Authors point-to-point responses Referee 'Comment on tc-2023-80', Anonymous Referee #1, 29 Sep 2023
Please find the author's responses in blue below the reviewer's comments.

Many thanks for the review, which help to improve the quality of the manuscript.

The authors have demonstrated commendable expertise and innovation in their work by introducing CNN in the field of radar altimetry retracking. Their research represents a significant contribution to the field, offering valuable insights and promising results. By employing CNN, the authors have opened new possibilities for improving the accuracy and efficiency of altimetry data processing, and possibly (not addressed) speeding up otherwise time-consuming retracking. The paper introduces this innovative approach and provides convincing evidence of its advantages. While comprehensive research is crucial for presenting a well-rounded perspective, the excessive length of the papers can be challenging for readers to digest. One suggestion would be to separate the retracking from a second paper on the applications or move some of the backgrounds to an appendix. This being said, I only have minor comments on the paper.

Thanks for this suggestion. We are aware of the length of the paper but we don't want to split the manuscript in a retracker and application part. We need to show the improvement which can be derived with our new approach and therefore need to apply it to data and compare to other products. We therefore think that we cannot decouple the application part from our new method.

One aspect of AWI-ICENet1 which could be addressed is the possible increased efficiency in the processing time. As the authors have great experience in retracking radar altimetry data, it would also be beneficial to highlight the efficiency of the AWI-ICENet1 compared to other methods. We are often faced with very long reprocessing times from the agencies.

Thanks for the advice. We will add a table comparing processing times of different retracking methods.

In general, the caption for the many figures is very shallow, please read through them and elaborate on them, so a reader who is not reading all 61 pages can follow the main conclusions of the figures.

Yes, we do understand that it is beneficial to add more info into the caption and will follow the advice in the revised version.

L2: "long-term observations of surface elevation change are required to", agree with the meaning but I would suggest that you also acknowledge other methods and hence remove the "Surface"

We fully agree that it is important to highlight other methods, too. Many thanks for pointing this out! We will change it accordingly.

L5: The snow penetration is an issue in most places, just leave this sentence open and remove "especially over the…"

We agree, unfortunately, it is an issue in most places! We are removing 'especially over ice sheets' in the revised version.

L15: This shows a broader application and suggests writing "This technique provides new opportunities to utilize convolutional neural networks in the processing of satellite altimetry data, which can be applied to historical, recent, and future missions."

Thank you, we will change the sentence accordingly.

L21: missing reference at “2010 onwards”

We will add a reference here

L30 may start with “Ku-band satellite altimeters…” as the mentioned satellites are all Ku.

Thanks, we will follow your advice.

L40 Why add the PLRM, this is a pure post-processing product.

Thank you, we will remove this sentence as it is not necessary in the context here. However, we think the PLRM data is still very useful as it allows comparing SAR and LRM and enable to link older missions to the new SAR generation.

L43: ICESat-2 operating at green-wavelength is penetrating the snow. And add references for ICESat-2.

We will add a reference for ICESat-2. To our knowledge there is little known about penetration of green lasers into snow/ice . A TCD paper of Studinger et.al. (currently under review) propose a “differential penetration which likely exist in lower-level ICESat-2 data products” at least over thin or finger rafted sea ice (https://tc.copernicus.org/preprints/tc-2023-126/tc-2023-126.pdf). However, their modeling approach suggest only minor penetration in dry snow. We will add this reference here and weaken our statement that laser is not penetrating the snow firn pack.

L46: please elaborate on the sentence “Because…”,

We will reformulate the sentence.

L73: the common abbreviation is CNN, please consider using this

We have chosen ConvNet on purpose and would prefer to use it in our manuscript. The reason is the following: Both CNN and ConvNets are common abbreviations / names of the same machine learning framework, e.g. Tan and Le, 2020, “EfficientNet: Rethinking Model Scaling for Convolutional Neural Networks” uses ConvNet as abbreviation for Convolutional Neural Network. Non-academic examples of the broader community showing that both terms are valid include

IBM: https://www.ibm.com/topics/convolutional-neural-networks

MathWorks: https://www.mathworks.com/discovery/convolutional-neural-network-matlab.html

SuperAnnotate: https://www.superannotate.com/blog/guide-to-convolutional-neural-networks

We prefer ConvNet since we agree with large parts of the Machine Learning community that a too strong emphasis on “neural” (as referring to the visual cortex of mammals) is misleading and bears risks. As a consequence, several leading examples (see a few below) even prefer “Convolutional Networks” instead of “Convolutional Neural Networks” which is not only more precise but also historically more correct as this was the term originally used (see e.g. LeCun and Bengio, 1995, “Convolutional Networks for Images, Speech, and Time-Series”):

LeCun et al.,, “Character-level Convolutional Networks for Text Classification”, NeurIPS 2015

Zeiler and Fergus, “Visualizing and Understanding Convolutional Networks”, ECCV 2014

Long et al., "Fully convolutional networks for semantic segmentation", CVPR 2015"

L124: why exclude Greenland?

We selected Antarctica to build the training data set, as it covers various kinds of flat and complex topography. Finally, we applied the saved model to observed CryoSat-2 waveforms also over Greenland. Results are very promising and show that the training over Antarctica is sufficient to improve the retracking over both ice sheets. As we show and discuss in the paper the new approach can also handle the melt event in 2012. Although possible, we therefore do not see the need to include Greenland in the training of the CNN.

L128: to help the reproducibility of the study please give more insights into the chosen backscatter cross-section values.

sigma_0 = 10 dB . At this stage we didn't consider any heterogeneity nor angular dependency of sigma. We will refine our the text to be clear in the revised version.

L144: "rate in the following", the paper has a couple of these please proofread the paper once more.

Thank you, we will proofread and correct spelling mistakes.

L153 What is the resolution of the applied DEM in the modelling? (it might not fit here but should be discussed in this relation)

We used the REMA DEM with resolution of 1 km and slightly smoothed the DEM with a kernel of 3km to mirror the effect of an integrated signal within the pulse limited footprint. We tested different smoothing kernels to best match observed CryoSat waveforms.

L177: move the specific tensorflow package to the acknowledgement, and add a reference to this library.

Thanks for this advice. We will add this to the revised version.

L189: With possible differences/drift in Bs in the processing baseline only one should be used. For consistency use E.

We will rerun our processing and update products and tables accordingly as Baseline E is now fully released. By the time of writing the paper ESA still solved issue in their reprocessed products which affected data within the considered time period from 2019 to 2022.

L194: add a reference for ATL06.005

We will add a reference to the newly released ATL06.006 product. In addition we will rerun the ICESat2 processing and update figures and table accordingly.

L196: Add the resolution used.

Thanks. We will add this information to the revised version.

L210: so it is the 2 km product which is used, why is this chosen?

We found that 2km is the best choice to have enough data points to apply a solid regression but with less influence of topography.

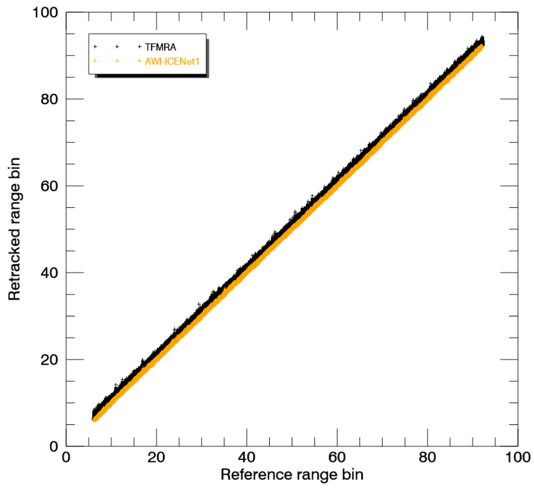
L221: “the correction adapted from Nilsson et al. (2022)” elaborates on what is used.

We will be more precise and add the following sentence:

We calculate different variants of corrections for transient penetration effects by using empirical linear relations between Delta h and LEW, Delta h and backscatter, or Delta h and LEW and backscatter. Instead of doing this in the context of multi-parameter fitting as in e.g. Flament .. Simonsen .. Schröder, we do it on the level of spatially interpolated monthly anomalies of Delta h , LEW and backscatter, following the approach of Nilsson et al. (2022). We assume that changes of electromagnetic properties ..."

F5: add a plot of the model vs. test point cloud. Maybe retracked range vs model range.

We can add the following scatter plot, but we don’t really see why this will help to further evaluate the retracker performance as already shown in Fig 7.



L238: As Greenland is different and possibly more complicated it would be nice the see an ROI in Greenland.

We applied the CP analysis for the compete LRM zone in Greenland as well as for the ROI in North Greenland, which was used in figure 14. However, as expected the Greenland results (see below) of the CP error show very similar results as Figures 8 and 9. We selected representative areas in Antarctica for the CP analysis over flat and complex topography. If needed we would suggest placing this requested additional figure in the supplements.

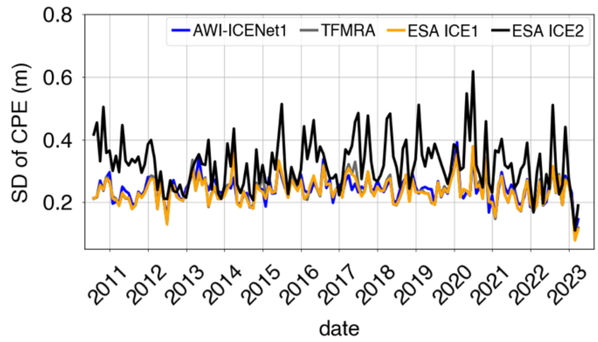


Figure 1: CP error analysis a region in North Greenland

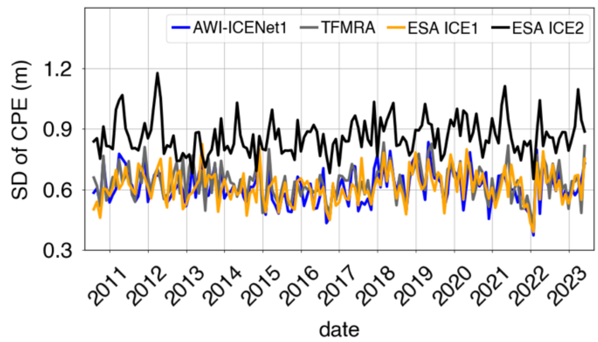


Figure 2: CP analysis fort the LRM zone in Greenland

F7: thank you for this very convincing figure. How does this look with respect to slope?

Thanks for this suggestion. We have to think about how this can be accomplished to be meaningful. When we plot with respect to slope the influence of attenuation is dominating. We agree that a sloped surface will also slightly widen the leading edge. I guess this is what you want to see in such a figure. A suggestion would be to select a couple of attenuation bands and use those in the suggested figure.

L265: Roemer is a better solution however the LEPTA relocation seems to improve even further, how does this affect the results?

This is a good question. However, it is not within the scope of this paper as we focus on the improved retracking methodology. As we apply the same slope correction to all of the different retracked geolocated elevation points we avoid any influence of slope correction at least in the intra mission analysis. For the comparison to ICESat-2 we agree that the LEPTA slope correction might improve the product even more but this is out of the scope if this paper. We will add a sentence to mention this additional option to further improve the elevation change product.

L268: monthly crossovers are a very long time span for cross-overs on ice sheets please add a lower time constraining on the timing between orbital crossing evaluated.

Thanks for this suggestion, but we don't agree. The area covered with the LRM mode is the plateau of the Antarctic Ice sheet. Maximum elevation changes to be expected are less than +/- 1m/yr. This results in <0.08m elevation difference in a month. As the observed standard deviation is approx. 0.5m we think that uncertainties due to elevation change are not affecting the overall performance. Furthermore, if we only consider cross overs in < 10d we would considerably reduce the number of cross over points reducing the spatial coverage even more.

Exemplary, we run the CP error analysis with <31d, <10d and <5d conditions and found the following:

	<31d	**<10d**	**<5d**
Median CP error (m)	**0.003**	**0.01**	**0.01**
SD CP error (m)	**0.45**	**0.46**	**0.46**
valid cross points	**126491**	**62902**	**33173**
filtered cross points:	**90 (0.07%)**	**46 (0.07%)**	**27 (0.08%)**

Based on this evaluation we will stick with the old approach as it gives a better spatial coverage and thus also considers areas at lower latitudes.

L270: how many fall within this outlier filtering (maybe in %)

Usually less than 0.1%. Please see table above. Outlier removal is also dependent on the chosen retracker.

F8-F10: The ESA ICE2 seems as an outlier compared to the others, suggest removing this from the plots to see the specific difference in the others.

Thanks for this suggestion but as we consider all 4 retracker in the paper we would like to stick with the figures. We also show ESA ICE2 in all the remaining figures and tables and don't want to remove this comparison, as we think it is important to show how large the effect of retracker can be for higher level products.

L423: "…focus on the time from January 2019 to December 2021…"

Thanks for spotting this. Will be changed .

F20: Is the trackiness due to errors in the CS2 or ICESat-2 data?

The plot shows the standard deviation of the h anomaly for ICESat-2 only.

L472: Guess this is ATL15 this should be mentioned.

No this is not the case. We apply our dhdt processing to the ATL06.006 point cloud data product to avoid differences in the processing.