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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 #3

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The manuscript of Gray et al. discusses improvements in POCA and swath processing of Cryosat-2 data. Comparing the satellite data to elevations from airborne surveys, GPS transects and DEMs, they show that their approach of using the point of inflexion generally provides better results than the standard ESA POCA data for two regions. Further improvements are obtained by introducing an additional roll angle correction. Whereas the improved performance of slope-dependent and threshold retrackers has been discussed before in other studies (including some of the first author), the findings about the roll angle correction are important. The manuscript is well written and the presented analysis is meticulous and thorough and illustrated with appropriate il-

lustrations, which make the paper accessible for non-radar specialist (although some background knowledge is still required). The application to lake height changes makes it interesting for the broader glaciological audience that The Cryosphere targets.

As one of the other reviewers also pointed out, section 4 feels a bit disjointed. It would be good to discuss if and how the improvements discussed in the other sections contributed to these results. Could these results be obtained with the standard data provided by ESA? The only weak part I could identify in the manuscript is section 4.1 on the effect of surface melt on SARIn waveforms. Although the reasoning is logical, this part is rather speculative ('We speculate. . .' appears 3 times) and doesn't add much to the paper. I would suggest to keep this part for a separate paper, backed up with in-situ/model data of the local snowpack characteristics (possibly at a different location where such data are available).

Most textual comments have already been raised by the other two reviewers. Below a list of additional suggestions:

P3L7: '[L2i contains] also the waveform': is this so? As far as I'm aware, the L2i data contains some waveform parameters (leading edge slope, max wavepower, etc.) but not the full waveform?

P5L14: 'binning and averaging the results in segments' : please clarify how you did the binning and averaging (e.g., the width of the segments).

P6L17: It's unclear to me how you estimated the error by looking at the cross-track slope. Or did you inspect the cross-track slope to remove bad measurements?

P7L5: I suggest to use a symbol for the phase. The ph might be interpreted as p times h.

P7EQ4: is this equation correct? Shouldn't it be -ph/KB + ph/KB(. . .)-. . .

P8L17: please provide separate numbers for the bias for ascending and descending passes

P8L30: what's the range of the sun elevation angle for the 2012 ascending passes?

P10L9-15: figure 8a (ESA L2) is based on 951 pairs, fig 8B (max slope retracker only 599). I assume that during the processing of the max slope retracker data, faulty data was already (partly) removed? For a fair comparison, numbers for the same pairs should be compared. NB: in figure 9A, there are suddenly 1608 pairs for the POCA data. Where does this difference come from?

P15L9: the choice for the 0.0075 deg bias is poorly motivated.

P10L28: two of the passes in fig 10B show a minimum std at $\sim$0.015 deg. Any idea why this is?

P13L25: SARIn data certainly has advantages, but it doesn't allow to estimate total lake volume as with Landsat/MODIS. Seems correct to point out this limitation.

P14L10: 'POCA better suited for temporal height changes': Forresta et al, GRL, 2106 recently estimated volume changes for Iceland using swath data and found that swath leads to more accurate results than POCA data. It would be good to briefly discuss how uncertainties in the swath data affect such estimates.

P15L6: a 73-day repeat would lead to a larger inter-groundtrack separation, i.e. a less dense coverage.

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