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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.

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In this paper monthly and inter-annual surface elevation changes of 5 Arctic Ice Caps are presented. The results are derived by a detailed and comprehensive analysis of CryoSat2 data acquired in the period 2010 to 2014 and evaluated to field, meteorological" and airborne data sets.

Special focus is given to seasonal changing surface conditions causing a shift of the main radar scatter horizon, where the radar elevation is tracked. This is a very important finding, which needs to be considered when estimating elevation change time series from radar altimetry, since it demonstrates that observed elevation change might not necessarily be a true surface elevation change.

The paper is well written, clear, concise, well structured and understandable and the data processing and analysis of high quality. I think the paper is of interest to a broader community with interest in evolution of Arctic ice caps and altimetry.

I have no major concerns and would recommend the paper for publication in TC. In the following I have added some comments and questions that came into my mind when reading the paper.

P2827L4: accumulation rates of 0.5m → m/a

P2827L21: accumulation rate of 0.13m → m/a

P2829L10-15: I would expect that internal layers are not resolved by CS2 and therefore not show up as single peak. Maybe they broaden the waveform due to increased volume scattering. I would expect that peaks in the later part of the waveform are more due to undulating surface within the CS2 beam width.

P2829L21-24: It would be of great value if you show the results you obtained by the comparison between the two methods. Are they within the error bar of the derived elevation changes or do they differ significantly? From the sentence it seems to me that your new approach using the max gradient in the leading edge to re-track the elevation eliminate the problem of a variable bias between the detected elevation and the physical surface. But in the rest of the paper your findings are clearly showing that this is not the case.

P2830L12-16: This method is an excellent way to improve the quality of the SIN CS2 elevation data used in the analysis, since you get rid of large outliers without neglecting them. To demonstrate the performance of your method you could add a comparison when using the method with and without a DEM.

P2830L19: Reference DEMs: Could you please add some more details about origin, time of creation, the quality, and resolution (slightly update table 1 should be fine).

P2830L28: How close are the CS 30 day sub-cycle tracks (are they within 1km or

5km)?

P2831L9: How many data points usually fall within a 400m footprint within a 30day subcycle? Are you averaging all elevations within one footprint of one time period and subtract the averaged elevation of the second period or do you average all possible combinations of height differences for the two time periods within one footprint? Maybe add an equation to make it clearer.

P2831L14 and P2832L9 When you derive an elevation rate (m/a or m/month) for temporal height change analysis (month to month) it is important to know the averaged time tag next to the averaged elevation difference. I could imagine that this difference of the averaged times are not exactly 30 days and could vary from a couple of days. Are you considering this as well?

P2832L9: "relatively small number of data samples possible in 30 day periods:" → What means small number? Do you use any criteria to neglect points with too low coverage?

P2833L15: Technical University of Denmark (TUD) acquired the ALS data during the ESA CryoVEx campaign. TUD also processed the GPS and ALS data, which is a lot of work. In the acknowledgments TUD is listed but I think it would be good to add a sentence directly in the text for this effort.

P2833L25: As mentioned before. Could you show the statistics without using your CS2 SIN processing DEM approach to detect and correct for phase uncertainties? Do you see similar mean and SD when applying the re-tracking at a certain threshold like Helm et. al. 2014 as you discussed before?

P2834 Error estimation: This is a good error discussion but I miss the explanation how you derived the uncertainty for the elevation changes. You mentioned 5 points, which need to be considered. It's not clear to me if you have considered those points and if yes, how exactly you did this.

E.g. in Fig. 7 and 11 there are no error bars but in Fig. 13 you show the error. Does your error estimate reflect your assumption of larger uncertainty in winter than in summer?

The scatter in Fig. 5 is pretty large. Are there any assumption why this is the case? Could medium scale roughness within the CryoSat2 POCA footprint, like sastrugies cause such differences? Could you please add a figure showing the elevation difference versus surface gradient and roughness derived from the reference DEMs. Maybe this could give some more information. Another idea might be to plot the coherence extracted at the retracked bin versus elevation difference.

P2837L5 You mentioned ASIRAS data before and CRESIS Ku band airborne data in this section. Who provided ASIRAS data, who processed it - not mentioned in the acknowledgments? Did you use ASIRAS or only CRESIS in your analysis – not clear to me?

P2838L14 It would be helpful to add the elevation profile as a subpanel as well as the different glacier facies (like percolation zone) along the profiles.

P2839L17 Please add uncertainty of AWS.

P2840L14 How does your result for Austfonna compare to McMillan 2014?

P2840L26 elevation change of 1 to 1.5 m

P2840L28 you mention AWS temperature data. Could you also add this in Fig 11, as it is used to explain the April/May elevation change?

P2841L4 Why does Duvebreen AWS show an increase in elevation and CS2 a decrease between Sep2012 to March 2013? It seems that CS2 completly penetrates the fresh snow and still tracks the surface of the last summer which densifies and moves down. With the onset of the warm air the new surface is tracked by CS2. Could you comment a bit more on this? Are there density profiles of snow pit data available?

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P2841L28 Please add uncertainty (0.18 +/_ ?).

P2842L25 Which error estimate is used for the other ice caps?

P2844L12 I think one should add a citation to Ricker (2015) and Nilsson (2015) who also explained the effect of “variable bias between the physical surface and the heights derived from CryoSat-2“

Nilsson, J., P. Vallelonga, S. B. Simonsen, L. S. Sørensen, R. Forsberg, D. Dahl-Jensen, M. Hirabayashi, K. Goto-Azuma, C. S. Hvidberg, H. A. Kjær, and K. Satow (2015), Greenland 2012 melt event effects on CryoSat-2 radar altimetry. Geophys. Res. Lett., 42, 3919–3926. doi: 10.1002/2015GL063296.

Ricker, R., S. Hendricks, D. K. Perovich, V. Helm, and R. Gerdes (2015), Impact of snow accumulation on CryoSat-2 range retrievals over Arctic sea ice: An observational approach with buoy data. Geophys. Res. Lett., 42, 4447–4455. doi: 10.1002/2015GL064081.

This would add some more weight to the argument that the so called "penetration bias“ need to be considered when deriving elevation change in areas observing summer melt or freeboard change over sea ice. Would you expect that this kind of seasonal variability is important for longer time series as well?

Fig1 - Use larger characters Fig6 - AWS positions hardly visible. Use different colour. Where is AWS B located? Fig7 – Labeling of y-axis is missing. Why are you not showing results of AWS 3 and 4 Fig11 - Why does CS2 not show the summer melt in 2012 as the AWS are indicating? It is not explained in text. Please add temperature of the AWS - similar to Fig. 7.

Comment:

Maybe it is worth to scale all height changes to a height rate (m/month), because e.g. in Fig. 7 the black bars, which are hard to identify have different lengths indicating the time used for a monthly average is not constant.

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