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The Cryosphere Discussions
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Interactive comment on "CryoSat-2 delivers monthly and inter-annual surface elevation change for Arctic ice caps" *by* L. Gray et al.

Anonymous Referee #1

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This paper develops estimates of surface elevation change for five Arctic ice caps, using radar altimetry data acquired by the CryoSat-2 satellite since 2010. The authors provide a detailed analysis of CryoSat's capability to measure elevation changes at both a monthly and annual sampling frequency, and evaluate their findings using a range of field and airborne datasets. Particular attention is given to the impact of changing snowpack conditions on the retrieved elevations.

I found the paper interesting, informative and comprehensive. The manuscript is very well written and has clearly been thoroughly proofed by the authors prior to submission. I would expect the work to be of relevance to a wide audience of The Cryosphere, in particular to readers with an interest in the current evolution of Arctic ice caps and the performance of satellite radar altimeters. More widely, the results are relevant to

anyone with an interest in geodetic estimates of ice sheet, glacier and ice cap mass balance. I have no major comments and would therefore recommend the manuscript for publication. I have listed some minor comments below, which I hope will help to clarify certain aspects of the work.

Minor comments

P2822 line 6: Suggest “contribution of subsurface to surface” => “ratio of subsurface to surface”.

P2823 line 4-6: ICESat and NASA acronyms are not defined.

P2827 line 21: 0.13 m w.e. / yr?

P2828 line 6: “indicate” => “indicates”.

P2829 line 11: “variation in the time history of the illuminated area”. Not sure I understand exactly what you mean here. Do you mean variation with time of the area illuminated by the wavefront?

P2829 line 19: Worth noting that Davis’s measure of repeatability was based on a single-cycle cross-over analysis and so, as I understand it, his low threshold retracker showed greater repeatability with respect to variations in antenna orientation between the ascending and descending passes (perhaps because of reduced sensitivity to subsurface backscattering anisotropy), but not necessarily improved repeatability with respect to changes in time (as is desirable for elevation change detection).

P2832 line 20: By ‘better’ do you mean that it gives a lower standard error of the mean, because of the larger sample size? This would presumably reflect a better precision on the measured mean elevation change, but not necessarily the most accurate measurement of the actual surface elevation change, because the measured elevation change may include the effects of changes in the dominant scattering horizon? In some cases, when melt occurs every year, then perhaps differencing summer elevations might give a more accurate measure of the real surface elevation change because of the neg-

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ligible bias between the real and detected surface. I'm not expecting the authors to redo any analysis but I am more interested in their opinion on how to best minimise the impact of time variations in the bias.

P2833 line 13: 'This' => 'These'.

P2833 line 14 'was' => 'were'.

P2835 line 12: 'Changing historical meteorological conditions' I don't find particularly clear. Do you mean year-to-year changes in meteorological conditions?

P2835 line 21: 'CryoSat-2' => 'CS2'.

P2836 line 17: 'Devon Ice Cap and Ausfonna'

P2837 line 13: If I understand correctly, you are comparing an AWS point measurement with a CS2 spatial average calculated over several thousand square km. Do you think the different sampling scales could explain some of the observed discrepancies, for example through spatial variability in meteorological conditions within the NW sector?

P2838 line 17: If the backscatter is dominated by the previous end of summer layer, then could another possible contribution to the decreasing height over winter be from the downward motion of the previous summer layer as a consequence of firn compaction?

P2838 line 28: Here you could refer to http://onlinelibrary.wiley.com/doi/10.1002/2015GL063296/full.

P2839 line 17: These are referred to as height changes and not height decreases, and so the values should presumably be negative?

P2840 line 4: Is the trend with respect to elevation significant given the dispersion of the height change data?

P2841 line 19: Why July 2010 to Dec. 2011 and not an integer number of years?

P2842 line 8: I can see that a specular surface would increase backscattered power,

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but wouldn't moisture in the snow result in more microwave absorption and reduced backscattered energy?

P2843 line 24: remove 'at'.

P2844 line 12: I appreciate this probably has no straightforward answer, but I wonder whether the authors have any thoughts on the extent to which their conclusions are specific to their chosen retracker, or are generalizable to other retrackers?

P2844 line 14: 'between the mean CS2 height'?

P2844 line 25: Might be worth commenting on the value of the length of the timeseries. I'd expect that the magnitude of the bias is constrained by the physical properties of the snowpack and microwave penetration, and that the bias varies most over seasonal to annual timescales depending upon changing meteorological conditions. Therefore over longer time periods, would you expect the relative influence of the variable bias on the measured height change to diminish?

P2844 line 27: 'A change. . . volume change'. This sentence seems a little out of place as the manuscript has focussed on elevation change and not mass change.

P2845 line 16. I don't find this final sentence particularly clear. Do you mean the interferometric capbility to locate POCA within the beam footprint, or something different?

Table 1: Consider splitting glacier facies types with commas or new lines.

Table 1: Would be helpful to give a brief description of the data type that each DEM is derived from.

Table 1. Is 'high temporal resolution' 30 days? If so might be worth mentioning this explicitly.

Figure 2: Replace CS with CS2 to maintain consistency with acronym used in text.

Figure 4 Replace CS with CS2 and STD with SD to maintain consistency with acronyms

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used in text. To avoid any misinterpretation, suggest you define more explicitly, perhaps in the caption, what <delH> is.

Figure 6 caption: ‘stations is indicated’ => ‘stations are indicated’.

Figure 6: I know ‘A’ is described in figure 7 but would be helpful to also mention what it refers to here.

Figure 7: Panels a and b are lacking vertical axis labels to specify units.

Figure 7: My understanding was that the horizontal black dashes in panel a were meant to mark the 30 day sampling periods. However, the Nov-Dec 2011 dash seems to cover a longer time period.

Figure 7: I can’t work out where the blue AWS B data are from. It doesn’t appear to be specified in the caption or visible in Figure 6.

Figure 7: Why have you chosen to compute year-to-year height change for AWS A rather than AWS B?

Figure 7: AWS B height change looks extremely stable between Dec 2012 and May 2013. Is this real or have the data been interpolated?

Figure 8 caption: ‘along the north-south transect shown’?

Figure 8a: Labels on colour bar are difficult to read.

Figure 10: ‘has been removed’ => ‘have been removed’.

Figure 10: Is background image a DEM?

Figure 13a: dashed black line is hard to see, consider changing to a more visible colour.

Figure 14: Suggest adding units to elevation ranges in panels a-d.

Figure 14 caption: ‘The winter-to-winter average height change’.

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Interactive comment on The Cryosphere Discuss., 9, 2821, 2015.