surui Xie

Summary: This manuscript presents detailed observations of the glacier front of Eqip Seria, Greenland with terrestrial radar interferometry (TRI). Digital elevation models were derived from TRI measurements and used to generate calving event data. By analyzing calving event numbers/sizes and their temporal and spatial distributions, the authors concluded that the deep and shallow water sectors of the glacier front have different calving statistics and styles: 1) In the deep water sector (70-100 m), TRI observed calving events are less frequent, calved icebergs are smaller and the sizes follow a power law distribution. Subaquatic calving is prominent here, combined with oceanic melt, they contribute up to 75% of the frontal mass loss. 2) In the shallow water sector (0-20 m), TRI observed calving events are more frequent, with much larger ice blocks (single volume larger than 5×10^4 m^3) occasionally calved, and the sizes of all calved volumes follow a log-normal model. Several possible reasons for the different characteristics in observed surface calving events were discussed, including subsurface melt, subaqueous calving, cliff height and shape, and bed topography. The authors concluded that subsurface melt and calving are the major contributors to mass loss in the deep water sector, but are trivial in the shallow water sectors. On the other hand, an inclined front geometry at the shallow water sectors results in a thicker and stable ice cliff, allowing larger potential ice volumes to calve. Besides, a rock ridge at the shallow water sectors caused less frequent but much larger calving events. In addition, several other types of data (i.e., air temperature, shortwave radiation and tide) were presented but no relationship with calving was established.

The manuscript is well organized, overall well written but can be improved. The topic meets the scope of The Cryosphere, the techniques of TRI and pressure sensor are suitable for this type of study, the references are appropriate. However, I do not think the current manuscript is suitable for publication in The Cryosphere. Below I list some major problems in the data analysis and interpretation. I encourage the authors to address these questions and consider a resubmission.

Major comments:

1) 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, please see my detailed comments) 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 5 m 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.

2) 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.

3) 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.

4) 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.

I think the above comments can already warrant a rejection or resubmission. There are also many other problems with the current manuscript, such as inconsistent numbers and unjustified assumptions. However, I do believe that the authors are working towards the right direction. Hopefully these comments can help.

Sincerely,
—Surui Xie

Detailed comments:

Page 2, line 4: "was" —> "were", data should be plural.

Page 2, line 6: “style” —> “styles”.

Page 2, line 8: “missing” —> “deficiency” ?

Page 2, lines 8-10: later in the manuscript, one conclusion was that that subaqueous calving and oceanic melt combined contribute ~75% to the frontal mass loss. However, here it seems that subaqueous calving itself contribute up to 75% of the frontal mass loss. Please clarify.

Page 2, line 18: It would be great if the authors can be more specific about “water masses in Greenland”. Did the authors mean “water masses around Greenland”, or “increase in surface water due to melt”?

Page 2, line 21: What are the major remaining limitations? A few examples briefly listed here would be helpful.

Page 2, line 24: I am not sure if calving controls tidewater glaciers’s react to environmental condition changes, or verse visa? Please clarify.

Page 3, line 8: “was” —> “were”.

Page 4, line 7: Figs. 1 and 2.

Page 5, line 13: “whole” —> “entire”.

Page 5, line 18: “climate” —> “weather”.

Page 6, lines 16-18: 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.

Page 6, line 17: Does “variability” mean “repeatability”, or changes of topography? Please clarify.

Page 6, lines 17-18: How stable/random is stable/random? Maybe outline the test area in one of Figures 1, 2 or 3? And what does the “values” in line 18 mean? Values of a selected area (if so, please outline it in Figure 1/2/3) or selected DEMs (if so, maybe mark the times in Figure 5c)?

Page 6, lines 23-24: 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 like

“ OOO
OOO
OOO ”, or a 2×8 shape like
“ OOOOOOOO
OOOOOOOO ”, or a shape like
“ OO
OOOOO
OOOO ”

Are these considered calving events if all “O” 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.

Page 6: Apart from using elevation changes to detect calving, it is also possible to identify calving from radar amplitude images. Including an example showing both changes in radar amplitude and elevation would be strong evidence that the method is reliable.

Page 7, lines 5-6: Did the authors mean that p should always be larger or equal to 0.1, or did they mean that one can only trust the sign of R when p≥0.1? If it is the latter, maybe rewrite the sentence to “which tells if one can trust the sign of R (when p≥0.1)”?

Page 7, line 15: Please also specify the low-pass frequency. Just as how the high-pass frequency was given.

Page 7, line 19: To avoid confusion for readers who are not familiar with radar, I suggest to add “line-of-sight” in front of “shadow”.

Page 7, lines 26-28: 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.

Page 8, line 2: 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 $5 \times 10 \times 3.75 \times 3.75 = 703$ m^3. 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 m^3). If this paper aims to do statistics of calving event sizes, please ensure that the statistics are correct.

Page 8, lines 6-9: I think to calculate cumulative calving height a Lagrangian frame needs to be used, because ice at the front can move at 16 m/day (line 21 on page 7), which means a ~100 m displacement during the observation period. If the authors were referring to cumulative calving height from calved ice height at each pixel in each calving events, then line 6 needs to be rewritten, at least taken away “differences” because calved height was estimated from the difference between two DEMs.

Page 8, line 8: If cumulative calving height exceeds 300 m but not up to 300 m, please consider using “extend=‘max’” for the color bar of Figure 4.

Page 8: 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?

Page 9, Figure 4: Comparing this figure with the text makes me confused. If ice velocities on the two sides of sector M are the fastest (see Figure 3) but cumulative calving heights are not the largest (see colormap in Figure 4), shouldn’t these two areas have the widest (along flow direction) spatial distribution of calving events? Please check data processing, and make sure the descriptions in lines 25-27 on page 6 are correct.

Page 9, Table 1: Please check calculations and make sure that numbers are consistent in the table and the main text. In the table, please at least ensure numbers in “Total event volume” equal to “Total event number” × “Mean event sizes”, unless a different math was used.

Page 10, Figure 5: 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). Second, I am confused by the right axis of Figure 5d. Can the authors elaborate on this? 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 "10^6 m^3" 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.

Page 11, line 4: "already observed" —> "shown"

Page 11, line 10: It is probably not correct to say "only … was observed" because small calving volumes are also visible at sector M in Figure 5c. Maybe rewrite to something like "less calving events but several of them are significantly larger than those observed at the other shallow water sectors".

Page 11, lines 11-12: I think there is an ambiguity in"the most individual events". Maybe rewrite it to "the largest number of calving events".

Page 11, line 14: I agree that no clear temporal pattern can be seen throughout the different sectors, it looks pretty much like random noise. Please see my comments above.

Page 11, line 15-16: 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.

Page 11, lines 19-21: 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.

Page 11, line 30: A summary of the meaning of the log-likelihood ratio R would be helpful for understanding the statistics. It seems to be an important parameter describing the likelihood of two different models. Also, were there any reasons to choose the three models here? Since one of the major conclusions came from statistics, more details should be provided.

Page 12, Figure 6: In the abstract I found “The size distribution of the deep sector follows a power law, while the shallow sector is likely represented by a log-normal model.” From this figure, could we say that both can be represented by a log-normal model? The R and p values are identical for the power law and log-normal models in Figure 6d. Or did I miss anything?

Page 12, line 9: Can the authors provide the unfiltered water level during the entire observation period? So that readers will know the overall characteristics of local water level variations. It can either be in Figure 9, or go to the supplement, maybe add one panel to Figure S2.

Page 13, Figure 7: 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.

Page 13, line 14: “several clusters of events”? I thought the authors only observed one cluster of calving events (lines 14-16 on page 11). Please clarify.

Page 13, line 15: How did the authors reach to a conclusion that “no clear temporal pattern of tidal or diurnal recurrence could be detected”? Can the authors elaborate? If no evidence, I suggest to omit this sentence. Or at least admit that this is based on the impression of looking by eyes.

Page 13, line 18: 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?

Page 13, lines 18-23 and after: Please try to keep number of digits consistent.

Page 13, lines 20-21: Not sure if it is correct to say “This value should match up to …”? Although the front position looks stable by eyes, but is this sufficient to support an assumption that total ice flux should match observed total calving volume? More rigorous analysis is needed.

Page 14, line 1: Remove “with”, add “,” before about.

Page 14, line 3: Please check if “160 m^3” is correct. And stacking does not likely contribute to the difference unless there are some errors in the data analysis.

Page 14, line 9: I bet the “$0.47 \cdot 10^6$ m^3 ” correspond to the deep sector? 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.

Page 14, lines 16-18: 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.

Page 14, lines 19-12: I don’t think the authors have shown enough evidence to reach this conclusion. If subaqueous calving account for ~50% of the mass removal from the deep sector, the average thickness removed at the glacier front could be calculate by doing simple math. I believe that will lead to significantly mass thickness removal and the deep sector may become afloat by the end. Plus, can the authors see icebergs coming out from subaqueous calving? If it accounts for ~50% of mass loss, then visual observations or time-lapse camera images (line 17) may be able to see icebergs coming out from subsurface. Please provide evidence to support the conclusion.

Page 14, lines 25-26: 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.

Page 15, lines 4-7: 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.

Page 15, line 12: Please show the "Observed subaqueous calving events". Because the authors wrote that there were time-lapse cameras (line 17 on page 14). If no captured subaqueous calving events by instruments, then please provide more detail of the available observations by authors in the field.

Page 15, line 19: "acting" —> "dominant"?

Page 15, lines 19-24: The paragraph relies on the assumption that submarine calving in the deep sector is a major contributor to total calving volume. Please evaluate the assumption based on comments above.

Page 15, line 30: 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.

Page 16, lines 4-6: 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.

Page 16, lines 13-15: Perhaps this paragraph needs to be rewritten, because I don't get the logic of cause and effect. Sentences before and after "Therefore" seem to be out of place. What do tides do with air temperatures and radiation? Other readers may also be confused.

Page 17, Figure 9: In (d) and (e), are the calving events in the deep sector plotted above calving events in the shallow sector? Please note this in the caption, otherwise readers can assume that all these histograms start from 0 in the y-axis.

Page 17, line 10: Maybe "surface" should be added to the front of "calving event" because subaqueous calving was also discussed?

Page 17, lines 13-14: Or it may not have to be explained by other processes? I don’t see the necessity of assuming similar ice flux in the two sectors.

Page 18, line 2: Here “center” was used, but at other places “centre” was also used. Please be consistent.

Supplement page 1, Figure S1: A more accurate job needs to be done if the front line was estimated visually, because this research studies many relatively small calving events. If there are satellite images at the beginning and the end of the campaign, then they should be plotted here. If no available satellite images at these times, TRI amplitude images can help.

Supplement page 1, Figure S2: As I commented above, please add a panel to show unfiltered water level.

Supplement page 2, Figure S3: I thought the tide data were heavily filtered, why there is a jump around 08:00 on the 20th? Please check.