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

Anonymous Referee #1

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This paper presents some of the first ice-sheet-wide results from the Cryosat-2 altimeter. It presents a set of methods leading to DEMs of the Greenland and Antarctic ice sheets, evaluates the accuracy of these DEMs, and presents elevation-change and mass-change estimates, basin-by-basin, for the two ice sheets. As I discuss below, the methods were the most interesting part the paper in that they gave a set of steps that might be followed to derive useful elevations from Cryosat data, which have a reputation for being difficult to work with. The DEM and the elevation-change results are interesting as a demonstration that the methods, broadly speaking, worked, but are not analyzed here in enough detail to really give new insight into ice-sheet processes and dynamics.

The paper suffers from nonstandard English syntax and idiom and needs to be edited

carefully, preferably by a native speaker. I have not made an effort to improve this aspect of the paper, and only comment on it where I felt that it might have prevented me from understanding the text.

The methods for deriving elevations and elevation change from Cryosat data are broadly consistent with state-of-the-art processing for other radar altimeters, with some added steps relevant to the processing of interferometric data unique to the SIRAL sensor. This part of the paper was somewhat disappointing in that the level of detail presented here was generally inadequate: the retracking algorithm and the method for deriving cross-track angles were both explained by citations, to papers by Curt Davis and Laurence Gray, respectively, but do not describe how these authors' techniques were applied to the Cryosat-2 data. Was a simple threshold at 25% of the first maximum applied to the LRM data? Is that all? Was the same technique applicable to the interferometric data?

The DEM generation seems to follow a reasonable set of steps, using an iterative approach to slope correcting the Cryosat data. It is not clear if the slope correction is a standard linearized correction or if a more exhaustive search is performed to find the POCA; it would have been good to give an indication of the magnitude of the correction after the fourth, final iteration of the correction as a way to demonstrate that the iterations had converged.

The DEM evaluation with ICESAT data makes good use of the two data sets, although it is not explained how the elevation change between the end of ICESAT and the reference date for the DEM is taken into account. In a few areas this would have amounted to tens of meters, and might play a role in some of the large errors derived for the DEM. The error plots for the two datasets seem to indicate large inaccuracies in the DEM at slopes approaching 1 degree. Showing a map of the residuals might help readers evaluate the source of these errors: Is the RMS dominated by a few, particularly bad areas, or is it consistently large? It would also be good to show, for reference, the expected error in Cryosat data as a function of surface slope. The error propagation for the elevation change includes an expected error value, which could be plotted in figure 5 as a check on its own validity.

The elevation-change section is generally fine, although the revised paper should include a reference to the Kahn et al paper from Nature Climate Change. The precipitation anomaly in Antarctica should also be described in more detail, as this seems to be one of the major signals detected in the present paper.

The calculation of mass anomalies from the volume changes is largely unsatisfactory. The authors follow Shepherd and others in assigning low effective densities to volume change in thickening regions of the ice sheet and high densities to volume change in thinning parts of the ice sheets. This procedure is guaranteed to give a biased estimate of mass change unless the authors' preconceptions of the source of the changes happens to be correct. More recent studies have used firn models to do this calculation properly, and it is hard to take these calculations seriously using the older technique.

The section in which the DEM uncertainty is calculated is very hard to follow. Some of the steps seemed clear, if not very well motivated, but the choice of the weights W_i, is not explained well at all. This needs to be rewritten extensively, because I couldn't work out what the authors trying to do.

The calculation of the uncertainty in the elevation change rate seems somewhat lacking. Equation A5 appears to be wrong, as the last two terms should be of the form d hdot / dh_DEM, not d hdot / dh. There are at least two missing terms: one to take into account the non-independence of the DEM values used in the correction (essentially a slope-error term) and a term to take into account the uncertainty in the slope correction for the elevation measurements. Equation A6 also looks like it will produce values for the aggregated uncertainty that are too small, because it treats all elevation-change errors as independent, where they are likely influenced by significant correlated errors.

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