Author's Response to Referee #2 comments on "Snow depth uncertainty and its implications on satellite derived Antarctic sea ice thickness" - Price et al.

We thank Referee #2 for their review and detailed comments. Our responses are below after each point made by the referee and are highlighted in bold.

1. What the authors are producing is unlikely to be true sea ice thickness, but rather some representative parameter. The factors influencing radar penetration and freeboard in the Antarctic are numerous and are still not well understood (the Willatt et al. (2010) paper remains the key study on this subject). This is partly addressed this by applying different snow penetration depths in their freeboard to thickness conversion, but this solution which will not capture spatial variation in penetration, or temporal variation if the solution were to be used in different months. More transparency is needed that the retrieval of "thickness" is still highly problematic, and there are limitations in this approach. It should be stated early in the manuscript that "freeboard" is radar freeboard rather than ice freeboard, as this may not be obvious to the wide readership that the paper will attract.

We thank the reviewer for this comment and other detailed considerations about radar altimetry through the paper. We agree that the paper does not sufficiently describe the complexity of microwave interaction over Antarctic sea ice but have suitably pointed to the literature that does and have now included references to surface roughness studies (see response to point P2 L51-53). We agree that the freeboard terminology was loose and have amended it accordingly developing the terminology from the initial 'radar freeboard' to the freeboard we are using to derive thickness (see response to point P4 L135).

We have attempted to cater for the range of sea ice thicknesses that the ESA radar freeboard could represent by altering the surfaces it could be in the thickness equations. We understand that this is a simple approach regarding the complex interaction of the radar energy with the surface, but seeing as the Level 2 product is actually only a surface height there is little else that can be done, except vary where this surface height might be. We agree that if we were approaching this from an earlier stage in the processing we would need to take account of all the variables in our own retracking procedure, however we are not doing this. Here, it is simply not required. Please see responses to 'P2 L51-53' and 'P5 equations' for more detail.

2. The comparison of the various CS-2 sea ice thickness results with in situ data is not sufficient to conclude that any of the CS-2 data show good agreement with in situ data (as suggested on P11 final sentence). Figures 4 and 5 and related discussion provide an initial and basic comparison of sea ice thickness results, but cannot be considered an evaluation of the product in any way. In general, some clarification is needed for this analysis:

We disagree with the reviewer which could be based on a misunderstanding. We attempt to clarify below:

We have taken the retracked elevation provided by ESA which provides us with surface height. To assess the range in inferred thickness, as a result of the uncertainty surrounding what surface the radar freeboard represents, we used different assumptions about where this surface is and vary these assumptions between the air-snow and snow-ice interfaces. Our analysis (Fig. 5 (in new manuscript)) shows this range of thicknesses

as trends through the year and essentially shows the uncertainty in the CS-2 inferred thickness from the ESA product from both the freeboard uncertainty and a snow product uncertainty. We argue that this is an evaluation of a sea ice thickness product as derived from CS-2 measurements. The study does not attempt to build a retracking procedure of its own, but is simply using the information available from the ESA L2 product. As there is no more information on what the radar freeboard represents, there is nothing else that can be done. However, by varying the penetration depth (or the range of surfaces the ESA retracked heights could represent) we find using the interpolated in situ dataset (the best available 'snow product') that CS-2 derived thickness, assuming a penetration of 0.07 m, agrees with in situ mass equivalent thickness within 0.02 m. This is a very good result, but we are not suggesting that this can be universally applied but do argue that it is an evaluation of the ESA L2 product. We are open to further discussion about what else the reviewer suggests can be done with the L2 product, or why it is not considered an evaluation of the L2 height product.

P10 final sentence: Wording suggests that all CS-2 and in situ thicknesses are mass equivalent thickness. However, it doesn't appear this way from Figure 4 which shows in situ measurements in November falling below the mean mass equivalent thickness. It should be clear in the text what thickness is being plotted/compared. If in situ (red) and CS-2 thicknesses are not equivalent than this assessment needs to be repeated.

The referee is correct to identify the difference between these two different thicknesses. In Figure 4 both these types of thicknesses are plotted, the mass-equivalent thickness (inclusive of the influence of the sub-ice platelet layer) represented by the black plus-sign and actual solid sea ice thickness represented by the red line. This red line is a linear fit between measurements taken of solid sea ice thickness in July and November. This will be thinner than mass-equivalent thickness as it does not account for the sub-ice platelet layer and is a direct measure using a tape measure of the consolidated solid sea ice. CS-2 thickness is only comparable to mass-equivalent thickness (black plus-sign) as the freeboard measurement will be influenced by the buoyant force of the sub-ice platelet layer (Price et al. 2014). Therefore, for comparison of in situ thickness to CS-2 thickness the reader should only take note of the difference between the black plus-sign and different CS-2 thickness with varying *Pd* values. The red line is an additional resource to give the reader an idea of the expected sea ice growth rate as an additional comparison to the CS-2 thickness trends.

We have made the following changes to give clarity on the different thicknesses:

End of the first paragraph in section 5: "*The only exception to this is the red line in Fig.4 which is a linear fit between two measurements of consolidated sea ice thickness in July and November 2011 used here to show an expected sea ice thickness growth rate for comparison to CS-2 thickness trends.*"

Figure 4 caption, sentences amended to: "*The red line shows sea ice thickness from in situ measurements of consolidated sea ice thickness with a tape measure taken in July and November in one location in the south of McMurdo Sound joined assuming a constant growth rate. The black plus sign is the mean 'mass-equivalent thickness' from all in situ measurements in November. This is slightly thicker than the end of season thickness indicated by the red line given it takes account of the influence of the sub-ice platelet too.*"

***This is what CS-2 derived thickness should be compared to as the freeboard measurement from the satellite will also be affected by the buoyant influenced of the sub-ice platelet layer.*"**

I assume that July and November in situ thicknesses are spatial means for those months, but it's not stated in the text.

This needed clarification. The line is simply a linear fit between measured sea ice thickness in July and measured sea ice thickness at one location in McMurdo Sound. This has been used in the plot to show the growth rate during this period for comparison to the CS-2 thickness trends. This has been clarified with the amendments made in response to the previous comment.

Figure 4: The caption gives the first mention of in situ sea ice thickness measurements being taken in July. This should be included briefly in section 2.1, as surely the July and November data are not being averaged over the same area.

In addition to the amendments above this has been included in section 2.1: "*Two more in situ measurements of sea ice thickness are included in the analysis. These are two measurements taken at one location in McMurdo Sound in July and November. Assuming a constant growth rate between these measurements they are used in section 5 as a comparison to CS-2 inferred sea ice growth rates. More detail on how the in situ thickness measurements are used and how they should be interpreted is provided in section 5.* "

Further comments:

Title and abstract: The title is too broad – it suggests that the scope and study area of the paper are far wider than what is presented. The abstract also needs to state that the study was limited to fast ice in McMurdo Sound.

We agree the title is too broad and have changed it to:

'*Antarctic fast ice thickness from CryoSat-2 using different snow product information*'

McMurdo Sound and fast ice are now included in the abstract.

P1 L27: Understanding of what?

This sentence has been amended to: "*The knowledge of Antarctic sea ice extent, area, drift and roughness have been greatly improved over the last forty years, principally supported by satellite remote sensing.*"

P1 L42-43: Move all discussion on snow depth assessments to next paragraph, which addresses it in more detail. Seems out of place here.

Moved sentence "*Dedicated basin-scale snow depth assessments are available (Markus and Cavalieri, 2006) but continual improvements in our monitoring ability are key to support the current ESA satellite altimeter missions, CryoSat-2 (CS-2) and Sentinel-3 and NASA's planned ICESat-2 expected to be operational in late 2018.*" to next paragraph.

P2 L51-53: I disagree with point 2, that the retracking procedure is a principal source of error in thickness estimates via snow. The presence of snow will slow radar propagation but the waveform shape will be dictated by the roughness of the reflecting surface. The principle of retracking is to select a given location on this waveform that corresponds to "the surface" at nadir without knowledge of its exact location. This is why the ESA L2 product is considered radar freeboard rather than ice freeboard. Therefore, it is the assumed radar penetration that contributes to the error (up to the user), rather than the waveform retracking procedure applied.

We agree with the reviewer that this needed re-wording. The sentence has been amended to: "*2. Uncertainty about what surface the retracking point on the radar waveform actually represents between the ice freeboard and snow freeboard. This initial measurement is commonly referred to as radar freeboard.*"

We have added a statement and reference directed at surface roughness in section 2.4:

"*It is clear that the presence of snow influences the CS-2 height retrieval but precisely how is dependent on the surface roughness (Kurtz et al., 2014; Hendricks et al., 2010; Drinkwater, 1991), its depth (Kwok, 2014) and its dielectric properties (Hallikainen et al., 1986).*"

References added for surface roughness inclusion:

Hendricks, S, Stenseng, L, Helm, V and Haas, C (2010) Effects of surface roughness on sea ice freeboard retrieval with an Airborne *Ku*-Band SAR radar altimeter. In *International Geoscience and Remote Sensing Symposium (IGARSS 2010), 25–30 July 2010. Proceedings*. Institute of Electrical and Electronics Engineers, Piscataway, NJ, 3126–3129. doi: 10.1109/IGARSS.2010.5654350.

Drinkwater, M. (1991) *Ku*-band airborne radar altimeter observations of marginal sea ice during the 1984 Marginal Ice Zone Experiment. *J. Geophys. Res.*, 96(C3), 4555–4572, doi: doi.org/10.1029/90JC01954.

The reviewers comment supports the method of the paper. As "the surface" at nadir is undefined in the ESA product (i.e. no indication is given of where it might be) and it is essentially left to the user. The only way for the user to establish what it represents is to compare it against in situ measured freeboard and thickness. In the manuscript we alter the possible positions at which it could be between the air-snow and ice-snow interface and estimate thickness accordingly. This analysis presents the range of uncertainty as presented by the ambiguity of the radar freeboard and the current inability to accurately define at what point above sea level the retracked surface height represents.

P2 L62-66: It is not clear from the author's description that the assumption of zero ice freeboard is only applicable to laser altimetry, where the snow surface is believed to be the dominant scattering horizon. There is no evidence for this being true with radar altimetry, which is why no hemisphere-wide Antarctic sea ice thickness results have been published for CS-2.

Good point, "*Using laser altimetry*" added at the beginning of the sentence.

P2 L74-76: Confusing sentence structure.

Sentence amended to: "*The high-resolution model results are compared to snow products from two other independent datasets, the first ERA-Interim precipitation and the second satellite passive microwave snow depth from AMSR-E.*"

P2 L78: "CryoSat-2" to "CS-2"

Amended.

Section 2.1: Please provide comment on how many snow density, ice freeboard and ice thickness measurements were made at each site

Sentence amended to: "*This involved sea ice thickness, freeboard and snow depth/snow density measurements at 39 sites. Freeboard was measured 5 times in a cross profile at each site, once at the centre of the cross and once at the terminus of each line, as was thickness. Mean snow depths for each in situ site represent 60 individual snow depth measurements over that same cross-profile at 0.5 m intervals. Snow density was measured at 18 sites, well distributed across the area, the mean of these sites is used for this analysis. A full overview of the measurement procedure is provided in Price et al. (2014).*"

Section 2.2: Not all ice comprising the "large areas" will appear on the same day, so how is the exact date of fast-day-zero established?

We have added this sentence to provide more detail in section 2.2: "*By comparing motion and patterns between sequential images we are able to identify three areas that froze independently of one another.*"

The reviewer is right to question the accuracy of the fastening dates and we have addressed this by including this in section 2.2 "*The largest gap in the Envisat image string is 8 days but no large gaps are around key fastening dates. The typical spacing is 1-2 days so we have confidence we have reduced our error in the fastening date to less than 2 days.*"

The term fast-day-zero has been removed from the paper as this just caused confusion.

P4 L1117-118: Provide a brief (just a sentence will do) summary of how gridded snow depth values are calculated from spectral gradient ratio

More detail provided and this section has been reworded as: "*Gridded snow depth values are calculated using the spectral gradient ratio of the 18.7 and 36.5 GHz vertical polarisation channels. For snow free sea ice the emissivity is similar for both frequencies. Snow depth increases attenuation from scattering and it is greater at 36.5 GHz than at 18.7 GHz, resulting in increased brightness temperatures at 18.7 GHz (Comiso et al., 2003, Markus and Cavalieri, 1998). Using coefficients derived from a linear regression of in situ snow depth measurements on microwave data and a 36.5-18.7 GHz ratio corrected for sea ice concentration snow depth can be estimated (Comiso et al., 2003).*"

P4 L135: Define "SIN" for readers who may not be familiar with CS-2 data

Sentence amended and new sentence added: "*The instrument has three modes and operates its interferometric (SIN) mode in the coastal Antarctic. This mode uses both of the satellites antennas to identify the location of off-nadir returns accurately.*"

P4 L135: ". . . **radar** freeboard measurements. . ." Here would be a good place to highlight that freeboard is radar freeboard, rather than sea ice freeboard. Therefore, "thickness" is just a representative parameter rather than true sea ice thickness.

We have provided more clarity on exactly what is being represented by our references to freeboard through the paper. Here we have started with: "*The ESA L2 baseline C SIN mode (SIR_SIN_L2 – available at: http://science-pds.cryosat.esa.int/) data set provides a retracked height for the surface over sea ice and this initial measurement is termed radar freeboard.*"

Following this in the same section (2.4):

"*Each CS-2 radar freeboard measurement is cross-referenced to freeze-up areas 1, 2 and 3 and assigned a snow depth (Ts) value from the described snow products.*"

"Given this uncertainty we apply a simple methodology to discover the range of thicknesses as inferred via this CS-2 data. We explore this possible range by using a varying penetration depth (Pd) into the snowpack. Equation 1 assumes that the snow surface is detected, equation 2 that the sea ice surface is detected and equation 3 that an arbitrary surface at incremental Pd values into the snow pack represents the retracking point varying from 0.02 m to 0.50 m (or to the snow-ice interface, whichever criteria is met first). The radar freeboard is corrected when snow is present and penetration is assumed (i.e. Pd > 0) for the reduction of the speed of the radar wave through the snow pack following the procedure described in Kurtz et al (2014). We derive sea ice thickness (Ti) using the newly corrected freeboard (Fb) and the described equations;"

All the following references to freeboard follow the same logic i.e. they are corrected radar freeboard (*Fb*).

It should also be noted that 'true sea ice thickness' is essentially always a representative parameter (within the error of all inputs) from altimetry. This is absolutely the case if the mean backscattering horizon represented by the radar freeboard is unknown. That is the main purpose for trying different horizons in this paper and establishing the range in this representative parameter of sea ice thickness.

P4 L143: Was 0.5 m chosen from in situ measurements or otherwise?

Yes, it was selected from in situ information in 2011. "*(as measured in situ in 2011)*" added to the end of this sentence.

P4 L145: Again, the authors can't be sure

We have included a description of what we mean by freeboard earlier in the paper as suggested in the comment above (comment P4 L135). What freeboard is in the paper is now established throughout.

P5 L157: See comment on P2 L51-53. The suggestion that the retracking procedure itself introduces uncertainty is misleading. The purpose of the ESA product is to provide range and freeboard to the "surface" at nadir. It is up to the user to decide what that surface is.

See response to P2 L51-53.

P5 equations: I appreciate the authors consideration of differing penetration depths on Antarctic sea ice retrievals. However, a large number of factors influence radar propagation over Antarctic sea ice (ïňˇCooding, icy layers, depth hoar, snow ice, crust, sea ˊ water wicking etc). Which of these factors has the dominant impact on radar reflection will depend on the age and depth of snow on sea ice. Therefore, penetration depth is unlikely to be constant even over relatively small areas and a more representative way to vary penetration would be through varying penetration depth by a percentage of snow depth (say 25%, 50%, 75%). Why did the authors not choose that approach for this study?

If we have understood the question correctly, we absolutely agree with the reviewer about the complexity of radar interaction and that penetration depth will be variable, even over small areas. However, we do not agree with the proposed percentage approach.

If we take a given percentage then penetration universally increases into the snowpack with increasing snow thickness. This is contrary to evidence in the literature, especially if the surface roughness and complexity of the snow structure are increasing over time (i.e. grain size, layering). We could vary the penetration through the season, starting at 75% and decreasing toward 25% at the end of the season but we have no data to support at what rate these percentages should decrease, so we choose a fixed depth. There are pros and cons to each approach but we think a percentage approach could actually introduce additional uncertainty, whereas a fixed depth gives us the range in sea ice thickness through the growth season through many of the potential horizons.

In further support of our approach our observations show that the snow surface and volume in the area of investigation are very uniform. We therefore believe that the radar penetration over larger areas is relatively constant (certainly in comparison to pack ice) and given the high latitude and non-summer months included in the analysis maybe even shows little change over time. We appreciate the need for more comprehensive interpretation of the waveform and the use of detailed statistics from the ground to aid this procedure, but that is beyond the scope of this study and requires better data to inform the procedure. Surface roughness information at the radar wavelength scale is required (radiometric roughness) and larger surface roughness features (geometric roughness) along with detailed information on snow depth, layering, grain size and wetness. Ideally this would all be completed and compiled as statistics that represent each CS-2 radar footprint specifically (i.e. georeferenced to the full 380 m x 1560 m footprint).

All that the user has to work with from the level 2 product is surface height. The authors do not see the value in attempting to include the complex variables that influence the

waveform at this stage. This should all be considered when the retracking is done. All we are doing here is varying where the surface height could be between the air-snow and snow-ice interfaces (inclusive). We compare the assumptions to the in situ thickness and establish (i) the range in thickness estimates associated with the uncertainty and user based interpretation of the ESA radar freeboard and (ii) identify at what assumed freeboard interface produces the thickness closest to in situ measured thickness.

Also as the snow depth principally influences the width of the waveform and trailing edge an algorithm based on a percentage of snow depth isn't ideal either (but this isn't even relevant because the retracking is already complete i.e. all we have is a surface height).

With the percentage approach our time series assessment (new Fig. 5) will have a constantly changing penetration which will make the analysis more difficult to interpret.

In section 2.4 we have also added a sentence which clarifies that if the *Pd* is higher than the snow depth, then we assume full penetration to the snow-ice interface as this needed clarification.

"*We explore this possible range by using a varying penetration depth (Pd) into the snowpack. Equation 1 assumes that the snow surface is detected, equation 2 that the sea ice surface is detected and equation 3 that an arbitrary surface at incremental Pd values into the snow pack varying from 0.02 m to 0.50 m (or to snow-ice interface, whichever criteria is met first).*"

Section 3.1: Provides a very nice, clear introduction to SnowModel

No action taken.

Section 3.2: More information required on the use of ERA-Interim reanalysis data. 1.) Is this the total precipitation 2.) Are there any temperature constraints on what falls as snow 3.) Why is evaporation not considered

This is total precipitation reported in mm water equivalent. We have not considered temperature in the ERA-Interim precipitation analysis. From April, at this latitude we have assumed all precipitation is snow. We have not considered evaporation as we expect it to be non-existent from April-November or certainly negligible. We have only observed melting on snow on sea ice in McMurdo in December and this month is not included in the analysis. Sublimation is possible and expected once the sun rises in austral spring (first sunrise 19th August) but we have no way of accurately including this in the snow mass balance analysis for ERA-Interim data.

Figure 4: Make penetration depth labels larger

Label sizes increased and also moved off the plot for easier interpretation. Dashed lines also changed to solid line with colour gradient between the two extremes of air-snow and snow-ice interface. See all changes in amended Fig. 5 in the new manuscript.

P14 L405: Specify ICESsat-2 footprint, and CS-2 footprint earlier in the manuscript

CS-2 footprint now given in section 2.4.

ICESat-2 footprint mentioned in the discussion as it is not relevant when the satellite is described earlier in the manuscript. Sentence in discussion amended to "*These meter-scale features will be important to capture, especially to support compatibility with smaller satellite altimeter footprints, in particularly ICESat-2 with an expected 0.7 m along-track sampling rate (Abdalati et al., 2010).*"

Reference added: Abdalati, W., Zwally, H.J., Bindschadler, R., Csatho, B., Farrell, S.L., Fricker, H.A., Harding, D., Kwok, R., Lefsky, M., Markus, T., Marshak, A., Neumann, T., Palm, S., Schutz, B., Smith, B., Spinhirne, J and Webb C. 2010. The ICESat-2 laser altimetry mission, Proceedings of the IEEE, 98(5), 735-751. doi:10.1109/JPROC.2009.2034765.

P15 L478: Penetration can also vary spatially over small study areas (see Willatt et al., 2010), which is why a percentage penetration factor may be more applicable than fixed depth.

See response to 'P5 equations' above.

L506-507: "at least as reliable" is a strong statement, and not proved in the manuscript, considering the authors did not show overlap of AMSR-E snow depths compared with in situ.

Agreed this statement is too strong, amended to: "*With improvements to redistribution mechanisms and adequate representation of the effect of topographic features atmospheric models could be used as an alternative to contemporary passive microwave algorithms.*"

Conclusion: It would be good to finish with a statement regarding the potential for Antarctic-wide application of SnowModel (and limitations) for sea ice thickness retrievals, as the paper title suggests.

Sentence added: "*If these two variables can be adequately incorporated SnowModel could provide a valuable resource for snow and sea ice thickness investigations over the wider Antarctic sea ice area.*"

<u>**References**</u>

Price, D., Rack, W., Langhorne, P. J., Haas, C., Leonard, G., and Barnsdale, K.: The sub-ice platelet layer and its influence on freeboard to thickness conversion of Antarctic sea ice, The Cryosphere, 8, 1031-1039, https://doi.org/10.5194/tc-8-1031-2014, 2014.