

Interactive comment on “Improved processing and calibration of the interferometric mode of the CryoSat radar altimeter allows height measurements of supraglacial lakes in west Greenland” by Laurence Gray et al.

Anonymous Referee #2

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The manuscript by Gray et al., provides an analysis of CryoSat SARIn mode for the study of surface elevation over the Devon Ice cap and over a sector of the West Greenland Ice Sheet. The manuscript describes an improved calibration of the satellite's attitude and a fine tuning of the signal characteristic to obtain precise height measurements. There is then a detailed and informative discussion about the signal properties observed above supra-glacial lakes and of supra-glacial lakes elevation determination during a 6-year period.

The paper provides rigorous, thorough, novel and relevant observations of CryoSat's

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performances over ice caps and ice sheets. My comments address the clarity of the manuscript that I think would benefit from improving on the following points:

The paper should make it clearer where the improvements in processing are (title) compared to previous work. As written only the improvement on calibration is explicit.

The paper should make it clearer when the roll-biased corrected heights have been used to generate bias and dispersion from airborne dataset.

Through the paper there is variability in the tuning of the various technical steps (e.g. processing's thresholds, filtering and binning) for different height products and area that often seems arbitrary. It would be good to have more details on the observations that led the authors to tune the processing the way they did so as to educate the reader.

The link between the calibration and the supra-glacial lake survey is not particularly explicit, the two parts are somewhat disjointed. What observation over the lakes led you to develop this improved calibration? I would like to see more on relating the impact of the better calibration with the gain in precision in the lake height measurement maybe via a dedicated section or by a better articulation of the two sections.

Here are detailed comments related to specific page and line numbers:

Title: CryoSat-2? ESA prefers this appellation.

Title: It is relatively clear that the paper provides an improved calibration however the improvement in processing upon previous work is not clear.

P1L15-16: How did you come up with these numbers? I could not find them in the manuscript.

P1L23: If I understood correctly this value of 0.5m is obtained from a consistency check between ascending and descending orbit path of CryoSat data, which can both be affected by a systematic bias, and not from validation with an auxiliary dataset. If so then I would rephrase this sentence and use a different word than accuracy.

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P2L9: 'Point-Of-Closest-Approach'

P2L17: This is another nice piece of work on the subject:
<http://ieeexplore.ieee.org/document/7542661/>

P2L20: and ice sheet margins

P2L21: not sure what 'borrows heavily' means?

P3L12: Hawley et al., GRL 2009 maybe also for a bit of history

P3L13: With respect to use of L1b product and generation of swath altimetry, it would be worth mentioning some recent work generating and applying swath altimetry to derive geophysical variables over ice sheets: Christie et al., GRL, 2016 (10.1002/2016GL068972), supra-glacial lakes: Ignéczi et al., GRL, 2016, (in the sup. mat. 10.1002/2016GL070338), Ice Caps: Foresta et al., GRL, 2016 (10.1002/2016GL071485)

P3L26: This sentence "Our method in working ..." is awkward.

P4L12: What reference power is used to calculate the logarithmic values?

P4L10: Not sure to what this last sentence refers to.

P5Section2.2: What criteria do the authors used to identify swath returns from POCA returns? There seems to be a process by which these two records are identified within a waveform but the methodology/criteria to achieve this are not described.

P5L14: What distance is this? In the ground plane? Does the surface slope matters and how does that value varies with the slope? It is not clear how this values relates to the 4-bin filter described above.

P5L12-15: The binning step is unclear? As written it reads as if it is a consequence of the filtering, is it? Why do both? This section, and the need for binning/filtering, needs more justification.

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P5L18: What are the gains of using this alternative setup for summer data? It would be good to describe further the motivation/justifications behind this customised processing.

P5L20: In Gray et al., 2013, the range of acceptable cross-track slopes for swath processing is between 0.5 and 20. Are the authors revising this range? If so it would be interesting to have a paragraph or so discussing this.

P5Section2.3: What constrained was applied on the time difference between CS+ and validation data?

P8L16: fig. 5 is mentioned before fig. 4

P9L15: How was 0.0075o determined from the data in fig. 4/5? What is the uncertainty attached to this value?

P11L4: Is this after the bias correction is applied? How does this changes with a bias of 0.0075o applied?

P11L8: Why “appears”? Could this be checked?

P12 L14-18: A few references would be helpful in this paragraph.

P12L23: A sentence or 2 on the use of surface topography to map and model supra-glacial lakes is warranted here: e.g. GIMPDEM: Leeson et al., 2015 doi:10.1038/nclimate2463, and GIMPDEM and CryoSat-Swath DEM: Ignéczi et al., GRL, 2016 10.1002/2016GL070338

P13 L6 ‘for the six ascending passes’. Is that seven passes?

P13L15: The amplitude of the decrease is as large as the increase in both L1 and L2, albeit with a different timing - it seems therefore to be a significant signal. Could you expand on the relative differences and on the reason behind this signal? Especially since drainage as a cause is excluded.

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P13L23: Sentence starting by “If we assume . . .” seems incomplete.

P13L31: Is it an advantage of swath or is it because of your choice to limit the swath data to small look angles (P5L20)?

P14L13: I would soften/rephrase this statement; Foresta et al., GRL, 2016 show that there are no significant differences between rates of height change from POCA and from Swath over Icelandic ice caps. Second the greater spatial coverage offered by swath can lead to measure of height change where POCA fails and so provide a ‘better’ solution (Smith et al, TC, 2016). Finally it depends on the surface slope (direction and magnitude). I would rephrase the paragraph using the studies of height change over Iceland and Thwaites to show existing evidences for the benefit of swath for height change measurements, and listing the various associated caveats.

P15Section6: Is a bullet-point conclusion appropriate for TC conclusions? I leave this to the editor to decide.

P15L16-17: Where did the paper demonstrate the relative accuracy?

Fig3. Transect in fig. 1 seems to be missing

Fig8. Specify time-period and/or CS2 passes used

Fig9. Same as for Figure 8

Fig10 '(A) 16 passes plotted. . .' There is only one plot on panel A. Delete '(top)' at the end of the first sentence since the panels are labelled A and B.

Fig15. Add latitude on y axis and possibly range window on x axes

Fig17. Units (m) are missing on both x and y axes

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