# Interactive comment on "Evaluating Airborne Ku-Band Radar Altimetry over Landfast First-Year Sea Ice" by Paul Donchenko et al.

**Anonymous Referee #2**

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Dear Authors

The following comments reflect my observations on the TC manuscript "Evaluating Airborne Ku-Band Radar Altimetry over Landfast First-Year Sea Ice" by Donchenko, King and Kelly. The study focused on quantifying the impact of snow properties and surface characteristics, and its impact on the accuracy and penetration of the TFMRA retracking algorithm, operationally used for sea ice thickness estimations from radar altimetry.

This study is relevant to the scientific community with respect to improving our understanding of how snow critically impacts the accuracy of satellite radar altimeter-derived sea ice thickness estimates from presently operational and forthcoming radar altimeters such as CryoSat-2, ALtiKa and CRISTAL missions.

Although the study focuses on the uncertainties, after thoroughly reading the paper, I was left with even more uncertainties and research gaps (mainly due to unclear methodology and lack of rigorous analysis and discussion). The manuscript, as it stands, reads 'incomplete', and requires significant major addition and revision, before it can be deemed worthy of publication in TC.

I am willing to review the revised manuscript once all my overall general comments are addressed. I would like to keep the review short to addressing major concerns in the manuscript, before delving into the editorial comments (probably in the next round of review). Please see my general comments below, which covers my major concerns.

a) My main issue is Figure 5 (which is the corner stone of this study, correct?), and how you arrived at Figure 5 (no methods mentioned), what data was used, and no proper discussion (or even analysis) of the different snow/surface parameters you used to correlate with the TFMRA thresholds. a1) Although, the authors do vaguely mention about the different snow pits dug during their campaign, there is no explanation of these snow pits where you need to demonstrate how the different snow covers looked like in terms of vertical profiles of properties such as density, temperature, salinity, grain size etc. How are the snow pits spatially different? Were snow pits cold or warm? (because snow temperature also affects the radar penetration, especially at high frequencies correct?). Were the snow pits vertically homogeneous? (any layering or ice lenses present?). You mention snow salinity as a critical factor affecting the retrievals. Were the snow salinity profiles typical of FYI?

In summary, since this study depends heavily on the snow properties, the authors needs to spend time to decipher your snow pits.

a2) On that note as a follow up to a1), if your snow pits are diverse, then I am curious to learn how the radar penetration proportion through snow is affected by the variability in the snow properties. Yes, you do show the range in Figure 5. But I cannot believe the numbers. For example, if you look at the grain size estimates, from a range of 1.93

cm to 2.42 cm (which is insanely high), I didnt see a standard deviation. For a high frequency such as Ku-band, I cannot believe (sorry) that the penetration is the same for these two extreme snow grain sizes. The same is applicable for snow salinity. You labelled in the plot as 'no salinity' and 'salinity present'. I think this information is very vague. 'salinity present' can be 0.00001 psu/ppt correct? Linking back to a1), the authors need to be clearly mention the range of these properties and clearly demonstrate the sensitivity of these properties.

a3) I am not clear about how (more important) and why the authors used a layer weighted average (Line 108) method to summarize the pits. That instantly shows the flaw in your analysis, which needs to be rectified (linking a1 and a2).

b) My second problem is the methods section. There is no description or flowchart of how the observations and modeling was employed. This causes the reader to go clueless about how the results and analysis were conducted.

b1) The authors has already done a good job introducing the radar measurements. However, they need to showcase how the snow properties were used to interpret observations, and hence produce Figure 5 and its analysis.

b2) It would b great if the authors can show the readers how the environment (sea ice) and distinct snow pits looked like (they mention about snow pit faces?).

c) My next concern for now is the discussion section. From my initial reading of this section, it seems the authors blindly recommends improvements to be made for TFMRA, without a proper discussion of the results. I see this section more as a 'Future recommendations' section than a discussion of the results. With a significant room for increasing the word limit (in regards to page length), the authors should refocus on the analysis and presentation of results (linking a)) and then discuss the results and also MENTION the study limitations also.

d) My last concern is definitely the introduction section. Its short (that's still fine with

me), but lacks guiding the reader through the key concepts (including the role of snow in sea ice thickness retrievals) and particularly focusing on the ambiguities and uncertainties. Yes, the authors do talk about it, but only vaguely. I am also curious to see a literature review (brief) of how different snow properties and ice types (not just FYI) affect the ice thickness retrievals. That will anyways help the authors to have a strong platform from where, they can introduce the rationale, research gaps and objectives.