

## ***Interactive comment on “Simulated Ka- and Ku-band radar altimeter scattering horizon on snow-covered Arctic sea ice” by Rasmus T. Tonboe et al.***

### **Anonymous Referee #1**

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#### — Summary —

In this study the authors use a multi-layer radar scattering model to simulate Ku- and Ka-band radar penetration into snow on winter Arctic sea ice. The model is forced with snow depth and density data from the ESA RRDP, and then geophysical snow parameters collected in situ. The authors conclude that the Ka- and Ku-band track point difference is a function of snow depth. While the manuscript was generally well-structured, I found that clarity was lacking in parts. I summarize this in my general comments below and also list some more specific comments. These should be addressed before publication. When completed, the manuscript will be highly relevant

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to the sea ice remote sensing community, with the dual-frequency (Ku- and Ka-band) CRISTAL satellite being a high-priority Copernicus candidate mission.

#### — General comments —

> The presentation of results, and related discussion, is not always clear. This issue starts on P12. The paragraph was too long (a whole page), and still didn't sufficiently explain what is being shown in Figure 6. The contents of Figure 6 need to be clearly described (both in the text and caption) before the authors try to present an analysis. It may help to rearrange this paragraph and the following paragraph into three paragraphs providing 1.) a description of what is shown in Figure 6, 2.) analysis of the RRDP model runs, 3.) analysis of the profiles summarized in Table 2.

> The conclusions presented in Section 7 are not sufficiently justified. On P17 L400-401, the authors state that they advocate avoiding the use of a snow climatology. However, in the above paragraph they state that "the snow climatology results in a small impact on the derived sea ice thickness" and "The small impact of the snow on the measured freeboard is the reason why the sea ice thickness can be derived using radar altimeters even without actual snow information". This collection of statements seems contradictory. I'd like a more clearly structured argument and justification of the conclusions.

> I felt like the authors could "sell" their model a little more. There is novelty in their multi-layer approach and use of in situ profiles, so they should really highlight that!

#### — Specific comments —

P1 L19-20: Make it clear that OIB and climatology are used only as part of the RRDP data, and not as separate datasets

P1 L28-30: "radar freeboard", rather than "measured freeboard"? Either way, this sentence needs to be re-worded for clarity.

P2 L40-42, L45-46: It's confusing to state that geophysical properties of the ice impact

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the radar scattering horizon. I know what the authors mean, but in remote sensing terminology, "scattering horizon" is commonly used to refer to a location within the snow pack. Therefore, a more accurate statement is that geophysical properties of ice affect radar height estimates, as height estimates depend on radar scattering horizon in the snow, and floe buoyancy. This is an important difference, which should be explained and then maintained throughout to avoid confusion.

P2 L50: Ricker et al., 2014 is an excellent paper. However, it is cited extensively throughout this manuscript. The authors should note that multiple other publications are also relevant, and some more so as they were published earlier.

P2 L55: This makes it sound like the MYI isn't sampled at all. Re-phrase to e.g. "height estimates dominated by FYI" or similar.

P2 L60: Hendricks et al., 2016 reference; you've listed all the products above already. Be care not to show too much of an AWI bias.

P5 Fig. 1 and Fig. 2: It'd be great to see these mapped too, to get a better representation of values over regions associated with different ice types

P6 L137: "These profiles were sampled from relatively smooth, land-fast FYI..." This information is repeated below so no need to include it here.

P6 150-151: State here why snow correlation length is important, otherwise its inclusion in Figure 3 is confusing until you read much further on

P6-7 Profiles 1-5: Include correlation length in profile descriptions

P7 L178-180: Final sentence is wordy and quite confusing

P8 L186: "...surface\*\*/interface\*\*..."

P9 Eqn (2): How does the model account for the different radar frequencies? This is a key principle of the paper and as far as I can tell, the information is missing. It needs to be really spelt out for those of us who are familiar with remote sensing, but not so

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much with radiative transfer modelling.

P9 L214: Length scale of "smooth patches"? I assume they mean "smooth" on the order of the radar wavelength.

P10 L240: How do they calculate the noise floor?

P10 L243: Expand on what you mean by "surface scattering", i.e. from which surface (snow, ice, somewhere in-between)

P10 L243-244: I would like more justification as to why a 50% threshold was chosen. In the manuscript they mention their own 2010 paper and the Ricker et al., 2014 paper, but there are many other studies that suggest a different threshold is preferable.

P11 L276-277: Stating that the tracking point difference is "mostly insensitive" to snow depth could be misleading, when differences can reach up to 8 cm. This would have a significant impact on sea ice thickness estimates. In fact, I would just get rid of this sentence.

P11 L279: Change "smaller snow correlation lengths" to "fine-grained (0.1 mm) snow depths" or similar, for consistency with the rest of the paragraph.

Figure 5: Belongs at the end of P11. This is a clear and interesting plot. However, it may not be the most insightful for sea ice altimetry applications. More useful would be a figure showing the fraction of penetration as a function of snow depth (and density), for Ku and Ka separately. This could be included as a second panel.

P15 L342-345: This description should be included when Figure 6 is first introduced

Figure 6: I can't make out the black crosses, and the legend covers some data. It'd also be useful to number the profiles again.

P17 L388-389: Isn't adding to an unknown an impossibility? This needs to be explained better.

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— Technical comments —

P2 L36: “UCL” -> “CPOM”

P10 L255: “. . .\*\*Ka and Ku\*\* waveform. . .”

P11 L264: “. . .OIB snow depth and the Warren et al. (1999) snow density \*\*pairs\*\* . . .”

P12 L312: Define “\*\*second-year ice\*\* (SYI)”

P15 L352: “\*\* . . .Ka and Ku\*\* track point. . .”

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2020-196>, 2020.