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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.

Anonymous Referee #1

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This paper documents a new correction to ICESAT data that has the potential to make significant changes in the interpretation of elevation change signals for Antarctica and, to a lesser extent, Greenland. It describes the correction in some detail, and discusses the extent to which the correction can be applied retrospectively to estimates of ice sheet mass balance that did not include this correction. It is important that ICESAT users know about this correction and that its impact be documented, and TCD is a good forum to the correction to be documented.

The paper is well written and, for the most part, quite clear in presenting the material. The structure of the paper is somewhat unusual in that it gives a narrative of how the error in data processing that gave rise to the correction was discovered, instead of the usual data, methods, conclusion organization. In this case, the paper is probably

easier to read with this unconventional structure than it would be otherwise.

Other than the presentation of the graphics, which I discuss below, I only had three concerns about the content of the paper. The first is the case study for the application of the correction to the Ross and Filchner Ronne ice shelves. For both shelves, the authors estimate the shelf mass balance by averaging elevation trends on a per-datapoint basis, instead of dividing the shelves into grid boxes and calculating a trend for each box, then averaging the trends for the boxes. Using their simplified scheme, they find that subtracting the mean of the G-C correction gives nearly the same answer as correcting each point, then taking the trend. But because they compute a per-point basis, their average is strongly weighted towards the southern end of the ice shelves, where the ICESAT tracks converge. Since the southern ends of the ice shelves are far from the ocean, they can be expected to have very few clouds, so the number of data points in each campaign should be very close to the number used to calculate the trend in the G-C correction, and it is not surprising that the correction was close to the mean correction. Their argument would be much stronger if they performed the mean elevation change correction as actual studies do, using proper area weights.

My second concern is that section 3 discusses campaign biases in the context of the compilation presented by Urban et al, 2012. Unfortunately, since the citation is to an AGU talk, there is no way for readers to learn what the different biases actually were. This section would be much more useful if the different compiled biases were restated here in a table.

My third concern is about the presentation of Appendix A. This calculation is vastly overcomplicated as written: Equations A1-A4 are just a demonstration that a weighted linear regression is a linear operator: If we summarize the regression with weights sigma as b=R(X; sigma), then R(x+y; sigma)= R(x; sigma)+R(y; sigma). It should not be a controversial assertion that linear regression behaves this way, and if there's any doubt, the authors can cite a statistics or inverse theory text. A similar argument could greatly simplify equation 8.

Last, the graphics. Figures 4 and 6 both show dense clouds of points that become entirely black near their vertical centers. It is impossible to learn anything about the structure of the point clouds from this, except at the edges where outlying points can be distinguished. At minimum, the running 25th and 50th percentiles of the point clouds should be plotted to give an idea of the spread of values.

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