

Interactive comment on “Retrieval of snow freeboard of Antarctic sea ice using waveform fitting of CryoSat-2 returns” by Steven W. Fons and Nathan T. Kurtz

Anonymous Referee #1

Received and published: 24 October 2018

Review of

Retrieval of snow freeboard of Antarctic sea ice using waveform fitting of CryoSat-2
returns

by

Fons, S. W., and N. T. Kurtz

Summary: This paper investigates whether the considerable scattering of satellite
radar altimetry at Ku-Band, namely CryoSat-2, that can be expected to occur at the
air-snow interface can be exploited to estimate the elevation of the air-snow interface

relative to the ocean surface and hence get an estimate of the total (sea ice + snow) freeboard. To do so a two-layer physical model is used together with least square fitting to obtain a fitted waveform to CryoSat-2 Level 1B data from which the elevations are obtained for lat winter / spring October months of 2011-2017. CryoSat-2 elevations are compared with observations from Operation Ice Bridge airborne topographic mapper for two quasi-coincident OIB-CS-2 overflights, one in 2011, one in 2012. Total freeboard is computed and averaged over the entire period 2011-2017 for the entire Antarctic and discussed and compared with ICESat total freeboard maps. Also, the potential to combine air-snow and ice-snow interface elevations for snow depth on sea ice retrieval is tested. While first evaluation results are promising and suggest that total freeboard derived from CS-2 could potentially be used complementary to ICESat and ICESat-2 data more work is required to better understand the observed differences between CS-2 total freeboard and independent data.

I find this an interesting and important contribution to the existing literature and suggest publication of the research results - provided that the authors take into account the various, partly substantial, suggestions for a major revision of their manuscript. I list my major concerns below in the general comments.

General comments: GC1: The introduction lacks to present the state-of-the-art of sea-ice and/or total freeboard retrieval in the Antarctic. Several studies exist that are using radar or laser altimeter data for this kind of retrieval. In addition, the introduction lacks to present the state-of-the-art of freeboard-to-thickness conversion and inherent problems and uncertainties. While mitigation of the former lack is required to understand why it might make sense to try to derive total freeboard also from CS-2 data, mitigation of the latter lack is required to understand your attempt to retrieve snow depth on sea ice as well.

GC2: I don't find the interpretation of Figures 3 and 4 particularly convincing as a motivation why there is substantial(ly more) information about the air-snow interface in the echograms of the snow radar and the Ku-Band altimeter. See my respective



specific comments.

TCD

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GC3: Even though Figure 6 and the interpretation is intended to stay qualitative (my guess), I strongly suggest to discuss these results in more detail. Putting more emphasis on increasing the credibility of the elevation estimates at this stage is very important in my eyes. You have the unique opportunity to have quasi-coincident air-borne and space-borne measurements. That's luxury and I have to admit that I am a bit disappointed that you do not exploit this luxury situation further. If I'd be allowed to recommend something, then I would i) quantify the temporal and spatial differences in the tracks and try to investigate whether a correction towards a better spatiotemporal match is worth an effort, ii) collocate the tracks with ice-type information (it might be sufficient to figure out where first-year ice and perennial ice was present), iii) collocate the tracks with meteorological information, e.g. from ERA-Interim or MERRA-2 or perhaps even from one of the higher-resolving weather forecast models to figure out whether air temperatures have been close to 0degC and/or whether and what kind of precipitation potentially occurred (Ideally you have a look at the meteorological conditions of not just the day of the coincident measurements but also of a 1-2 weeks period before to catch potential melt events and hence snow metamorphism near the air-snow interface.). See also my respective comments for Figure 6 and its interpretation.

GC4: The interpretation of Figures 7 through 9 would also very much benefit from a more critical discussion which should also involve more work done by other researchers. I find a lack of attempts to explain the differences observed, e.g. in Figure 9. See my specific comments to these figures.

Specific comments:

Abstract: I suggest to add the standard deviation or Root-Mean-Squared difference in addition to the mean difference values given. If computed, also modal values of the difference would allow to give the obtained values more credibility.

Page 1 L30: I guess, since you are focussing on Antarctic sea ice it would not hurt

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do use citations referring also to the albedo observed over Antarctic sea ice: Brandt RE, Warren SG, Worby AP and Grenfell TC (2005) Surface albedo of the Antarctic sea ice zone. *J. Climate*, 18(17), 3606–3622 (doi: 10.1175/JCLI3489.1) and Zatko and Warren, *Annals of Glaciology* 56(69) 2015 doi: 10.3189/2015AoG69A574

Page 2 Line 2: I suggest to cite Comiso et al., *J. Climate*, DOI: 10.1175/JCLI-D-16-0408.1 instead of Beitler 2014; the former is a peer-reviewed paper.

Line 6: Please add "Antarctic" or "Southern Ocean" to make clear that these ship-based observation based sea-ice thickness data set is valid there but not general in the polar regions.

Line 5-13: - Is there a reason why you refer to multiyear ice only for the Arctic? - Is there a reason why you refer to sea-ice thickness in the Arctic only while for the Antarctic you refer to sea-ice thickness in volume? Is the sea-ice thickness retrieval in the Antarctic more accurate so that it makes sense to also derive the volume?

Line 14-20: - I am not sure I like the mentioning of Kwok et al. (2009) and Kurtz and Markus (2012) as the role models for sea-ice thickness measurements from active satellite sensors. Since you are basically referring to the principle, wouldn't it be sufficient to simply write what you wrote without these two references? I I guess my dislike comes from the fact that there have been earlier papers that describe how laser altimetry (which is the main focus in this paragraph) can be used to get an estimate of the total freeboard of snow-covered sea ice: Kwok et al. (2004) or Kwok et al. (2006) for the Arctic and Zwally et al. (2008) for the Antarctic.

L21-30: - I suggest to re-organize the sentences starting in Line 24 to avoid that sea-ice freeboard is used before being explained. How about you write along these lines: "... 2010-2012. The difference between laser ... [continue until Line 29] ... above the sea surface, known as the "sea-ice freeboard", and is used to calculate sea-ice thickness applying appropriate assumptions (see previous paragraph)."

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L31-39: - I suggest to expand rightaway in Line 31: "by the depth and variable vertical structure of the snow on top ..." - In Line 32, I suggest to add that it is not simply more precipitation but "... more and more frequent precipitation ..." - Line 34: "sea ice down near the" → perhaps better: "sea-ice surface down near or even below the" - I suggest to break in Line 38 for a new paragraph, starting with "While ... ". - Is there perhaps also the chance that you quantify how large or weak the scattering at the air-snow interface is compared to that at the ice-snow interface? This could make your motivation stronger about why it might be reasonable to look for the snow surface scattering contribution even in Ku-Band. I strongly suggest to seek for evidence in the literature about the possible strength of the snow surface backscatter at Ku-Band (at nadir) to underline that it is physically reasonable to use CS-2 SIRAL returns for snow freeboard retrieval. I am stressing this because there exists literature in which one aims for snow-depth on sea ice retrieval by confidently assuming that Ku-Band penetrates to the ice-snow interface and combining it with a Ka-Band radar such as from SARAL AltiKa (Guerreiro et al., 2016). Your attempt is clearly counter-acting their assumptions.

Page 3: Line 10: "builds off" ?

Line 22: I don't understand the mentioning of the "originally 128". What is this for?

Line 34-37: The motivation for choosing data from October is clear. You could have stated why you did not also use data from November. Most of the ICESat spring measurement periods last well into November. Here you state years 2003 to 2009 for ICESat as years with measurements but actually using you are only data from 2003 to 2007. I can understand that the main motivation for this is to use the data produced by one of you. But from NSIDC and potentially also from University of Hamburg you could possibly have obtained ICESat freeboard data for 2003 through 2009, i.e. from an equally long period as you have CS-2 data from. You stated yourself explicitly, that "Seven years of data allows for a longer-term average to be computed"

Page 4: Line 1: - It would be helpful for a better understanding of Figure 3 to mention

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the frequency range of the FMCW snow radar. - "First, ..." → Where is the "Second"?

Line 8-12: Is this a gridded product? If yes, which grid resolution does it have and what is done to fill the gaps between the ICEsat overpasses?

Line 13-16: You are using sea-ice concentration data obtained with the NASA-Team algorithm. While when choosing a 50% sea-ice concentration threshold is might not really matter which product to use there is published evidence that the NASA-Team algorithm often severely under-estimates sea-ice concentrations in the Antarctic compared to the truth - particularly in late winter / spring. You could avoid students and early-career scientists being trapped by your choice by choosing a more appropriate sea-ice concentration product right from the beginning, i.e. based on the Comiso-Bootstrap algorithm or the Eumetsat OSI-450 algorithm.

Line 18: "... altimetry tend ..." → I suggest to insert: "for ice freeboard retrieval"

Line 20: You could cite Willat et al. (2011) here.

Line 35/36: "This result is expected, as it means that the scattering power from the air-snow interface is closer in magnitude to that of the snow-ice interface in snow radar returns" I do understand your conclusion from the smaller difference in power (about 13 dB for snow radar and 14 dB for Ku-Band altimeter) but I don't understand why this is expected. Lets assume for simplicity that the peak power is 20dB at the ice-snow interface for both instruments. Then the power at the air-snow interface would be about 7 dB for the snow radar and 6 dB for the Ku-Band altimeter and with that the power at the air-snow interface would be SMALLER at that frequency from which you assume that the backscattering at the air-snow interface is LARGER. How does this fit together?

Page 5: Line 2: At the end of this paragraph interpreting Figures 3 and 4 I have a few questions. i) How accurate are the two instruments with respect to the dB values shown? ii) How relevant is the similarity in the histograms shown in Figure 4 with

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respect to the shared mode at about 11dB while at the same time the histogram shows secondary modes at 16dB (snow radar) and 20 dB (Ku-Band). In addition I have a few comments: iii) What were the meteorological conditions during that flight? Can we expect homogeneous snow properties in terms of snow wetness etc. iv) How would Figures 3 and 4 look for the October 2011 campaign? Would they result in the same result? v) What is the length (in Kilometers) of the transect (or echogram) shown in Figure 3? vi) The Ku-Band altimeter histogram in Figure 4 has a substantially longer right tail with high dB values. It is almost certain that these values are responsible for the 1dB difference observed between the snow radar and the Ku-Band altimeter data. Are these particularly large differences the result of a particularly low power at the air-snow interface compared to the peak power or is this the result of peak powers being generally elevated at Ku-Band compared to the snow radar? vii) What explains the larger time difference between the locations denoted by red and black points in Figure 3 for snow radar echogram compared to Ku-Band? viii) What is the source for the staggered echos above the air-snow interface at Ku-Band? Such echos are not at all present for the snow radar data.

Line 21: What is "n"?

Page 6: Line 2: I guess "thicker snow depth" and "scattering effects from the snow surface" are not as much linked with each other as scattering effects from the snow volume. What makes a thicker snow cover to have more surface scattering than a thinner snow cover?

Line 27: Why "Though"?

Page 7: Line 16: What is the motivation to only use a different initial guess for alpha in case resnorm is too high after the first fitting attempt?

Page 8: Line 8: Are these PP and SSD thresholds also taken from Laxon et al. (2013)?

Line 12: "seasonal average freeboard datasets" -> perhaps better "datasets of the

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seasonal average total freeboard"?

Line 15: A very good place to cite the paper by Kwok and Maksym, 2014 in J. Geophys. Res., doi:10.1002/2014JC009943

Lines 23-25: Please explain how the OIB data area used. Did you take data from both flights? Did you average over all valid points? What is meant by "respective surfaces"?

Two final question at the end of this section: What happens in the special case of a snow free ice floe? What happens in case of a wet snow surface, where penetration of the Ku-Band into the snow cover is almost zero?

Line 32: "found" → perhaps better "computed" or "derived"?

Page 9: Lines 7-13 & Figure 6: - Please provide a measure of the total distance along the tracks shown in Figure 6 a) and b). This would make referencing to certain feature more easy in addition to simply providing an easier interpretation of the spatial scale we are looking at. - I suggest to add a vertical line at zero difference ATM minus CS-2 in images c) and d). - What is the average difference in successive measurements in images a) and b)? - Suggest to use the same y-axis scaling for a) and b) for a better visual comparability of the elevation variations. - I'd say that the overall agreement, i.e. the large-scale agreement is better for 2011 than 2012. - For 2011 the mean is very close to zero, right. But the modal value is between 5 and 10 cm with CS-2 underestimating ATM elevation. - CS-2 elevations quite often exhibit strong variations in magnitude; in 2011 more during the first third of the track, in 2012 during the first two thirds of the track. These strong variations (or jumps) are as large as about 40 cm and except in one case do not have a counterpart in the ATM elevations. Please try to give an explanation to these. - While in 2011 the large-scale agreement is quite good (you could even stress this impression by adding elevation profiles with large-scale smoothing applied), in 2012 there appears to be a systematic under-estimation of the ATM elevation by CS-2. Please try to give an explanation to these as well. - You argue that differences between the two elevation data sets might be caused by "initial

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temporal and spatial discrepancies between the two data sets. Would you be able to quantify these differences? Would it make sense to do a correction of the track of one sensor with respect to the track of the other sensor? - You write that both datasets "appear to detect similar locations of troughs and ridges along the flight line". I don't find this statement particularly convincing because there are also many cases where troughs in one dataset and ridges in the other dataset coincide.

Lines 19-33: - Line 25: "fewer than five data points" → What is the distance of successive data points? How many data points would fall into one 25 km grid box at maximum, i.e. diagonal crossing? Can you comment on the data density as well? How many CS-2 overpasses or days with CS-2 overpasses in one grid cell do you have in one month? - Lines 29/30: "Any points within each grid box ..." → Could it be that this sentence should be placed before the previous sentence? I am asking because the previous sentence already describes the method used at grid level. - Line 32: "are smoothed" → Why is this? Why do you do that? Is it because of the gaps between the overpasses? Please state so in the paper.

Page 10 Paragraph ending in Line 7: - While there is not too much work yet about freeboard distribution from radar altimetry in the Antarctic I still suggest that you consider comparing your results with the results published by Giles et al. (Geophys. Res. Lett., 2008), Schwegmann et al. (Annals of Glaciology, 2015), and Paul et al. (TC, 2018). - While the work of Nghiem et al. (2016) is really interesting and certainly not invalid over parts of the Antarctic MIZ I suggest that you also take into account (and at least mention if not discuss) the potential effect of ocean swell, lower CS-2 data density and hence a larger representativity error, and ice types being different in the MIZ than in the pack ice; a large fraction of the Antarctic MIZ is formed by the often several hundreds of kilometers of pancake ice or cake ice or first-year ice with small floe sizes (< 100 m) for which I doubt that CS-2 is going to provide reasonable elevation and hence freeboard estimates. - I note that the distribution of total freeboard shown in Figure 7 is quite patchy and contains several artificial south-north oriented freeboard variations (possi-

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bly caused by sampling issues). I note that the freeboard in the southern Ross Sea is indeed lower than further north. However, given the fact that this is an area of extensive new-ice formation and export paired with low precipitation and hence thin snow cover, the freeboard values shown are certainly at the higher end of what is typical there - if not a proper overestimation. Sea-ice thicknesses in the southern Ross Sea are 20 to 50 cm ... total freeboards (without snow) therefore in the range of between 2 and 5 cm and not between 10 and 20 cm as indicated in the maps.

Lines 8-15 & Figure 8 - "smallest measured freeboard" -> perhaps better "smallest measured mean October freeboard" - 25.77, 27.6, 12.97 ... I suggest to give these figures with the same number of digits, i.e. 25.8, 27.6 and 13.0. - Showing the mean total freeboard together with the sea-ice area is certainly fine even though, as you stated correctly, it is not too clear why you find a good correlation between these two quantities. However, instead of the sea-ice area one could plot other variables as well. One would be the standard deviation of the mean total freeboard as a measure of the scatter of the mean values. A second one would be to show either the number of 25 km grid resolution grid cells with valid CS-2 data or even the number of individual valid freeboard (or elevation) measurements. Since you have many gaps in the original CS-2 data it would potentially be a very interesting additional information. - I note that the maximum inter-annual difference of the mean October freeboard is 2 cm. Is this within or outside the retrieval uncertainty?

Lines 17-25 and Figure 9: - I suggest to color the open water in a grey tone (different than Antarctica of course) to ease discrimination between areas with differences close to zero and open water. - I suggest to reduce the range of the differences shown to +/- 30 cm to show more details. The way the range is chosen currently only reflects the larger differences. - I find it essential that you mention three things in your discussion of this Figure: i) the larger number of years for CS-2 (7 instead of 5), ii) the fact that the ICESat data cover different time periods with at least 2 of the five years have a substantial if not dominating overlap in time with November and hence conditions changed

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towards spring (As far as I recall you analysis you did make the effort the average CS-2 from exact those dates from which also ICESat measurements exist.), iii) the fact that we look at years 2011-2017 for CS-2 but 2003-2007 for ICESat, i.e. two different, not overlapping time periods. While this might not have an effect it needs to be stressed once more in the context of this discussion. Finally, iv) one could ask whether you used the same method for averaging the CS-2 data (and filling gaps, extra- or interpolating gaps) than was done in Kurtz and Markus, 2012? Because of these four issues I warmly recommend to delete the last sentence in Lines 24/25. - When talking about a difference of only 1.9 cm: What are the uncertainties in monthly mean freeboard from CS-2 and from ICESat? Is the difference about the uncertainty? - You do not make any attempt to explain the highlighted negative freeboard differences CS-2 minus ICESat in the Weddell and Amundsen Seas. Why? - How do your results compare to the work of independent researchers: Yi et al., 2011, Kern and Spreen, 2015, Kern et al., 2016, Li et al., 2018?

Lines 27-36, Figure 10: - Figure 10 is indeed quite interesting because the highest "snow depths" are not observed in the Weddell Sea but on East Antarctic sea ice. Puzzling. This is even contradicting your own work (Kurtz and Markus, 2012), where the freeboard maps shown are assumed to represent the snow depth while assuming sea-ice freeboard to be zero. In that work maximum freeboard and hence "snow depth" was observed in the Weddell Sea. What is further interesting is that the "snow depth" is nowhere considerably smaller than 10 cm - even not in the southern Ross Sea where there is little or no snow on the young sea ice. - I would have found it again very useful, if you would have related the results shown in Figure 10 also with other work, i.e. snow depth based on satellite microwave radiometry (Markus et al., various) or based on ICESat data (Kern and Ozsoy-Cicek, 2016).

Page 11: Lines 1-4: That the peak-picking algorithm provides snow depth along that flight line which is within 10% of the values published by Kwok and Maksym (2014) is an encouraging result and should be highlighted more. Lines 4-6: I am pretty sure

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that this is a frequency issue and not an issue of bandwidth or footprint size: The snow radar used on OIB is a 2-8 GHz radar, right?, while CS-2 operates in Ku-Band. With the "correct" snow conditions it is very likely that CS-2 does not penetrate down to the ice-snow interface, explaining the considerably lower "snow depth" value estimate from the two elevations. Actually, the OIB snow depths are potentially even higher because of the difficulties to retrieve snow depth in areas of deformed sea ice and on multiyear-like ice reported elsewhere. Comment: Again I doubt that the precision and accuracy of the data warrants to give mean values with 3 digits = millimeter precision here. I guess 0.29 m, 0.26 m and 0.15 m would do it.

Line 8: I suggest to delete "slightly". It is considerably greater. Line 12: "validating" → perhaps better "evaluating" or even only "understanding". Line 13: "to better understand the snow depth distribution on sea ice" → perhaps better: "to use it together with the air-snow interface for snow depth on sea ice estimation."

Lines 15-27: - Line 16: "air-snow interface of sea ice" → perhaps better "air-snow interface of snow on sea ice". - Line 20: "validate" → "evaluate" - Line 24: "data from comes from" ...? - Line 22-27: I strongly suggest you revise these conclusions based on the additional analysis, interpretation and discussion that is recommended in the general comments. In particular, figures for standard deviation and potentially also uncertainties should be given in addition to the mean values. One can have a mean value close to zero with one part of the data pairs having -50 cm difference and the other part having +50 cm difference ...

Lines 28-33: - Line 28: "retrieved ice freeboard" → you did not really retrieve ice freeboard, did you? You computed the snow depth from the difference between air-snow interface elevation and snow-ice interface elevation. - Line 29: "lower than typically expected" → since you did not show any other results about snow depth on Antarctic sea ice - expect for the case of East Antarctic sea ice - it is difficult to follow this statement. - Line 31: I agree about the potentially wide-spread flooding of Antarctic sea ice but sea ice is mostly flooded when the ice-snow interface is submerged be-

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low the sea level and in that case an ice-snow interface does not exist in that sense anymore. If it still exists, e.g. through to lateral flooding it is possibly close to zero. It might therefore be more correct to again refer to sea water and brine wicked up into the snow, creating a saline snow - non-saline snow interface which is possibly the interface seen by Ku-Band. Question: For the location of the air-snow interface you have a-priori information from seasonal mean ICESat snow freeboard. For the ice-snow interface you don't have any a priori information, do you? This could be one explanation for the sub-optimal performance with regard to detecting the ice-snow interface as well. - Line 34: I guess it would be fair to cite the already existing literature about using CS-2 data for Antarctic freeboard (sea-ice thickness) retrieval: Paul et al., 2018, and change "... observing Antarctic sea ice." into something like: "... observing Antarctic sea ice with satellite radar altimetry in addition to Paul et al. (2018)." Maybe there is even more work out using CS-2 data in the Antarctic? Please check!

Page 12, Line 7: Again I think it would not be too bad to add the work of other authors here to avoid the impression that you are the first on this field: "... for improved retrievals of Antarctic sea ice thickness, complementary to sea-ice thickness retrievals based on the 15+ years long time series of combined Envisat - CryoSat-2 freeboard estimates (Paul et al., 2018)."

Page 23: Line 23: Giles et al. This paper was in Geophys. Res. Lett. and not The Cryosphere

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2018-164>, 2018.

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