Revisions According to Referee Comments (Round 2)

Assessment of Sentinel-3 Altimeter Performance over Antarctica using High Resolution Digital Elevation Models
(https://doi.org/10.5194/egusphere-2024-3054)

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We thank the reviewers for their constructive comments and suggestions. Our responses are provided inline beneath each referee's comments and are clearly [highlighted and bracketed] for ease of reference.

Reviewer 1

General comments:

The updated manuscript offers notable improvement to the readability and understanding, especially the additions to the sections on range window placement and the capture of the Point of Closest Approach, and the separation of roughness from slope yields interesting and useful results for current and future missions.

Given the central role of the footprint concept in the analysis, it is important that the paper provides very clear and consistent definitions to avoid ambiguity, which is currently lacking. There are examples in the specific comments. In addition, would it be possible to give some metrics in meters of the real observed footprint? The literature is generally missing a reference to this.

[Thank you for your comment. We agree the description of the footprint characteristics was not always clear and unambiguous, and so have therefore improved it in line with the line-specific comments. Specifically, we have referred to the definition and characterisation given in Donlon et al., 2012a, which to our knowledge gives the most authoritative description of the instrument characteristics. We nonetheless recognise that the definition of the real 'observed' footprint for any given acquisition is more nuanced than the high-level instrument characterisation provided by Donlon et al., 2012a, due to the impact of a number of factors, including satellite altitude, surface roughness, and surface slope. We believe that it is beyond the scope of the current study – which is already quite long – to undertake a detailed analysis of these additional elements; however, in line with the reviewer's comment we have modified the text to ensure that these dependencies are more clearly stated.]

The results section has become quite long, as it incorporates the discussion. This structure can make it challenging to maintain focus; separating results from discussion could improve clarity, or including summarizing tables along the way may help provide an overview - though this may simply be a matter of style preference

[Thank you for highlighting this. We agree that the result section is lengthy, reflecting the range and depth of analyses undertaken. After some consideration, we believe that it makes sense to keep the results and discussion as one, so that each distinct theme of analysis is presented together, and to avoid the reader having to jump back and forth between different sections of the manuscript. Nonetheless, we acknowledge the reviewer's underlying point, and so to address this, we have introduced clearer sub-headings and breaks between the different sections, in order to make it easier for the reader to navigate the results and discussions as a whole.]

Specific comments:

- L154: "In areas with sloped terrain (>0.5 ◦), improvements reached up to 83% and 90%." Sentence seems incomplete, is it median and MAD? or between 83-90%

[Thank you for pointing this out. This is in reference to the median bias and median absolute deviation with respect to IS2 ATL06 in the prior sentence. We will make this clearer in the text.]

- L194-196: I am a little puzzled by the footprint description. For a SAR altimeter, the along-track resolution (~300 m) is set by the synthetic beam pattern from SAR focusing. The ~18.2 km you quote (from Donlon et al., 2012) corresponds to the physical antenna footprint, i.e., the beam-limited footprint from the antenna pattern, and not the SAR processed resolution. It is the maximum illumination width across-track within which SAR processing operates, effectively the swath width. I think it would be clearer to state the 18.2 km as the theoretical beam-limited width from the antenna pattern at 3 dB. Furthermore, the across-track (pulse-limited) length of 1.6~2km. I do not find this in Donlon et al. 2012a.

[Thank you for raising this point. We agree that the footprint description was confusing and have revised this section to provide a clearer explanation, specifically in relation to along-track doppler footprint achieved by the SAR focusing, and the across-track antenna beam footprint. Regarding the 1.6-2 km range stated for the pulse-limited across-track resolution, the 2 km values was in reference to Table 6 in Donlon 2012a: “SAR across track resolution - >2 km depending on Hs (Ku band)”. The 1.6 km value was obtained from https://sentiwiki.copernicus.eu/web/s3-altimetry-instruments Table 1, with the reference accidently omitted. We will add this citation and also make clear that it represents a lower bound (as per Donlon 2012a).]

- L217: Please add the abbreviation BC to the Baseline Collection as you use it later

[Thanks for pointing this out. We will add this abbreviation.]

- L259-264: You should look at Scanlan et al. (2023) https://doi.org/10.1029/2022GL101702. They presented a new approach for characterizing the monthly variability in surface roughness across the Greenland icesheet via the strength of radar altimetry surface echoes. Their approach is based on the Radar Statistical Reconnaissance method.

[Thank you for pointing out this reference. We were aware of their new approach and its potential; however, we understand that it aims to characterise wavelength scale roughness, rather than the much larger roughness length-scales that we are interested in within this study. We have now added this reference, alongside relevant discussion clarifying this distinction, within the revised text.

- L358: You refer to the 3 dB bandwidth, but I do not remember you mentioning this before. You are introducing simple altimetry, then the 3 dB bandwidth should also be explained. I may have missed it, but I do not find an explanation of the ~36 km extended across-track footprint. Why 36 km?

[Thank you for the comment and for highlighting this omission. As requested, we will provide an improved description of the 3 dB antenna footprint earlier in the text, in-line with the prior comment on improving the footprint description, and we will also summarise the concept again at this current point in the text. Regarding the second part of the comment, we have also added additional text to explain our use of the extended ~36 km across-track region. Specifically, this is done to allow us to identify instances where the POCA lies outside, but proximal, to the 3 dB beamwidth (i.e. there will still be some limited sensitivity to backscattering from this location). The precise choice of 36 km is somewhat arbitrary, but is chosen to reflect approximately double the 3 dB beamwidth.

- L390-435: You refer to the ~15 km along-track distance. Where does it come from? You could describe it when you describe the footprints in the data section

[Thank you for highlighting this. The ∼15 km along-track distance corresponds to 50 consecutive waveforms at the Sentinel-3 along-track posting of ∼300 m. The precise choice of distance is somewhat arbitrary, but was chosen here to provide a representative scale for evaluating waveform decorrelation over distances relevant to the common aggregation of altimetry measurements (order of ~10 km), for example in the processing of rates of surface elevation change. We note that Reviewer 2 raised a related point regarding the choice of aggregation length, and so we have clarified in the revised text both the origin of this distance and the practical rationale for its selection.]

- L478-480: For the lacy reader, please explain (again) why a better correlation for TRI gives a better slope-roughness correlation as TRI likely does not deviate from slope and roughness...

[Thank you for raising this point. We agree that this deserves clarification for readers. TRI-derived roughness estimates are artificially correlated with slope because the method does not explicitly remove the slope component - even a perfectly smooth inclined plane yields non-zero TRI roughness estimates that increase with surface gradient. By computing the Pearson correlation coefficient between slope and roughness for both methods, we demonstrate that TRI exhibits substantially higher correlation (0.947) compared to our approach (0.720). The lower correlation in our method indicates successful reduction of this artificially introduced algorithmic correlation, with the remaining correlation reflecting genuine physical relationships whereby steeper terrain tends to exhibit greater surface variability orthogonal to the surface slope. We have therefore revised the text to make this distinction clearer.]

- L509: double "is"

[Thank you for pointing this out. We have removed the additional “is”.]

- L614-619: What does this mean in kilometers? Does this change the way we define the size of the actual S3 footprint?

[Thank you for this question. We have clarified the text to specify that the 3 dB beamwidth corresponds to +/-9.1 km across-track from nadir, and that our REMA-based approach uses an extended 36 km across-track search diameter without imposing the 3 dB restriction applied in L2 processing. This analysis does not redefine the S3 footprint size. The 3 dB beamwidth remains the physically meaningful boundary for effective antenna illumination. However, our results demonstrate that in topographically complex terrain, the geometric point of closest approach can lie beyond this boundary.]

Reviewer 2

General notes:

I think this manuscript has been substantially improved and can be published once a few concerns have been addressed.

In general, the manuscript is well written but quite long, with a strong focus on the derivation of surface slope and roughness, as well as the along- track correlation. In my view, these are not the most interesting or important aspects of the study; the key contributions are found in Sections 6.2 and 6.3. I suggest shortening the sections on slope/roughness and along-track analyses, and placing greater emphasis on window optimization and POCA analysis.

[Thank you for this feedback. We are pleased that you find the revised manuscript to be well-written, and we acknowledge that it is quite long given the range of analyses presented. Despite this, our preference is to largely maintain the current extent of the slope/roughness and along-track correlation sections in its present form, as we believe they will be of interest to a broad range of The Cryosphere readership, beyond solely the altimetry expert. More specifically, our motivation for keeping this analysis is firstly that we believe that our new slope and roughness derivation represents a genuine methodological advance compared to conventional approaches, due both to its capacity to better separate slope and roughness in an algorithmically robust manner, and its more generalised form which is able to operate over variably-sized windows; as such, we feel that it is important to provide a complete description and comparison to other methods. We note also that a previous reviewer specifically requested comparison to established methods such as Horn's approach, suggesting there is genuine interest from the community in having this information. Secondly, our along-track correlation analysis provides new quantitative evidence to support the conceptual understanding of the relationship between topography and waveform variability, at ice-sheet scale. Although less central to the altimetry performance assessment, we believe that this information has value for researchers undertaking similar work, and illustrates how new high-resolution DEM's can be used to further our understanding of altimetry performance, beyond simple high-level evaluation of elevation statistics. Nonetheless, we do agree with the reviewer that Sections 6.2 and 6.3 contain arguably the most important contributions, and so to better highlight these key findings, we have adjusted the conclusion section accordingly, as per the responses given for the line-by-line comments.]

Additionally, there is considerable back- and-forth between the BC-004 and BC-005 products. The focus should be on the latest version, as the older one is now redundant. This would also help streamline and clarify the manuscript.

[Thank you for this comment. As suggested, we have aimed to put the primary focus on BC-005. Nonetheless, alongside this we do believe that it is still important to retain the BC-004 comparisons, because they inform our understanding of the reasons for the observed behaviour. More specifically, they demonstrate - in a way that cannot be determined from analysis of BC-005 alone - what the performance impact of specific algorithm changes (extended window processing, waveform centering) is over varying surfaces. This provides actionable information for understanding how processing changes affect retrieval quality, which can be used directly to inform the processor design for future missions such as CRISTAL. We nonetheless recognise that the rationale for these comparisons was not sufficiently clear. We have therefore added additional text to better explain why these comparisons are valuable.]

Nonetheless, I acknowledge that the paper is currently mature enough, and these suggestions are meant to help further refine the focus on the most important findings.

[Thank you for acknowledging this, and we are grateful for the time you have spent reviewing our manuscript.]

Line by line comments:

- L40: "Subsequently, Sentinel-3 (S3), which to date comprises two satellites launched in 2016 and 2018, ushered in an era of operational monitoring and global Synthetic Aperture Radar (SAR) coverage, thus representing another significant milestone in the historical progression of satellite altimetry (Abdalla et al., 2021)" I think you have missed CryoSat- 2 here as it lead the way for the operational use of SAR and SARIn. You should add an extra sentence of how CS2 has pawed the way for S3 in the form of technology development, especially as you discuss ERS and Enivsat more than CS2. CS2 is just mentioned in passing, so add what CS2 have done for ice sheet application with a sentence or two.

[Thank you for this comment. We entirely agree about the importance of CryoSat-2, and have revised the text to highlight CryoSat-2's pioneering role in SAR and SARIn processing, noting the improvements these innovations brought to ice sheet observations (SAR: ~300 m along-track resolution; SARIn: ability to improve measurement accuracy in regions of complex topography). We have also clarified how CS2's technological advances paved the way for Sentinel-3's operational SAR implementation].

- L46: "the failure of assumptions used in Level-2 processing under such conditions, and the limitations of the instrument's spatial resolution" — The assumptions and limitations should be stated explicitly for the reader.

[Thank you for raising this point. We agree and have added explicit statements of the key assumptions in conventional L2 processing and how they break down over complex topography. Specifically, we now state that L2 processing assumes (1) the point of closest approach can be correctly identified, and lies within both the range window and beam-limited footprint, and (2) the identified waveform leading edge corresponds to backscattered energy from this closest point. We also explain how steep slopes and high surface roughness can violate these assumptions.]

- L69: "due to numerous discrete regions of backscatter within the illuminated beam- limited footprint" — I would rephrase this as "due to differences in elevation within the illuminated pattern, as each peak in the waveform corresponds to a scattering surface at a different elevation."

[Thank you for the comment. We have revised the text to more clearly explain that waveform complexity arises from backscattered energy originating from discrete regions at different elevations within the footprint, which contribute to the overall waveform structure.]

- L103: The references (Villadsen et al., 2016; Passaro et al., 2022) are somewhat unusual here, as they relate to inland water and ocean processing. Adding ice-sheet-focused references would improve clarity, for example: Aublanc, J., Boy, F., Borde, F., & Féménias, P. (2024). A facet based numerical model to retrieve ice sheet topography from Sentinel-3 altimetry. EGUsphere.

[Thank you for this suggestion. We have added the suggested ice-sheet-focused reference (Aublanc et al., 2024a) alongside the existing references, as the general categorisation of physical versus empirical retrackers applies across different surface types (inland water, ocean, and ice sheets). The Aublanc et al. reference provides valuable ice-sheet-specific context while the existing references support the broader methodological framework.]

- L122: "the magnitude of this slope correction can reach tens of meters vertically" — It can actually be much larger. I suggest noting that it can exceed 100 m for a 1° surface slope.

[Thank you for this correction. We have revised the text to note that the slope correction can exceed 100 m for surface slopes of 1° or greater, which better reflects the magnitude of corrections required over steeper terrain.]

- L124: I would also add the following reference: Schröder et al. (2017). Validation of satellite altimetry by kinematic GNSS in central East Antarctica, The Cryosphere.

[Thank you for suggesting this reference. We have added Schröder et al. (2017) to support the statement regarding slope correction uncertainties.]

- L195: "which operates with a delay-Doppler beam-limited footprint of ~300 m along- track by ~18.2 km across-track..." — This sentence is unclear. The 300 m along-track dimension is not beam-limited, and it's ambiguous whether 18.2 km is a radius or diameter. Consider rewriting along the lines of: "Sentinel-3 provides ~300 m along-track resolution through delay–Doppler processing, with a pulse-limited along-track footprint of XXX km and an overall beam-limited footprint of XXX."

[Thank you for this suggestion. We agree that the original footprint description was unclear. Following feedback from Reviewer #1 regarding the distinction between beam-limited and pulse-limited footprints, alongside your suggestion to lead with the achieved resolution, we have restructured this section. The revised text now leads with the achieved resolution (~300 m along-track through SAR processing, ~1.6-2 km pulse-limited across-track) before describing the overall beam-limited footprint dimensions, which we hope should provide greater clarity.]

- L303: Add a reference to the figure and associated table here, if they exist.

[Thank you for this comment. We have added a reference to Fig. 4 where these comparisons are presented.]

- L397: The purpose of the aggregation step is unclear. A decorrelation length is relevant for multilooking because it directly effects waveform formation and retracking. However, elevation-change products are typically generated or compared at scales of ~1 km. Are you aiming to quantify how slope or roughness influences correlation in elevation-change estimates?

[Thank you for your observation. As you correctly suggest, the purpose of this analysis is not to investigate decorrelation within the frame of multilooking, but rather within the scope of estimating surface elevation change. In this regard, elevation-change (dh/dt) estimates often combine elevation measurements that are derived from waveforms acquired over length scales of the order of 10 km. Thus we chose 15 km as indicative of this length scale. The ~15 km along-track distance corresponds to 50 consecutive waveforms at the Sentinel-3 along-track posting of ~300 m. The precise choice of distance is somewhat arbitrary, but was chosen here to provide a representative scale for evaluating waveform decorrelation over distances relevant to the common aggregation of altimetry measurements. Whilst it would be possible to compute decorrelation over different distances, our aim here was to provide an initial assessment at a reasonable scale rather than an exhaustive analysis. We note that Reviewer 1 raised a related question regarding the origin of the 15 km distance; the revised text now clearly explains both the derivation of this distance and its relevance to assessing topography-driven waveform decorrelation.]

- L441–445: This section could be shortened, as the information can be readily estimated by most algorithms and does not appear to represent a major new result.

[Thank you for this comment. As requested, we have condensed this section.]

- L552: "The most substantial improvement occurs between 60 m and 120 m, with diminishing returns at larger window sizes." — This is an important conclusion and could benefit from more detailed discussion. This seems more impactful than the analysis of heading, which does not appear to be a major factor. Additionally, since BC-004 is now outdated, the focus should likely shift to BC-005, with the BC-004 analysis minimized.

[Thank you for highlighting this. We agree this is an important finding and have expanded the discussion to better emphasise its implications for mission design. Regarding the heading analysis, we retain this brief assessment as it directly addresses a potential systematic bias in coastal acquisitions that could affect window placement - ruling out this factor strengthens confidence in our other findings. Regarding BC-004, as noted in our response to the earlier comment, we believe that this comparison is informative, because it demonstrates how specific algorithmic changes impact performance, providing valuable context for future mission development.]

- L670: It is good that new algorithms were tested for deriving slope and roughness, but the manuscript states that they produce results similar to standard approaches. For that reason, this does not seem to be a major conclusion of the study. The more significant findings relate to window optimization and POCA capture.

[Thank you for this. We agree that window optimisation and POCA capture are the primary contributions of this study, and we have therefore condensed this section of the conclusions to focus more on these elements.]

- L686: This is an interesting result, but it is not discussed in sufficient depth in the results section. More detail would add value, potentially replacing some of the extensive slope/roughness analysis.

[Thank you for highlighting this. We agree that the range window and POCA capture analysis is a key result and, as suggested, we have therefore expanded the results and discussion to provide additional quantitative detail. This includes discussion of the variability in capture performance, and the implications for adequately observing complex terrain and for future mission design. In response to this and an earlier comment, we have also condensed the text relating to slope and roughness.]

- L699: This may not be a new insight — it has long been understood that decorrelation increases over more complex surfaces.

[Thank you for this comment. We agree that the conceptual relationship between topography and decorrelation is well established. However, we believe that quantifying this relationship at ice-sheet scale through the combination of altimetry and DEM datasets adds value by providing specific metrics (e.g., correlation coefficients, decorrelation rates) that were previously lacking and, as such, support existing understanding with new empirical evidence. In accordance with this, we have therefore revised the text to clarify that our contribution is quantification rather than conceptual novelty.]