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Interactive comment on "Seasonal variations of the backscattering coefficient measured by radar altimeters over the Antarctica Ice Sheet" *by* Fifi I. Adodo et al.

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Interactive comment on "Seasonal variations of the backscattering coefficient measured by radar altimeters over the Antarctica Ice Sheet" by Fifi I. Adodo et al.

In this study, Adodo et al. observe seasonal variations in radar backscatter over the Antarctic Ice Sheet using three different radar frequencies. The authors define regions over Antarctica where backscattered power is found to peak in the summer for the S band, winter for the Ka band, and in both summer and winter for the intermediate Ku band. The authors perform a sensitivity study to help understand the effects of surface snow density, snow temperature, and snow grain size on backscattered power from

each radar band. This study, in particular the delineation of these summer and winter 'peak zones', as referred to by the authors, represents a worthwhile addition to the literature. However, in my opinion some of the reasoning the authors provide in the discussion section to relate these seasonal variations to physical processes is lacking in places, and I would appreciate if they could respond to the following comments.

âĂć We appreciate that the reviewer considers our study as "a worthwhile addition to the literature", and we would like to thank her/him for her/his careful and thorough reading of this manuscript, and comments (shown in italics). Our responses are given below.

P1 L8: I would suggest rephrasing to "radar wave interaction with the snow. . ." instead of "radar wave penetration. . .".

âĂć The sentence has been modified as suggested. The sentence now reads (Line 8) :

" The radar wave interaction with the snow provides information both on the surface and the subsurface of the snowpack, due to its dependence on the snow properties."

P1 L27: I feel the phrase "More or less corrected" is quite vague here: corrections for atmosphere/ionosphere and slope errors are well established in the literature and minimise these errors with good accuracy – radar wave penetration is the main outstanding problem listed.

âĂć The text has been modified according to the reviewer's suggestion. Indeed, in the text we have separated the first errors and the last the one that is more critical. The text has been rephrased as follows (Line 26 to 31) :

"However, altimetric observations are affected by several errors : errors due to atmospheric/ionospheric propagations, slope error and error due to the radar wave penetration into the cold and dry snow (Ridley and Partington, 1988). The first two errors are usually corrected with good accuracy (Remy et al., 2012; Nilsson et al., 2016), while the last one is the most critical and the most challenging problem to tackle (Remy et al., 2012) as it results in an overestimation of the observed distance between the satellite and the target, leading to a negative bias in the surface elevation estimation."

P1 L30: I have some concerns with the 2012 Greenland melt event being used as an example here. This is a positive elevation bias (not negative as the authors discuss in the preceding sentence) caused by a resetting of the radar scattering horizon due to an anomalous surface melt event. This is a process without equivalent in Antarctica, and one that has also been corrected for in the literature when measuring surface elevation change with radar altimetry (Nilsson et al., 2016, McMillan et al., 2016). In my opinion the authors should clarify this here, or include more examples on the effects of radar penetration on time series of elevation in Antarctica from radar altimetry in order to better establish the problem they are addressing.

âĂć The Greenland example has been removed and the text has been edited. We have added two references. The sentence now reads (Line 31 to 35) :

"The magnitude of the penetration error on the estimated surface elevation is between a few tens of centimeters and few meters (Remy and Parouty, 2009). For instance, Michel et al. (2014) have found a surface elevation difference of -0.5Âăm between ENVISat and ICESat crossover points over Antarctica. Authors relate this negative bias to the difference in the penetration depth between the radar altimeter wave that penetrates within the snowpack and the laser altimeter beam that not penetrates within the snowpack."

"Michel, A., Flament, T. and Remy, F.: Study of the Penetration Bias of ENVISAT Altimeter Observations over Antarctica in Comparison to ICESat Observations, Remote Sens., 6(10), 9412–9434, doi:10.3390/rs6109412, 2014."

P1 L41: The authors fail to mention the work of Davis and Ferguson here (Davis and Ferguson, 2004), however I feel this is a significant contribution to the literature which the authors should include.

âĂć Our impression is that the improved method of Davis and Ferguson (2004) is original but does not fit with the scope of this paper. Our purpose is to mention the two most popular approaches that process the satellite data using the crossover or the along-track analysis, and use the backscattering coefficient to adjust and reduce the effect of the spatially varying radar penetration error. Note that, we are not proposing a new method but addressing the contrasted behavior of the seasonal variations in the observed radar backscatter between frequencies and regions.

P2 L46: The authors should cite Ridley and Partington 1988 here.

âĂć Thank you for spotting this lack. The reference has been cited.

P2 L50: I think it would be helpful for the reader to know the wavelength of each radar band in addition to the frequency, either here on in Section 2.1.

âĂć The wavelength of each radar band has been added.

" S (3.2ÂăGHz $\sim$ 9.4Âăcm), Ku (13.6ÂăGHz $\sim$ 2.3Âăcm) and Ka (37ÂăGHz $\sim$ 0.8Âăcm) bands."

P1 L55: The manuscript states later on that the orbit of AltiKa has recently been shifted so I would suggest amending this sentence to reflect this.

âĂć The sentence is reworded to account for reviewer's suggestion. The sentence now reads (Line 69):

" The launch in March 2013 of the radar altimeter SARAL/Altika that operates at the Ka band (37ÂăGHz $\sim$ 0.8 cm) and had the same 35-day phased orbit as ENVISAT until March 2016, allowed comparisons with much higher frequencies for the first time."

P2 L79: The authors describe the radar waveform, but the concept has already been introduced in the previous section. I would suggest formally defining the waveform where it is first mentioned as opposed to here, as it is a key concept needed for the paper.

âĂć We have moved the waveform definition to the appropriate paragraph in the Introduction (Line 38).

P3 L87: The authors should rephrase this sentence to make it more clear that the ICE-2 retracker is used to obtain backscattering coefficients for the Ku and S bands.

âĂć The text has been revised, as suggested. The sentence now reads (Line 87):

"The ICE-2 retracking process was applied to the Ka, Ku and S band waveforms allowing estimation of the range, the backscattering coefficient ($\sigma 0$), the leading edge width and the trailing edge slope."

Do the authors consider ascending and descending tracks separately? A previous study has shown radar backscatter has an anisotropic dependence resulting from the interaction between the radar polarization direction and wind induced features of the firn (Armitage et al., 2013).

âĂć This point has not been addressed in the manuscript because we found no influence on time varying component. In fact, we explored the seasonal amplitude and phase of the backscattering coefficient using crossover and along tracks analysis. We considered the ascending and descending passes separately at the satellite cross tracks and at the along-track. No significant difference or geographical pattern (similar to that observed by Remy et al. (2012) or Armitage et al. (2014)) have been found. We found that the azimuthal anisotropic effect is quite stationary from one cycle to another, therefore does not affect the seasonal characteristics. Consequently our analysis of the seasonal cycle of the backscattering coefficient using both ascending and descending passes at along tracks analysis is free of anisotropic effects. In order to keep a higher density of available data points and cover most Antarctic Ice sheet, we have prioritized the use of both the ascending and descending passes instead of one of them.

âĂć A sentence has been added to specify this point in the section 2.2. (Line 108) :

"We have found that along track analysis of the seasonal parameters of $\sigma 0$ showed no dependence to anisotropic effects. In the following, both ascending and descending measurements are mixed to keep a high density of observations and cover most AIS ($\sim$ 1.9 million data points)."

Figure 1: Can the authors please include a map to indicate where this location is in relation to the continent, and along the orbit tracks of the 3 bands.

âĂć The illustrated location has been indicated in the maps with a cross mark.

P3 L105: Do the authors place any controls on poorly constrained fits due to e.g. poor match between observed and modelled seasonal peaks?

âĂć We placed a criterion on the length of the time series. The sentence has been edited to reflect this missing information. The sentence now reads (Line 105) :

"The fit was done with the Ordinary Least Squares (OLS) method and all data points with time-series length less than 11 cycles (about a year) were discarded."

âĂć The OLS method fits data with the minimized root mean square error (rmse). The minimum rmse of the fit using equation 1 are under 1ÂădB at the three frequencies and the regions with highest rmse are near the coasts, on ice-shelfs and on a part of the Western Antarctic. This may be explained by the large variation in the signal in these regions linked to ocean influence. In East Antarctica, where the delineation is the most remarkable, the rmse is less than 0.5ÂădB in the inland of the continent. We have confidence in the OLS method because we also checked manually the fit at many data points.

In addition, can the authors please provide more information on how the amplitude and phase are gridded. Do they use the mean? If so, are there grid cells which have a high variance? How many coefficients, on average, are binned into a 5 km grid cell for each radar band? What data coverage does this provide in more challenging regions such as the margins and the Peninsula?

âĂć We gridded data only for visualization needs as explained in the text. As suggested by the reviewer, we have added a text to explain thoroughly how the data are gridded over the AIS (Line 110) :

“For visualization needs, seasonal parameters are interpolated on a map of grid by averaging with Gaussian weights. We considered all data points within a 25 km radius and weighted with a decorrelation radius of 10 km. ”

âĂć A text has been added to detail the dataset used in the section 2.1 (Line 91) :

“The ENVISAT and AltiKa datasets used in this study were averaged at a 1-km scale on the ENVISAT nominal orbit.”

P4 L151: Can the authors please comment on the validity of applying this firn density profile obtained at one location to the rest of the Antarctic ice sheet – how sensitive are the results to this assumption?

âĂć The assumption of the firn density profile used has a negligible effect on the results of the simulations. The simulations show the same evolution in the magnitude when applying a constant vertical density profile. The snow density profile reliability will be questionable if the absolute value of the backscattering coefficient had been simulated. In fact, snow density profile ideally increases with depth, due to snow compaction over time. One can idealize the firn density profile over the AIS with a given density profile for a sensitivity tests, as snow density variation range is known.

A text has been added to specify this :

“The choice of the vertical density profile has a negligible effect on the results of the sensitivity test.”

P5 L168: Which day/month of the year do these peaks correspond to? More information can be provided to the reader here.

âĂć Details on day/month of the year have been added in brackets, as also suggested

by the reviewer 2. The summer and winter seasons were accurately defined further in the section (Line 179).

Figure 2: It would be useful to plot elevation contours (or an inset elevation map) if elevation is used to delineate backscatter patterns in the text. It would also be helpful for the authors to indicate the locations of regions they refer to in the text (e.g. Wilkes Land, Dronning Maud Land).

âĂć Elevation was not used at all in the manuscript. Instead the delineation of the radar backscatter patterns is the ‘date at which the backscatter reaches a maximum’ derived from the seasonal phase of the backscattering coefficient at the Ku frequency.

Would it also be possible for the authors to mark out the summer and winter peak zones they define in the text? Finally, I would also suggest using a different colour scale, the differences between pale yellow-green-blue are quite hard to make out.

âĂć As suggested by both reviewers, the boundaries of the SP zone have been drawn on each map. These boundaries show regions where the backscattering coefficient at the Ku band peaks before April. We have also changed the color scale and have plotted a cross mark on the map to indicate the location of the time series shown in figure 1.

P5 L177: Do these percentages refer to the observed area, or the entire Antarctic ice sheet? This applies to any percentage stated in this way.

âĂć The correction has been made. These percentages refer to the observed area. The sentence now reads (Line 181) :

“With these definitions, the WP and SP zones represent 42% and 45% of the observed area, respectively.”

P5 L178: Do the percentages in brackets also refer to the area of these summer and winter zones for each band? As written it is not clear to me, I would suggest rephrasing this.

âĂć As suggested, we have rephrased the sentence as follow (Line 182) :

"The histogram of the date of maximum $\sigma 0$ at the S and Ka bands are unimodal with a peak in summer for a lower frequency (WP : 11%, SP : 66%, using the summer and winter periods previously defined) and a peak in winter for a higher frequency (WP : 50%, SP : 14%, using the summer and winter periods previously defined)."

Figure 4: I would suggest using a different colour scale which is preferably divergent to make the figure clearer.

âĂć The suggestion has been taken into account.

P6 L187: How are the uncertainties in backscatter coefficient derived here? They appear to be quite large to me.

âĂć If we understand correctly this comment, we have not derived the uncertainties of the backscatter coefficient but the average and the standard deviation of the seasonal amplitude in the WP zone, using all the data points where the backscattering coefficient peaks between the Julian days 175 and 275 (June to September) , i.e. around 42% of the observations (see Figure 6).

P6 L195: Can the authors please expand on how they are deriving surface elevation, is it also from the ICE-2 retracker, and binned at the same 5 km grid used for the backscatter coefficients? Have these elevations been corrected for atmosphere/slope? In addition, how are the values of dhdσ derived?

âĂć The surface elevation were indirectly estimated from the retracked range (computed with the ICE-2 retracker) at each data point. The surface elevation has been corrected for atmospheric errors. However, there is no need to correct for slope error because the computation is done at each along track data point. Seasonal parameters have been gridded for visualization needs.

âĂć dh and d$\sigma 0$ are the elevation residuals and the backscattering coefficient residuals, respectively, obtained by subtracting the mean signal from the times series.

âĂć dh/d$\sigma 0$ is the correlation gradient or the slope of the linear relationship between residuals of the elevation and backscattering coefficient at each available data points over the AIS.

The text has been expanded as follow (Line 201 to 212).

"Figure 7 shows the spatial distribution of temporal variations of the estimated surface elevation residuals with respect to $\sigma 0$ residuals at the Ku band, hereafter denoted dh/d$\sigma 0$. The surface elevation were indirectly estimated from the retracked range (computed with the ICE-2 retracker) at each data point and were corrected for atmospheric errors. dh and d$\sigma 0$ were derived by subtracting the mean value from the time series of the elevation and backscatter, respectively. dh/d$\sigma 0$ represents the correlation gradient or the slope at each data points over the AIS. Negative values of dh/d$\sigma 0$ indicate that surface elevation decreases when $\sigma 0$ increases, implying that temporal variations in $\sigma 0$ are due to changes in the deep snowpack properties, i.e. in the volume echo. In fact, the inverse relationship between surface elevation and $\sigma 0$ is related to a greater backscatter from depth that shifts more power to greater delay times in the received waveform, thus increasing the retracked range and decreasing the estimated elevation (Armitage et al., 2014). On the contrary, positive values of dh/d$\sigma 0$ indicate that the surface elevation increases with $\sigma 0$. In this case, the temporal variations of $\sigma 0$ are related to changes in the surface echo. The map in Fig. 7 shows that near-zeros and negative values of dh/d$\sigma 0$ (in blue) are found in the WP zone. This means that the WP zone undergoes large variations of volume echo."

Figure 7: The units in the caption state dB not m/dB.

âĂć The correction has been done.

P6 L217: "...resulting in a decreases of the radar wave in the volume. . .". Are the authors referring to a decrease of backscattered power? I suggest the use of more precise language in instances like this. Also decreases should be decrease.

âĂć The correction has been done. We are referring to the backscattered power. The sentence now reads (Line 228): "This sensitivity is explained by the fact that increasing snow temperature increases absorption resulting in a decrease of the radar wave penetration in the medium, thus limiting the volume echo."

P6 L220: It would be helpful to show this WP zone on Figure 4 to make clear to the reader.

âĂć The suggestion has been taken into account.

P7 L222: Do the authors have any evidence to back up this assertion of volume echo variations being driven by temperature? I agree this is a reasonable conclusion to propose, however the authors do not offer enough evidence to convince that this is indeed the case. I would suggest that the sentence is reworded to make the authors argument clearer.

âĂć In Figure 7, we showed that a greater backscatter comes from depth in the WP zones where the signal peaks in the winter at the Ku and Ka band. In addition, in Figure 8 we demonstrated that the variations in the volume scattering with respect to the snow temperature is 2 times greater than that of the density and grain size. In Figure 3, there is a lag of about 40 days between the peaks of the Ka and Ku bands in the WP zone (see, Figure 4). We have simulated the seasonal phase of the volume echo (Figure not shown) and found that only the temperature gradient can cause a lag between the Ku and Ka bands.

The sentence has been reformulated as follow (Line 234):

"As the temperature controls the snow grain metamorphism and the radar wave penetration depth, the variation in the volume echo would be predominantly driven by the seasonal variations of snow temperature in the WP zone."

P7 L230-240: I am not sure I agree with the soil analogy – in my opinion it doesn't offer any clarity to the reader and isn't needed. Can the authors please expand on what they

mean when they state the snow surface is sensed "as a volume scattering medium at the Ka band" – in reality there will always be a surface component of the radar echo controlled by incidence angle and topography on the footprint scale.

âĂć We can understand the concern of the reviewer about the soil analogy. Here, we are not comparing the soil and snow media but their common surface scattering behaviors in radar altimeters and in surface scattering models. It is important to keep in mind that we are addressing, in this study, the seasonal characteristics of the observed backscattered power. It is obvious that a surface component is always present in the signal, but if it does not vary over time, it can not explain the seasonal cycle. For instance, the spatial distribution of the seasonal amplitude of the Ka band is an evidence that the surface component is present and would be much greater in the wind-glazed surfaces region (smooth and polish surface).

This paragraph has been edited to explain thoroughly our argumentation and the soil analogy has been removed. Now reads :

"From the S to Ka band, the radar wavelength decreases by a factor 12 from 9.4Âăcm to 0.8Âăcm corresponding to a scale change from centimeter to millimeter. The scale at which the surface roughness plays a role in radar backscattering coefficient depend on the radar wavelength (Ulaby et al., 1982). On a rough surface, the surface scattering consists of two components: the coherent and incoherent scattering (Ulaby et al., 1982). The former is the scattered component in the specular direction while the latter is the scattered component in all directions. As the radar wavelength is shortened to less than a centimeter, the surface appears rougher and the surface coherent component vanishes (Ulaby et al., 1982). The surface incoherent component magnitude is small, and thus is concealed by the volume scattering which consists of only incoherent scattering. The backscattering coefficient at a smaller wavelength or on a rougher surface would be consisted of only incoherent components therefore appears as a volume-scattering medium. Simulations in Fig. 7 emphasize this contention showing a greater amplitude of the volume echo at a higher frequencies. We can therefore argue that the seasonal cycle of the observed $\sigma0$ at the Ka band is governed by the volume echo. This explains the peak of the observed $\sigma0$ in the winter at the Ka band over the AIS."

P7 L244: Do the authors mean to reference Fig. 8 here and not Fig. 3?

âĂć We are referring to the histograms of the date of maximum $\sigma0$ in Fig. 3.

P7 L246: Do the authors have any evidence for a seasonal cycle of snow surface roughness?

âĂć We have demonstrated that the volume echo is nearly constant at S band therefore can not explain the observed seasonal cycle. In the other hand, we have demonstrated that the seasonal cycle observed at the three frequencies can not be explained solely by the snow density changes. So, the only remaining reasonable conclusion is that the S band seasonal cycle stems from the surfaces roughness seasonal variations. Nevertheless, we do not have clear and consistent evidence of widespread seasonality in surface roughness.

A text has been added to clarify our argumentation (Line 260) :

"Therefore, it is likely that the seasonal cycle of the observed $\sigma0$ at the S band, predominantly driven by the surface echo, stems from the seasonal cycle of the snow surface roughness. There is no field observation that confirms this fact, but our findings suggest that such information would help to understand the altimetric signal in the future."

P7 L249: The authors state here that the seasonal variability in surface roughness is poorly known, therefore I'm not sure they can argue that it controls the seasonal cycle in the S band (please see my previous comment).

âĂć We have demonstrated that neither the volume echo nor surface snow density can explain the seasonal signal observed at S band. Since, the seasonal cycle observed at S band can only be explained by the surface echo which is related to the surface

roughness and density, one can inferred the seasonal cycle at S band to be driven by the surface roughness (please see our previous answer to this question).

P7 L250: I would suggest rephrasing point (iii) to make the argument the authors are trying to make clearer.

âĂć We have changed the text to (Line 264) :

"(iii) the relation between the surface snow roughness and density is complex because both variables are interdependent. The denser the snow surface, the larger the effect of surface roughness is. This amplification is due to the increase of the effective dielectric discontinuity with density (Fung, 1994)."

Figure 9: Should this figure have a colour scale? I would suggest a rework of this figure – it is not clear where the SP and WP zones are.

âĂć This map is a RADARSAT mosaic which is most objectively shown with gray scale. The rework of the previous figures should now make this one clearer.

P7 L256: The authors argue here that the WP zone maximum is due to the volume echo, but matches regions of megadunes and wind-glazed surfaces. I would appreciate if the authors addressed the following regarding this statement: (i) the Antarctic megadunes have surface features and sloped terrain on length scales similar in size to the radar footprint – how can the authors distinguish between the effects of surface and volume here? (ii) I would expect more backscatter in the summer over wind glazed regions due to the presence of large ice crystals near the surface, should that cause a backscatter peak in the summer in these regions?

âĂć Figure 4 shows that a greater backscatter comes from at depth in the WP zone. This means that the volume contribution is more important in the WP zones (see Armitage et al., 2014). Also, the presence of wind-glazed surfaces in these regions indicates that the surface varies very little over time due to the lack of snow accumulation related to the strong and persistent winds in the regions. Therefore, even if the surface component may be higher, the seasonal variations can only be ascribed to the volume component.

âĂć There will be more backscatter in the summer in these regions if only the volume echo was predominantly driven by the snow grain seasonal cycle. This is not the case. As we asserted on line 235 : The volume echo would be mainly driven by the snow temperature.

P7 L264-L266: Can the authors quantify this spatial coherence, or are they implying correlation from visual inspection? Are pixels with high seasonal wind speed amplitude correlated with the winter dates of high backscatter? I'm not sure I see the relationship looking at these plots, or from the average wind speed values.

âĂć Yes, we are implying correlation from visual inspection. With the rework of the figures, this visual coherence is hopefully more obvious and clear. Moreover, the computed correlation coefficient between the date of maximum of backscatter and the seasonal wind speed amplitude, after the interpolation of the date of maximum backscatter at Ku band on a 25 km grid cell (same to that of the wind speed dataset) by averaging with Gaussian weights considering all data points within a 25 km radius and weighting with a decorrelation radius of 10 km, is r = 0.4 (p<0.01).

âĂć The figure 1 shows the mean (blue) and median (red) seasonal wind speed amplitude with respect to the date of maximum backscatter at the Ku band. One can observe that high seasonal wind speed amplitude is correlated with the winter dates of high backscatter at the Ku band (between Julian days 175 and 275 (June to September)).

Can the authors also please expand on the seasonal amplitude of the wind speed – how is this obtained?

âĂć The correction has been done. We have added this details (Line 278) :

"To further investigate this point, we used ERA-Interim reanalysis wind speed data

supplied by ECMWF (European Centre For Medium-Range Weather Forecasts) on the period 2002 to 2010, corresponding to that of the Ku band. Equation 1 is used to compute the seasonal characteristics of the wind speed by replacing $\sigma 0$ with the wind speed. A visual inspection shows a high spatial coherence of the seasonal amplitude of the wind speed (Fig.Âă10a) patterns with the date of maximum $\sigma 0$ over the seasonal cycle at the Ku band (Fig.Âă2b)."

P8 L268: As per my previous comment, I am not sure of this correlation at Ka band either.

âĂć The rework of the figures should make this point clearer.

Figure 10: Please can the authors explicitly state the time period used in the caption. I find the elevation contours very difficult to make out, also.

âĂć We have used the time period of ENVISAT for the wind speed data (2002-2010). The caption has been modified to take into account the reviewer suggestion. We have deleted the elevations contours because, they are not necessary.

The caption now reads :

"Figure 10 : Seasonal wind speed amplitude (left) and average (right). Data are extracted from ERA-Interim reanalysis provided by ECMWF on a km2 grid cells, on the periods 2002 to 2010 corresponding to that of ENVISAT lifetime. Black contour lines delineate regions where the backscattering coefficient at the Ku band peaks before April. The star mark shows the location of the time series plotted in Figure 1. No observations are available beyond 81.5°ÂăS (black dotted circle)."

P8 L282: Isn't depth-hoar predominantly formed during the late spring and summer over these wind-glazed regions, according to Scambos et al., 2012?

âĂć Scambos et al. (2012) have conducted the in situ measurements the late spring and summer and observed a depth hoar formation caused by the sun light penetration in the smooth and polished surfaces of the snowpack. Champollion et al. (2013) have suggested a depth hoar formation at Dome C caused by a strong temperature gradient (positive) in the snowpack during the winter. Since there is a strong temperature gradient in the WP regions in the winter, depth hoar will develop.

“Champollion, N., Picard, G., Arnaud, L., Lefebvre, E. and Fily, M.: Hoar crystal development and disappearance at Dome C, Antarctica: observation by near-infrared photography and passive microwave satellite, The Cryosphere, 7(4), 1247, 2013.”

P8 L288: Over which time period were these grain size vertical gradients obtained? Over winter periods only or a multi-year average?

âĂć These grain size vertical gradients were obtained over a multiyear average from 1987 to 2002. The sentence now reads (Line 305) :

“For instance, Brucker et al. (2010) have found the highest vertical gradient in grain size, obtained over a multiyear average from 1987 to 2002, in the regions of the WP zone.”

P9 L308: “may therefore be a consequence of the presence or not of the wind-glazed areas” – I’m not sure what the authors are communicating here, I would suggest rephrasing this to make it clearer.

âĂć The correction has been done. The sentence now reads (Line 324) :

“The geographical patterns of the WP and SP zones are related to the seasonal amplitude of the wind speed. This is a result of the presence or lack of wind-glazed surfaces, induced by strong and persistent winds in the megadune areas. ”

Technical Comments:

Please find some technical comments below, but not all I have found are listed here. In my opinion the paper is in need of a thorough proof read, with a particular focus on grammar, sentence structure and the use of more precise language to increase readability.

âĂć Following the both reviewers technical comments, we have added and rephrased numerous sentences in the revised manuscript for readability. A special attention has been given to double-checking the english grammar, proofreading and sentence structure.

Title: Should read “. . .over the Antarctic Ice Sheet.”

âĂć As suggested by the reviewer, we have corrected the title.

P1 L13: Please rephrase this sentence to make this clearer.

âĂć We have rephrased the sentence as follow in the abstract (Line 12) :

“We identified that the backscattering coefficient at Ku band reaches a maximum in winter in part of the continent (Region 1) and in the summer in the remaining (Region 2) while the evolution at other frequencies is uniform.”

P1 L18: Should be: “At Ku band, which is intermediate. . .” and “. . .the seasonal cycle in the first zone is dominated. . .”

âĂć The correction has been made.

P1 L20: Should read “. . .should be taken into account for the more precise. . .”

âĂć The correction has been made.

P2 L60: Please rephrase this sentence for readability.

âĂć We have edited the sentence. The sentence now reads (Line 58) :

“The radar wave interaction with snow provides information on the snowpack surface and sub-surface properties, but it complicates the altimetric signal interpretation because the latter would be sensitive to many more snow parameters than if the signal only comes from the surface.”

P2 L65: Please rephrase this sentence for readability.

âĂć We have rephrased the sentence. The sentence now reads (Line 63) :

"The aim of this paper is to determine the prevailing snow parameters that drive the seasonal cycle of the observed backscattering coefficient at different radar frequencies and locations over the AIS."

P2 L78: "The footprint has around 5 km radius" Ân– please rephrase.

âĂć The sentence has been rephrased. The sentence now reads (Line 80):

"the satellite footprint has typically a 5Âăkm radius and no data were acquired above 81.5°ÂăS due to its orbit maximum inclination."

P3 L81: "To ensure post-ENVISAT mission. . ." this is incomplete, please rephrase this sentence.

âĂć The sentence has been completed as follow (Line 82) :

"To ensure a long and homogeneous time series with post-ENVISAT missions and to complement the Ocean Surface Topography Mission (OSTM)/Jason (Steunou et al., 2015), the Satellite for ARgos and ALtiKa (SARAL)/AltiKa was launched on 25th February, 2013, by a joint CNES-ISRO (Centre National d'Etudes Spatiales - Indian Space Research Organisation) mission, on the same 35-day repeat cycle orbit as ENVISAT."

P4 L148: Should this heading have a section number?

âĂć No, this heading has not a section number.

P6 L218: Please rephrase this sentence for readability.

âĂć The correction has been done. The sentence now reads (Line 227) :

"This sensitivity is explained by the fact that increasing snow temperature increases absorption resulting in a decrease of the radar wave penetration in the medium, thus limiting the volume echo."

P7 L250: ". . .interdependent and linked. . ." is a tautology, please rephrase

âĂć The correction has been made, and the redundant word has been removed. The sentence now reads (Line 264) :

"(iii) the relation between the surface snow roughness and density is complex because they are interdependent. The denser the snow surface, the larger the effect of surface roughness is. This amplification is due to the increase of the effective dielectric discontinuity with density (Fung, 1994)."

P8 L294: "The radar altimeter remaining on the same tracks. . ." is referring to two different satellites here, I would suggest rephrasing.

âĂć The sentence has been rephrased as follow (Line 311) :

" This study, using 35-day repeat radar altimetry data, allowed to carry out this spatial and temporal comparatives analysis of the seasonal amplitude and date of maximum $\sigma 0$ at the S, Ku and Ka bands."

P9 L314: Should read ". . .are the key to improving. . ."

âĂć The correction has been done.

References:

Armitage, T.W.K. et al. (2014), Meteorological Origin of the Static Crossover Pattern Present in Low-Resolution-Mode CryoSat-2 Data Over Central Antarctica. IEEE Geoscience and Remote Sensing Letters. 11(7),pp.1295–1299.

âĂć This reference has been cited.

Davis, C.H. and Ferguson, A.C. (2004), Elevation change of the Antarctic ice sheet, 1995-2000, from ERS-2 satellite radar altimetry. IEEE Transactions on Geoscience and Remote Sensing. 42(11),pp.2437–2445.

âĂć This reference does not fit with the scope of this paper.

Nilsson, J., et al. (2016), Improved retrieval of land ice topography from CryoSat-2

data and its impact for volume-change estimation of the Greenland Ice Sheet, The Cryosphere, 10(6), 2953.

âĂć This reference is already cited in the original text.

McMillan, M., et al. (2016), A high-resolution record of Greenland mass balance, Geophys. Res. Lett., 43, 7002–7010, doi:10.1002/2016GL069666.

âĂć This reference has not been cited.

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2017-238, 2017.

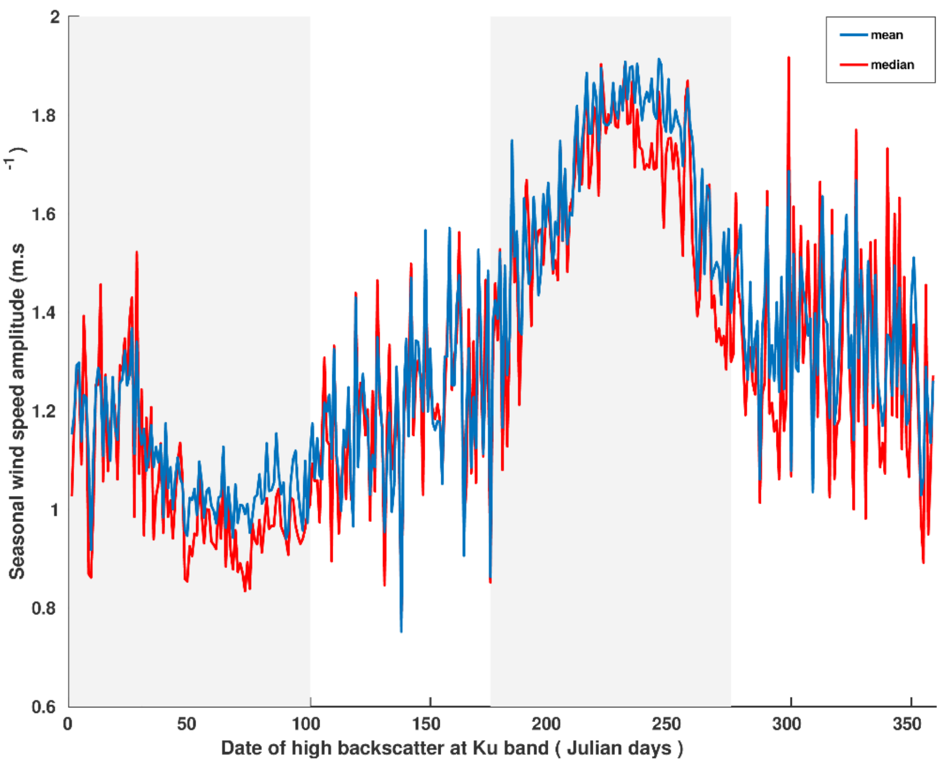

Fig. 1. Histogram of wind speed seasonal amplitude with respect to the Ku band backscattered coefficient date of maximum

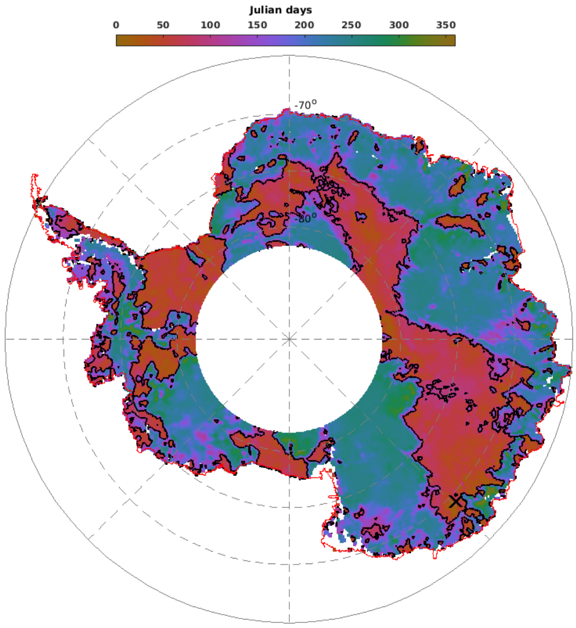

Fig. 2. examples of the new color scale, and boundaries of WP and SP