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Interactive comment on "A range correction for ICESat and its potential impact on ice sheet mass balance studies" *by* A. A. Borsa et al.

A. A. Borsa et al.

aborsa@ucsd.edu

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Responses to Bea Csatho's review comments:

1.) Our manuscript does assume reader familiarity with the various ICESat data products, but we see the value of an overview of the ICESat data product hierarchy and a clear description of the products to which we refer in the manuscript. We will add this text to the final manuscript.

2.) In addition to the general product description, we will provide additional detail on the waveform shapes assumed for different data products and the different range determination used for GLA14 and non-GLA14 products. We agree that this information will be useful to most readers.

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3.) In response to the review's interest in seeing the impact of the G-C correction on the salar de Uyuni validation results, we will include a Figure 2b showing the results of the G-C correction on the skewness/misfit correlation.

4.) This review makes the point (as does review #1) that the simple averaging scheme we employed for the Ross and Filchner-Ronne ice shelves is not sufficient for generalizing our results to other studies. We will reestimate the ice shelf mass balance values using area weighting and present those results in our final manuscript. We acknowledge that the flat geometry of the ice shelves might not be a good analogue for the ice sheet peripheries, but we believe it is relevant to the broad ice sheet interiors. We will state this distinction clearly in the text.

5.) We agree with the comment that applying a single, global linear trend to surface change estimates is a risky way to implement a correction to ICESat. This is because the actual trend associated with a set of biases/corrections is dependent on the relative number of samples between campaigns for each dh/dt estimate, yet this is seldom considered in studies that use bias trends (see A11 in our appendix). The most rigorous way to implement a correction is to apply biases/corrections at the level of individual elevations before calculating dh/dt estimates. However, for the purposes of this paper, we wanted to show that we can predict the impact of the G-C correction on mass balance estimates if the proper weighting is employed in the estimate of the linear G-C correction trend. We will include in the final manuscript a more trenchant warning about the potential errors associated with calculating and applying linear trends for either the G-C correction or inter-campaign biases.

6.) We will be more precise in the abstract about characterizing the changing magnitude of the G-C offset. Specifically, the change in the magnitude of the G-C offset with decreasing energy should properly be described as a change in variance, and we should mention that there is an energy threshold beneath which changes are observed.

7.) We will revise the cited ICESat target accuracy from 2 cm to 1.5 cm.

8.) The salar de Uyuni DEM accuracy was quoted in Borsa et al. 2007 as 2.2 cm RMS. Additionally, we have interpolated in time between two independent DEMs that bracket the ICESat mission period and that have slightly different shapes (to within 2 cm rms). We will estimate the relevant error on the reference elevations and report this in the final manuscript. The "hybrid DEM" is what we call our interpolated reference surface, but we did not specifically define this term in the text. We will add this definition.

9.) We agree that the term "metadata" has connotations that are different than we intended and will replace it with something more appropriate (perhaps just "parameters").

10.) We will clarify that the variables in Equation 1 are the parameter names from the actual ICESat data products, so named to assist investigators who wish to calculate the G-C correction themselves.

The authors would like to thank Bea Csatho for her review and for her insightful comments.

Interactive comment on The Cryosphere Discuss., 7, 4287, 2013.