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The Cryosphere Discussions
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Interactive comment on "Snow depth uncertainty and its implications on satellite derived Antarctic sea ice thickness" *by* Daniel Price et al.

Anonymous Referee #2

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This paper introduces a novel approach to deriving snow depth on sea ice in McMurdo Sound using a high-resolution model. The model is compared to other snow depth retrieval methods and their impact on CryoSat-2 sea ice thickness estimates is briefly discussed. The study of different snow depth retrievals is thorough and I'm pleased to see new solutions being developed in the Antarctic. However, I have a couple of significant concerns that need be addressed before the paper can be considered for publication.

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

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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.

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 can not be considered an evaluation of the product in any way. In general, some clarification is needed for this analysis:

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.

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

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.

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

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the study was limited to fast ice in McMurdo Sound.

P1 L27: Understanding of what?

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

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.

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.

P2 L74-76: Confusing sentence structure

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

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

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?

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

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

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.

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

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

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

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 (ïňĆooding, icy layers, depth hoar, snow ice, crust, sea 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?

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

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

Figure 4: Make penetration depth labels larger

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

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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

P16 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 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

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