

## ***Interactive comment on “Elevation and elevation change of Greenland and Antarctica derived from CryoSat-2” by V. Helm et al.***

### **Anonymous Referee #3**

Received and published: 21 May 2014

CryoSat-2 has been operational for 4 years now, and it is about time that the data get applied over the ice sheets at a larger scale. The authors have done an impressive job in putting together both DEMs,  $dh/dt$  grids and uncertainty grids over both ice sheets, but the manuscript seems to have been written in a hurry and both the structure and language should be improved. I have made some suggestions and edits below, but I think that a native English speaker also needs to go through it. The study focuses on volume change rather than mass balance, which I think is smart given the high uncertainty of any firn corrections over such a short time span (2011–2012). The results are carefully validated with independent DEMs and elevation (change) data, but I miss a brief general discussion of potential error sources like time-variable backscatter and pitch/roll biases (Galim et al. 2013/14, IEEE). Are they sufficiently corrected or assumed negligible? And what are the domains of the  $dh/dt$  study areas (ice masks)? Do you

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include peripheral glaciers or not? That can make a significant difference to the results (Gardner et al. 2013, Science).

As mentioned, the structure of the paper is confusing. The data and method descriptions come partly in the introduction, partly in the results and partly in the appendix. I suggest to put all these details into a separate data/methods section. After all this is one of the earlier papers that uses CryoSat-2 over the ice sheets, so many readers might actually be more interested in that part than the results themselves. The appendix is so short that it would easily fit in the main manuscript. It is also somewhat difficult to interpret the results before having read the full methodology and uncertainty assumptions.

Below are some smaller comments and suggestions that I have in chronological order:

Abstract: Some general material (and references) can be skipped to make it shorter and more focused (currently > 350 words, check the journal requirements). The language can also be improved to make it easier readable.

P1674, L7: “mass balance” is not presented, or?

P1674, L8: Shouldn't the DEMs be mentioned first since they are a prerequisite for the elevation change calculations?

P1674, L27: volume loss -> volume change (if thenegative sign is to be used)

P1675, L1-14: Not sure if you need to justify why we need DEMs. I would argue that the altimetry data themselves are more suitable for mass balance applications than the InSAR velocities that you want to improve, although also important for many purposes.

P1675, L18: “the loss of Envisat only”? I don't understand.

P1675, L24: Diameter?

P1675, L28-29: Be clear: “SAR mode is used..., whereas SARIn mode measures surface elevation in the steeper...”. A technical reference to Wingham et al. or similar

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would be appropriate somewhere in this paragraph.

P1676, L9: Reference the supplementary material for more details here, or move that to the main text.

P1676, L5-19: This “introduction” is essentially data description with a touch of methodology, and then the rest comes in the appendix. As suggested earlier, I would rather move everything into a separate data/methods section. It is also somewhat difficult to understand what’s in the ESA product, and what the authors have done themselves. Using “we” instead of the passive voice would make this much clearer. And please consider if your combined descriptions are sufficient for others to reproduce your results.

P1676, L19: Unclear what “search radii” mean here.

P1676, L25: Sudden change of tense.

P1676, L28: How variable is the real dt?

P1676, L29: Please explain briefly how this “block median” works. One should be careful with median dh/dt averaging over large distances because its typical statistical distribution is skewed towards negative values, causing a positive bias in median dh/dt.

P1686, L22: What does “Baseline B data” actually mean?

P1687, L7-9: Unclear sentence.

P1689, L2: Do you mean variance or standard deviation?  $V = SD^2$

P1689, L6: The 6 km radius needs to be mentioned in section A2 as well.

P1689, L15-16: “Finally” after the “final step”.

P1690, L10: Did Ewert et al. apply the G-C correction before estimating inter-campaign offsets? Otherwise, it might be double-corrected in your data (implicit part of IC-offsets).

P1690, L3: Baseline A data have not been defined earlier. Please do.

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P1690, L16: “re-tided LRM”? According to Fig. 14, tide corr. Are only applied to SARIn.

P1690, L23: Any minimum or maximum requirements for the resulting dt? In theory, they could be just months off. Some statistics on dt or the number of points that are typically found within the 1 km search distance would be good.

P1691, L2: Isn’t this done before calculating the actual dh/dt? If so, mention it before as well. Also specify which DEM you use for topographic correction (your own I assume).

P1691, L5: Clarify: “. . .averaged for each 1 km cluster of data” (or something similar)

P1691, L16-21: These equations govern the dh/dt calculation and should hence be in the previous section where the same concept is explained in words.

P1690, L15: low accuracy

P1690, L13-19: Fits better in the introduction as a justification for your study.

P1677, L20: “We derived three. . .”. As I understand it, this is DEM methodology that comes in addition to the Introduction and Appendix. Again, I would put it all together, especially since the appendix mentions 1/6 km but not 10/50 and 50/250 km like here.

P1678, L17: Logarithmic? The figure says nothing about that.

P1678, L27: decimetre-accurate

L1681, L25: I assume the single point accuracy refers to the vertical (10-15 cm) and repeat-track accuracy to the track separation (typically within a few hundred meters over AIS and GIS, not just 2 km). Explain better.

P1682, L1: delete “always”

P1682, L1-12: Fits better in the Introduction as a motivation for your study.

P1682, L15-18: Fits better in the methodology.

P1682, L25: Restructure sentence.

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P1683, L8: “deep into the ice sheet interior”.

P1683, L17: delete “also” (ort what does it refer to?)

P1684, L26: Locally or generally? See also comment at P1676, L29 about potential biases resulting from large-scale median averaging.

P1685, L6: Same issue as previous comment. Could it be that the rapid thinning along the margins is systematically underestimated? If so, how much? Please comment on this and justify why such a strong spatial averaging is needed (here or in the methodology).

P1685: The content of the Conclusions is fine, but the readability needs to be improved.

Table 6: Two of the columns are labelled “Bias (m)”. Confusing.

Fig. 6, caption: “. . .binned elevation differences at crossover points with respect to . . .”

Fig. 9: I cannot see any black ICESat points (unlike in Fig. 10).

Fig. 10: Why are there fewer CryoSat dh/dt points at the highest elevations? I would expect the opposite.

Fig. 11, caption: “The threshold first maxima re-tracker (TFRMRA) gives the best results”.

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Interactive comment on The Cryosphere Discuss., 8, 1673, 2014.

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