Response to Referees on tc-2021-176

We appreciate the reviews and comments from both Referees. Please find the response to Referee 2 on pages 1-4, and the response to Referee 3 on pages 5-7.

Response to Referee 2 on tc-2021-176

First, we would like to thank the referee for reviewing and commenting the manuscript again. Please find the item-by-item reply below, with the original comments in *italics* and the responses in blue. Simple changes in the text are responded with 'done'. The suggested changes haven been implemented in the revised text.

First, I would like to applaud the authors for the fully committed response to the initial review and the almost full rewrite of the manuscript. This revised manuscript now stands much stronger and I will recommend it for publication in The Cryosphere without any major revisions.

The increased focus on how the applied retracking thresholds is resulting in surface retrievals removes most of my initial concerns of possible elevation biases due to the ratio between volume and surface scattering. The method is shown for a warm year (2019) and seems to behave very nicely under these difficult conditions. I would suggest future work to look at the 2012 melt event, to see how the method is coping with the change in scattering regime in the LRM area during July. I guess from your comment to L157 of the first review this is also on your mind, and I only can support this.

One thing which is a bit puzzling is the good agreement between the ActicDEM and ICESat-2 (<2 cm), and then the 24 cm bias between the LEPTA and ActicDEM. This points to biases introduced in the inter-comparison of the data. Maybe something worth to be mentioning.

Thanks for pointing us to this issue. In the previous version of the manuscript, the comparison between ArcticDEM and ICESat-2 was conducted over the entire Greenland ice sheet, that is over both the CryoSat-2 LRM and SARIn zones. In the revised version, we have conducted the comparison only over the LRM zone. The median difference is 21 cm and the standard deviation 1.13 m, which are more coherent with the LEPTA-ArcticDEM difference.

I still find the 50x50 km grid for Greenland too coarse and as the figures are already made, I would suggest changing to the 25x25 km as the "salt and pepper" is giving insight on the retracker behavior.

This is changed in the revised manuscript (Fig. 3, Fig. 6, Fig. C1 and Fig. C2).

This leaves me with minor corrections:

L15-20: It is hard to judge the better performance as depending on the measure different methods perform similarly. Suggest simplifying and/or maybe only looking to the lower standard deviation. Or perform a significance test to see if you have statistically significant better performance. I know you argue against the t-test in the reply to my previous request, but tests might be more suitable. Otherwise, you should not use significant in L350 as you do.

We agree that it is hard to judge the better performance. We changed the formulation in the abstract from:

'Benchmarking of the LEPTA method to the slope- and point-based method based on CryoSat-2 LRM acquisitions over Greenland in 2019 shows that heights obtained by LEPTA outperform the other methods when compared to ICESat-2 observations, both in the flat, interior regions of Greenland and in regions with more complex topography.'

to (now L9-12):

'Benchmarking of the LEPTA method to the slope- and point-based method based on CryoSat-2 LRM acquisitions over Greenland in 2019 shows that, when compared to ICESat-2 observations, the method has a stable performance both in the flat, interior regions of Greenland and in regions with more complex topography.'

We also changed the sentence:

'Although ESA Level-2 products and the point-based method have good performance in either median and median absolute deviation, LEPTA stably outperforms the other methods.'

to (now L17):

'Although ESA Level-2 products and the point-based method have good performance in either median and median absolute deviation, LEPTA shows a good performance in terms of both metrics.'

We also removed the word significant in the conclusions section. The sentence is changed from:

'Regionally, the impact may be more significant. In particular, we observe significant changes in the vertical and horizontal position of the impact points towards the margins of the LRM zone.'

to (L349):

'Regionally, the impact may be larger. In particular, we observe changes up to 1.46 m in the vertical and 231 m in the horizontal position of the impact points towards the margins of the LRM zone.'

L28: 717km should be the real orbit of CS2.
The numbers have been changed accordingly.

L35: Suggest removing references as they already are mentioned above.
Done.

L47: (and multiple other places) This is a matter of style but suggests removing reference to a section in the manuscript.

They have all been removed.

L51-56: A matter of style but suggest to only keep:
"To assess the performance of the LEPTA method, we apply it to all CryoSat-2 LRM acquisitions over Greenland in 2019 and benchmark it to the slope and point-based methods by comparing it with laser altimeter ICESat-2 height measurements."
We prefer to include an outline of the paper as we think it helps the reader.

L63: Baseline E is now available, however, if you look at the release documentation this baseline does not suggest having many changes for ice sheets, and therefore are your results still valid. This should be mentioned.
This has been added to the Discussion section (now L381-383).

L75: You are properly right about the threshold but the reference to Bamber is rather old concerning baseline thresholds. Please clarify.
Unfortunately, we could not find an updated reference. Hence, we decided to remove the statement.

L100: (and L361) The link should not be the reference please refer in a more conventional way (author for the document).
(Now L99) Done.

L182: Remove sentence, section 3.2 is the next section.
Done.

L220: -> "from -7.5 to 2.5 meters"
(Now L217) Done.

L241: Please enlighten the reader with your explanation of this difference.
Unfortunately, we lack such an explanation. When we compare ICESat-2 and ArcticDEM we observe a similar pattern as observed when comparing LEPTA and the point-based method to ArcticDEM. Likely, it is a combination of elevation change signal and errors in the correction methods.

L267: But they are fitting internal despite the timing between ArcticDEM and ICESat-2
Please see the reply of the general comment.

L279: remove "As mentioned in Section 3.3,"
Done.

L351 "< 1 dm" -> "< 0.1 m" or "< 10 cm"
(Now L348) Changed to 0.1 m.

L392: Why is "novel" removed here, it is still in the abstract.
The 'novel' in the abstract has been removed.

L421 "B1-B1" -> "B1 and B2"

(Now L410) Done.

L421: The underlying skewness is hard to see. The figures should be improved. If the purpose of not showing the distributions in one panel is to show min and max values, these may be mentioned in the caption. The range should be centered to something like 10 meters.
The figures are now displayed between [-10 m, 10 m] and the captions of Figs. B1 and B2 have been changed from:

'Full probability distribution functions of heights between CryoSat-2 and ArcticDEM derived from a) ESA L2I, b) slope method, c) point-based method and d) LEPTA.'

to

'Probability distribution functions of heights between CryoSat-2 and ArcticDEM derived from a) ESA L2I, b) slope method, c) point-based method and d) LEPTA centred between [-10 m, 10 m]. To clearly show minimum and maximum values (values displayed with arrows), the curves are not displayed in the same panel.'

Response to Referee 3 on tc-2021-176

First, we would like to thank the referee for reviewing and commenting the manuscript. Please find the item-by-item reply below, with the original comments in *italics* and the responses in blue. The suggested changes have been implemented in the revised text. Simple changes are responded with 'corrected' or 'done'.

General comments:

Li et al present a new method for correcting slope errors in pulse-limited radar altimetry data, based upon using information in the leading edge and a high resolution DEM to more accurately locate the point of closest approach. The authors provide an extensive validation comparing their product and others to ICESat-2 measurements and a DEM, and a series of sensitivity analyses to explore the robustness of their approach.

I enjoyed reading the manuscript, the method detailed here offers genuine improvements over those currently used, and this is well demonstrated by the authors here by their analyses. The authors have done a considerable amount of work to address previous reviewers comments, which have improved the manuscript considerably. I only have one minor comment regarding the use of different retracking thresholds, which I would appreciate if the authors could address:

As I understand it from P3 L75, the ESA L2 product is using a 25% OCOG retracker, and the authors are using a 20% threshold – is this an additional source of height difference when comparing LETPA and ESA L2 heights, and has this been accounted for? To my knowledge it does not seem to have been discussed anywhere in the text (if it is indeed something worth considering). Is it possible to disentangle height differences due this choice of retracking threshold from the different slope corrections?

As pointed out by Referee 2, the reference to Bamber 1994 is quite old. We searched for an updated reference but could not find any. Therefore, we removed the statement regarding the threshold used in the ESA L2 product. To answer the referee, from our sensitivity analysis in Section 4.4, an OCOG threshold located between 30% and 40% can indeed result in a near-zero CryoSat-ICESat difference for the slope method. However, as mentioned in L345 of the revised manuscript, we cannot tell exactly why our implementation of the slope method differs largely (20 cm in median) from the ESA L2I products. Therefore, without knowing the exact retracking process of ESA, we cannot exclude that the difference between LEPTA and ESA L2I products is due to different OCOG thresholds, and the statement on L348 'Detailed analysis (not shown in this paper) shows that the differences cannot be explained by the fact that in our study we use another DEM as well as a different OCOG retracker threshold' has been changed ('as well as a different OCOG retracker threshold' is removed).

Specific comments:

P1 L4 – 'Therefore different correction methods have been developed ranging from the slope method to the point-based method' – to me this seems to have little meaning in the context

of the abstract as they are technical terms explained later in the paper, I suggest either expanding on their meaning or this sentence could be easily removed
We reformulated the sentence.

P1 L9 – 'within range' > 'within the range'?
(Now L8) Corrected.

P1 L9 – the phrase 'impact point' is used here and throughout the paper and seems imprecise language to me, and does not seem to be properly defined anywhere in the text. I suggest the authors more clearly explaining what they mean by this where it first appears in the main text
The phrase impact point is used by, e.g., Roemer et al. (2007) and Levinsen et al. (2016). It refers to the point from which the radar wave reflected. We have added this 'definition' to the main text (L32).

P1 L31 – the way these sentences are worded leaves it unclear as to which are 'slope' and are 'point-based' methods, I suggest re-wording to make this more clear
(Now L33) We reformulated the sentence. After stating that 'the most widely used methods involve both a correction to the height as well as a relocation of the satellite measurement location from nadir to the expected impact point' we say that 'Two implementations of this so-called "relocation method" are known as the "slope" and the "point-based" method (Levinsen et al., 2016).'

P1 L41 – 'footprint that illuminates the terrain' > 'footprint illuminated by the satellite on the terrain'
(Now L42) Done.

P1 L50 – 'begin' > 'beginning'
Done.

P2 L67 – 'proper' > 'appropriate'
Done.

P3 L75 – not sure what is meant by 'removed in case', do the authors mean 'removed if'?
True. All misuses of 'in case' have been changed into 'if'.

P7 L167 – 'firn-air' interface may be more appropriate than 'snow-air' for ice sheet interiors
(Now L165) Done.

P7 L186 – I'm not sure what the authors mean by 'conceptual assessment' here
We removed the sentence. We wanted to stress that the assessment based on Arctic DEM cannot be considered as a validation. This is, however, already stated in L186-187 of the revised manuscript.

P8 L193 – Again I'm not sure what the authors exactly mean by 'In case', do they mean 'if'?
All misuses of 'in case' have been changed into 'if'.

P11 Fig3 – It is more work so I will not ask the authors to do it for this paper, but in future they may wish to consider looking at the differences for each technique as a function of slope, this may in particular highlight the benefits of their method

Thanks for the suggestion. This may indeed be interesting. But so far we make no changes to the manuscript.

P19 L348 – If the detailed analysis is 'not shown in this paper' could the authors please provide more information as to where it is shown? There are other instances of this in the discussion section. Is this information not included in the appendices?

They are included in the response to the referees in the previous major review. However, adding the results to the appendices will be an overloading information. We agree to remove the 'not shown' statements in L253 and L354, but as for L348, we prefer to not remove it as it keeps the logic of the sentence.