

Interactive comment on “Improved retrieval of land ice topography from CryoSat-2 data and its impact for volume change estimation of the Greenland Ice Sheet” by J. Nilsson et al.

L. Schröder (Referee)

ludwig.schroeder@tu-dresden.de

Received and published: 28 July 2016

This paper is an important step towards the full exploitation of the abilities of the improved altimeter design of CryoSat-2. Even when working with the already out-dated dataset Baseline-B, the authors show how their own reprocessing steps can generate a dataset of outstanding quality. Three different methods were applied to determine the elevation change of the Greenland ice sheet. The high coincidence of the results, especially compared to the ESA product, points out how their processing chain helped to improve the accuracy and precision of our knowledge about the reaction of this ice sheet to climate change.

One main outcome of this study (as already of Davis et. al, 1997) is the predominance
C1

concerning accuracy and precision of low threshold retrackerers compared to functional fits (which have other benefits). In the paper it was more than once mentioned that this retracker is not so much affected by changes in the snow pack properties compared to ESAs retracker. However a clear evidence for that is missing. In the paper about the Greenland melt event 2012 (Nilsson et al., 2015) it was explained that at the NEEM site an elevation decrease of 13 cm was observed in July 2012 but Fig. 1c there shows that the CryoSat time series indicates an uplift of about 60 cm. To proof the statements about the sensitivity of the retracker a similar plot with the new retracker could be very helpful.

Another point I would suggest is more clarity concerning the dependency with surface slope. At line 342ff you explain how you estimated the sensitivity of the height residuals to surface slope. This rate is said to be an indication of the magnitude of the slope induced error. To quantify the effect you try to fit a slope-dependent component to the residuals. However as it can be seen from Fig. 1 especially for the elevations (a and c), the dependency with the slope is twofold. Firstly as is can be easily expected the standard deviation increases with slope. Secondly there is an positive trend in the offset (=mean difference) which is quantified by your trend fit and called “residual slope error”. I would suggest rather to consider the combination of both effects as “residual slope error” or to keep the effects separated but call this component something like “slope-dependent offset” and the increasing standard deviation “slope-dependent error”. In this case (which I would prefer) it would be also very interesting to analyze the increasing standard deviation for the different datasets too (Tab. 1).

Besides I have some minor remarks and suggestions:

l.12: Expr: This when compared

l.41: add brackets around “(e.g. Gardner et al., 2013; Shepherd et al., 2012)”

l.44: Expr: “the characteristics of which is”

I.60: Expr: "methods from improving"

I.77: I think 1° is no "low sloping terrain" anymore for radar altimetry. The switch to SARIn happens already at lower slopes ($\sim 0.5^\circ$).

I.101: In Baseline-B LRM has only 128 bins so I think the interval should end some bins before.

I.126: don't use the surname in the citation

I.128: SIN mode allows... repeats more or less the last sentence

I.142ff: This is not totally clear to me. Please explain a bit more in detail what the "coherence range power image" is.

I.205ff: Please use different letters for different variables (not again a0, a1)

I.323: I guess no ICESat campaign biases have been applied as in Nilsson et al., 2015b. Maybe the influence of those biases (~ 10 cm) on the seasonal amplitude and phase is not too big, but anyways this should be mentioned and discussed when taking ICESat as a reference for the "true surface amplitude" (I.551).

I.407: Why has no attempt been made? Please explain!

I.425: repetition: processing steps

I.692: remove "by"

To summarize, this is a very interesting study and I recommend it to be published after minor changes have been made. I am looking forward for future works including a calculation of mass trends but I agree that this is beyond the scope of this work.

Interactive comment on The Cryosphere Discuss., doi:10.5194/tc-2016-109, 2016.