

The Cryosphere Discuss., referee comment RC1 https://doi.org/10.5194/tc-2021-176-RC1, 2021 © Author(s) 2021. This work is distributed under the Creative Commons Attribution 4.0 License.

Review of 'A leading-edge based method for correction of slopeinduced errors in ice-sheet heights derived from radar altimetry', by W. Li et al.

Anonymous Referee #1

Referee comment on "A leading-edge-based method for correction of slope-induced errors in ice-sheet heights derived from radar altimetry" by Weiran Li et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-176-RC1, 2021

This manuscript describes a new approach for relocating radar altimetry measurements acquired over ice sheets; one of the most important processing steps for retrieving reliable surface elevation measurements. The authors outline the method, together with a proof-of-concept study whereby the approach is applied to one year's worth of CryoSat-2 LRM measurements over the interior of Greenland. They perform validation relative to ICESat-2 measurements and an independent DEM, alongside a sensitivity analysis to explore some of the inherent assumptions within their approach.

I found the manuscript very interesting; the proposed methodology is novel and definitely has the potential to improve upon current approaches documented in the scientific literature and implemented within ESA's ground segment. I therefore believe that it will be of interest to the subsection of The Cryosphere's readership that have an interest in radar altimetry processing techniques over ice sheets, ice caps and glacier surfaces. That being said, I believe that there is still some additional work required to (1) convincingly demonstrate the superior performance of the method relative to existing approaches, and (2) to provide the necessary level of methodological detail required to adequately document this promising new method. Without this, I am left feeling that I have a glimpse of an exciting new approach, but have many unanswered questions that prevent me from being fully convinced that it delivers the improvements that the authors claim. I hope that, by addressing these points, the authors will be able to provide a more compelling demonstration for The Cryosphere's readership. I have detailed these major comments below and would like to see each of them addressed in the revisions. Following these comments I have also listed a number of more minor points, which I hope will help to improve the clarity of the manuscript. Finally, I would recommend that the manuscript undergoes a thorough check for grammatical errors, as there were a considerable number throughout.

Major comments

Performance of LEPTA relative to other approaches.

The authors compare LEPTA to the ESA L2I product, and their own in house versions of the slope correction method and the Roemer et al. (2007) relocation method. Whilst the statistics show the superior performance of LEPTA, I am left with several important questions relating to the implementation of other approaches, which make it difficult to determine whether they have been implemented optimally; i.e. whether a better implementation could have yielded improved results more closely matching the performance of LEPTA. Specific points that I would like to see addressed are as follows:

- For ESA L2I have any of the quality flags included within the product been applied? More L2I data are available than for the in house methods and this makes me wonder whether stricter quality control has been applied in the latter, e.g. the waveform filtering mentioned on line 289. In other words, that some of the improvement of LEPTA relative to L2I is not due to the method used slope correction, but simply down to the quality control applied.
- For the authors' in house 'slope correction' method the results, e.g. as shown in Fig 4, indicate far worse performance than the ESA L2I implementation, and make me concerned that their slope correction method has been implemented sub-optimally. This, combined with point 1 above, means that I do not think that a convincing case has been made to justify the level of improved performance of LEPTA relative to the slope correction approach. This is not to say that LEPTA is not an improvement, but just that I feel that more work is required to justify this convincingly. Specifically, if the authors really believe that the difference between L2I and their in-house implementation relates to the Doppler slope correction, then I would like to see further analysis to demonstrate (1) that this really is the case (i.e. that the Doppler slope correction can be responsible for a difference of this magnitude), and (2) why it does not affect LEPTA in the same way (and should not be incorporated into the LETPA L2 processing). I would also like the authors to state the DEM resolution used for the slope correction (I couldn't seem to find it anywhere), and if it is 900 m or less, to justify why

this is an appropriate choice. From my perspective, the 'resolution' should be comparable to the beam limited footprint (i.e. 10's of km), not the pulse limited footprint, because it is preferable to relocate using the large scale slope across the illuminated area. If you use the '900 m' slope at nadir, then there is the risk that the slope you use will not be representative of the average slope across the illuminated area. Indeed I think you could be seeing this effect in Figure 7, where performance improves up to a resolution of 900 m, and raises the question as to whether you would see further improvements if the resolution was increased any more. As such, I would like the authors to either provide a justification to counter the above concerns, or to test this by computing the slope over a larger length scale (comparable to the beam limited footprint) and re-evaluating the performance of their slope-based method.

■ For the authors' point-based approach, I find the magnitude of the bias surprising, e.g. as shown in Figure 4, and that there is a general lack of detail or discussion required to assess whether this is due to the implementation of the approach. In particular, I cannot find any information relating to the search area that the authors have used; i.e. the illuminated area on the ground where they assume the leading edge reflection could have come from. It would be reasonable to base this upon the 3 dB beamwidth of the instrument, but it is not clear to me what the authors have used. As such, my concern is that an inappropriate choice could lead to a bias in the 'point-based' solution; for example if the criteria used is too strict, and does not allow for the POCA to be sufficiently far away from nadir. I would therefore like to see the authors (1) state what criteria is used, (2) justify why it is appropriate and not impacting the accuracy of the results, and (3) dependent upon these points, consider whether the performance of their point-based approach should be re-evaluated with a refinement to the allowed relocation distance.

Choice of delta-r.

The choice of delta-r seems rather arbitrary, yet central to the LEPTA approach, and so I would like to see some more discussion relating to this point within the manuscript:

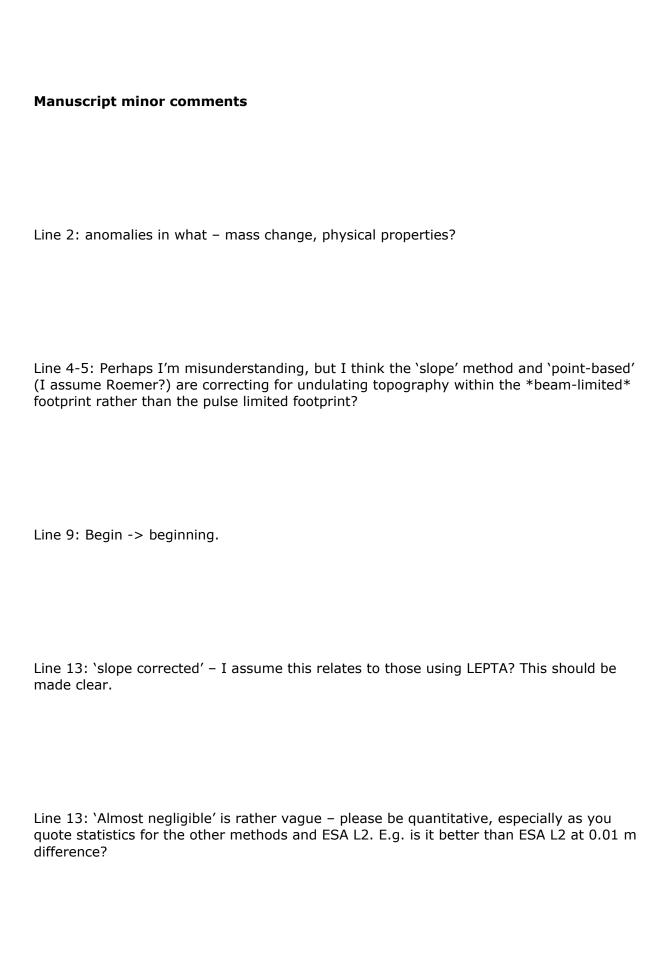
From a theoretical perspective, clearly it would make sense to let delta-r vary according

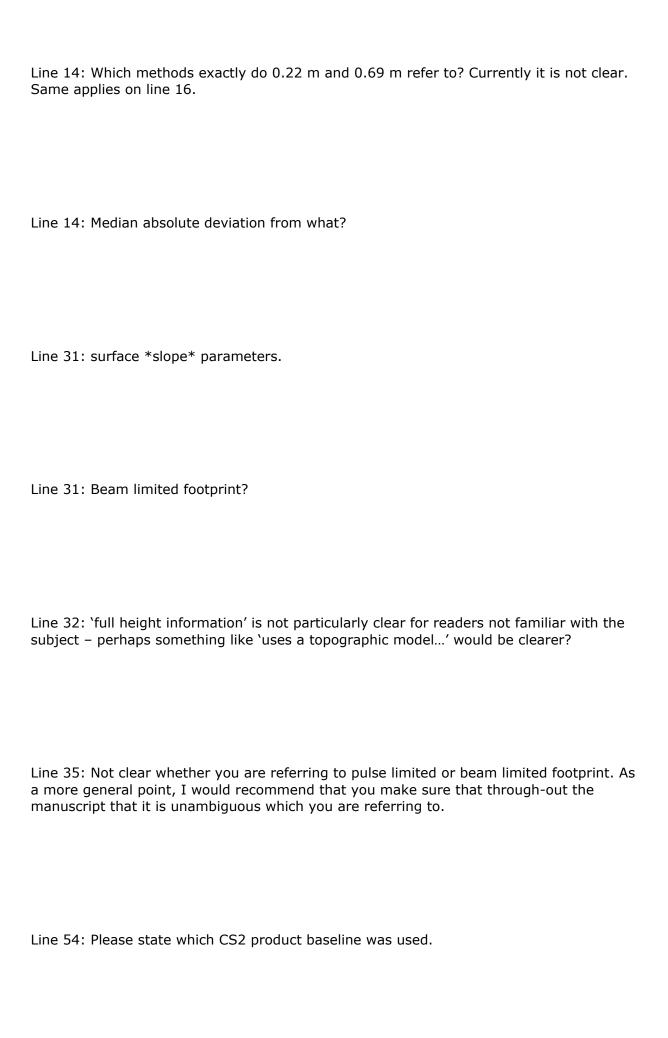
to the width of the leading edge of each waveform. I assume the authors have practical considerations for why they chose not to implement this approach, and I think it would be helpful for readers if they could therefore expand on this within the manuscript, to explain why such an approach was not selected.

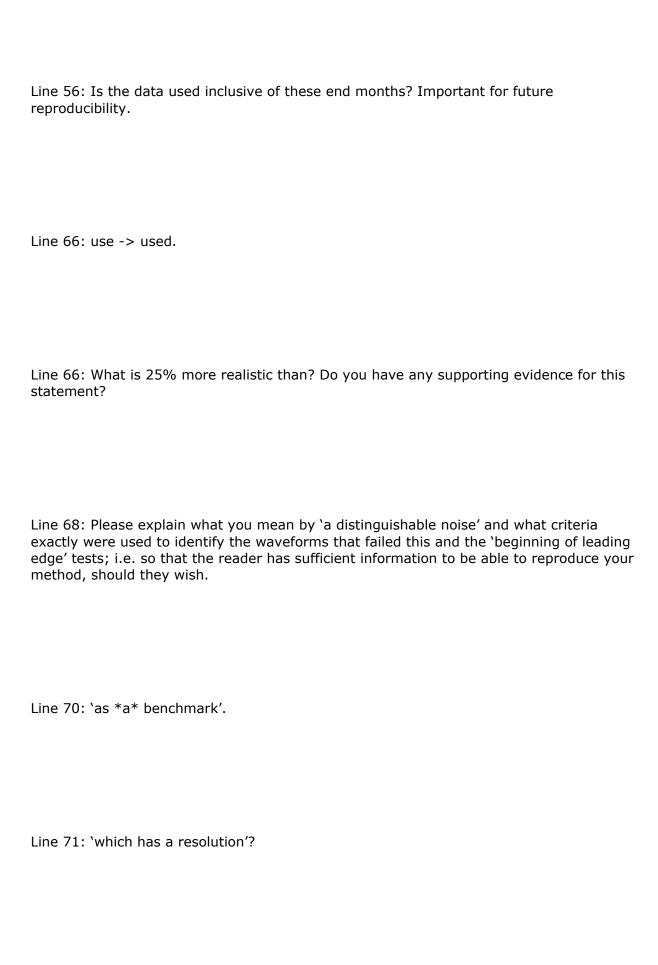
- I appreciate this is extra work, and therefore I would not insist upon it, but given the central role that the leading edge plays in the LEPTA approach, I think it would be really valuable for the authors to provide some quantitative measures relating to the characteristics of the CryoSat-2 LRM leading edge over Greenland. For example, can you provide statistics relating to the mean and standard deviation of the range spanned by the leading edge? This would provide really helpful context for judging the validity of the range of delta-r considered.
- Without point 2 being addressed, it's not clear to me why delta-r of 2 metres is a reasonable lower bound. I would therefore like to see the sensitivity analysis expanded below 2 metres, or a justification for why this is not appropriate; as, in theory, choosing a lower threshold would seem a sensible approach to ensuring that you always identify terrain corresponding to the leading edge.
- I also suspect that the optimal choice of delta-r might vary significantly spatially; yet this is impossible to assess based upon the median statistics presented. For example, that a delta-r of 2 m or lower might perform much better over simple topography. Given the central role of delta r in terms of the LEPTA approach, I think it would be interesting to produce spatial maps of the type shown in Figure 4 for a LEPTA-delta-r of 1 m and 2 m, to see the extent to which this can improve upon the 3.5 m case already plotted.

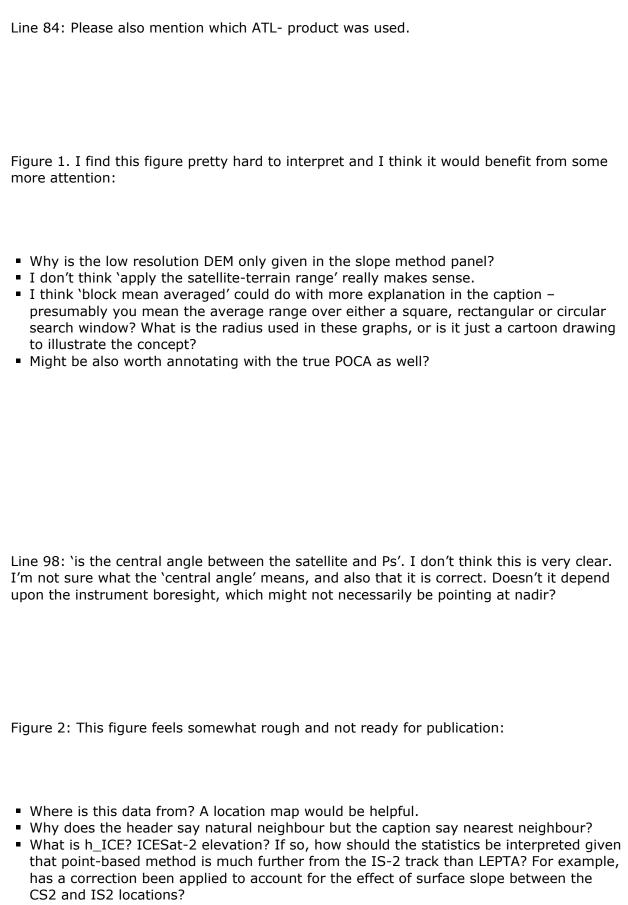
Impact of penetration

Throughout the manuscript, the issue of penetration into the snowpack is never mentioned. I do not think it requires further analysis, but I do think it would be helpful to include some discussion related to this phenomenon, and whether or not it has any implications for the LEPTA method; given that LEPTA uses range information from the leading edge, and the leading edge of LRM measurements can be modified by subsurface scattering.

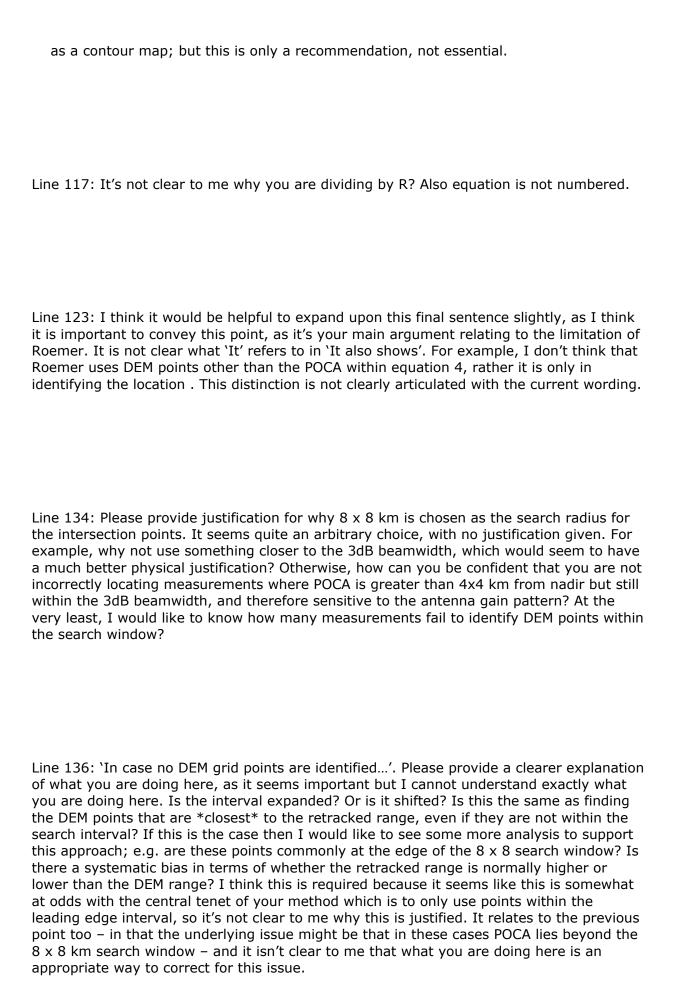


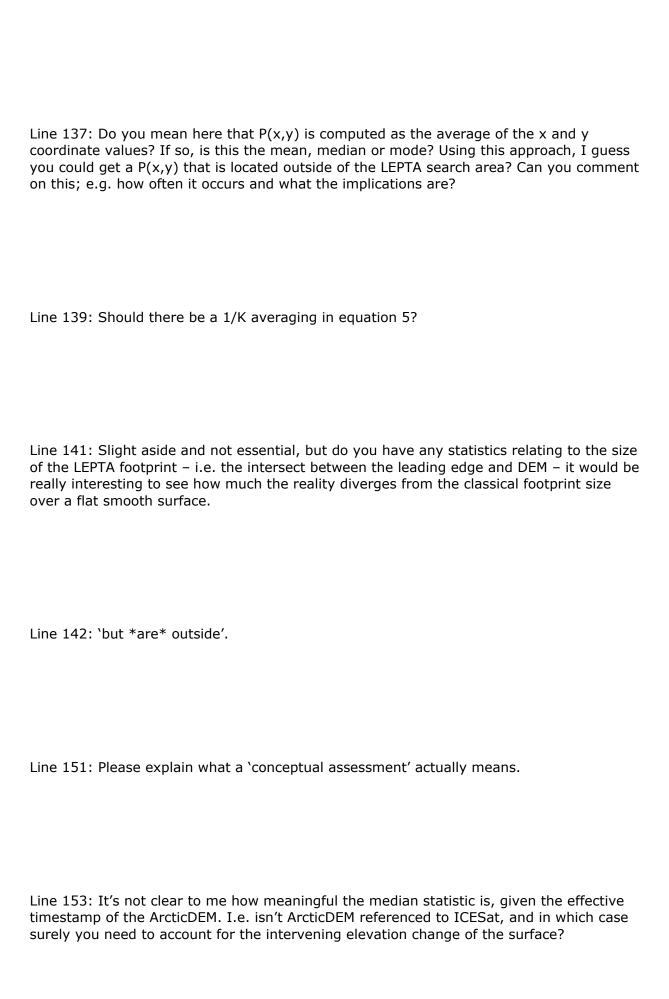






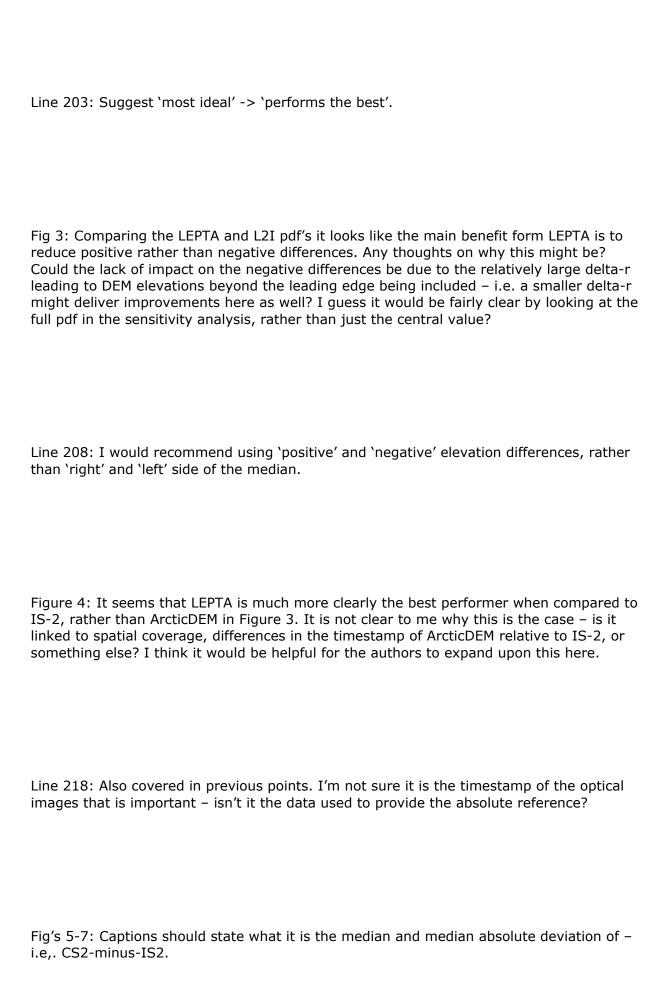
- What is d min?
- How were the ICESat-2 tracks that are plotted selected?
- Visually, I think it would be easier for the reader to interpret if the DEM was displayed

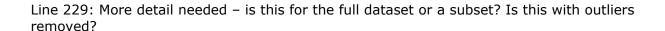




Line 160: In the case of nearest neighbour, is a correction applied to account for the effect of surface slope between the CS2 and IS2 locations? If not, why not and what are the implications? Given that ArcticDEM is already integrated into your processing flows, I assume it would be pretty simple to do this.
Line 167: 'h_DEM / h_ICE2' – replace '/' with 'or' to avoid any ambiguity with a division operator.
Line 169: Would it not make sense to also consider sensitivity to how the start of the leading edge is defined? Surely this is relevant too?
Line 186: 'best' relative to what – I assume you mean of all methods, but it could be construed as ArcticDEM vs IS-2, so worth making clear.
Table 1:
 'Before' and 'after' are not used in the table, so I would recommend not needing to refer to them in the caption. Please state whether differences are calculated as CS2 - ref, or ref - CS2. I'm not sure how useful it is to list all the percentiles in a table. Have you considered

showing these as a cumulative distribution figure instead? I think this would be much easier for the reader to interpret.





Line 230: It's not very clear to me how this choice of 2-5 m actually relates to the properties of the leading edge. I think it would help to justify this choice in the minds of the readers, if the authors could describe the typical width of the leading edge, and show that delta-r is a sensible choice within this context. For example, with the current analysis as it is presented, I am left wondering how common it is for the leading edge to be less the 20% OCOG + 2 m; i.e. to lie outside of the range tested. From a theoretical expensive I could see that a delta-r value of 0.5-1 m could make sense, but there is no analysis to explain why this parameter range was not explored; nor indeed why the actual range of ranges spanned by the leading edge of each waveform was used. Did the authors evaluate what happened when delta-r < 2 metres?

Line 244: I'm interested in why the sensitivity to a bias in the DEM is not symmetrical about zero. Can the authors expand upon this point; i.e. why having a biased-low DEM has very little effect, but biased-high does? Is this somehow connected to a generous choice of delta-r, i.e. that at 3.5 metres, it is actually including a significant buffer beyond the leading edge, such that when you bias the DEM low the true POCA still remains within the delta-r range? I think a slightly more in-depth evaluation and discussion for the observed behaviour would be useful here in terms of understanding the method, rather than a simple 1 paragraph summary of the sensitivity results with minimal interpretation.

Line 254: Again, I think the manuscript would benefit from critical interpretation here, rather than simply reporting the bare results. For example, can the authors expand on why the point based approach degrades so quickly with increasing resolution – is it due to topographic peaks being smoothed? Wouldn't you expect the point-based approach to tend towards the slope based approach; i.e. with sufficient smoothing then you remove all high frequency topography and are just left with the long wavelength slope?

