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

Anonymous Referee #1
Received and published: 16 September 2020

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

Thank you for your review and pointing out where the MS could be improved. Responding to your comments has definitely improved the MS.

— 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.

We don’t believe we have presented contradicting statements. We think it is not necessary to include a bias in sea ice thickness estimation, even if it is small. The reason why this practice has been going on for so many years is because of the small bias and the difficulty to detect. We understand why reviewer 1 makes this statement and we have added an additional clarifying sentence in the Conclusion to further describe what we are intending to say.

> 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!

Thank you for this comment. We have added some additional emphasis to this point in our Conclusion and Abstract.

— 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

It has been reformulated in the revised manuscript, so that it is clear that the OIB data and climatology are part of the RRDP. "(CCI) round robin data package, where NASA's Operation Ice Bridge (OIB) data and climatology are included,"

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

We have changed "measured" to "radar", and explained that the radar freeboard is a combination of radar scattering and buoyancy.

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

Thank you for this comment. Scattering is dominated by either the dielectric or surface roughness mismatch at interfaces such as the air/snow and the snow/ice interfaces. In general, scattering occurs at several vertically distributed interfaces as well. However, the track point or scattering horizon is not a real surface and that is why we prefer to call it the 'track point' because then it relates to the waveform, that's how it is detected. In the revised manuscript, we have tried to explain this 'track-point' terminology is better and we agree that the use of the term "radar height estimate" instead of the "scattering horizon", is a more appropriate description because as you say, it includes the buoyancy as well.

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.

Thanks for pointing out. We have made a conscience effort in include other key papers along these lines of inquiry. See, for e.g., the revised Introduction.

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.

Rephrased as suggested in the revised manuscript.

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.

Understood. the intension was not to present an AWI bias but to mention the same procedures (involving the buoyancy correction) are used in other processing centers as well.

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

Ok, we have shown the distribution geographically in a separate Fig. 3.

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

Ok, we have deleted 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

Ok, the correlation length, and why it is important, is described in connection with equation 3. We don't think that it fits into the data description. A description of the correlation length in each of the five profiles is included.

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

Ok, a description of the correlation length in the specific profile descriptions is included.

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

The sentence has been reformulated.

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

Thanks, we have added "/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 much with radiative transfer modelling.

Each of the variables in eq.2 are frequency dependent. The reflection coefficient in the surface scattering model sigma_surf is a function of the permittivity and therefore frequency, and the interface transmissivity T is a function of permittivity which is a function of frequency. However, the volume scattering sigma_vol is most sensitive to frequency: frequency to the fourth power. The loss L is a function of scattering and absorption, therefore frequency. This has been clarified in the revised version.

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

Yes, the facets are smooth at the given wavelength and large enough to return coherent backscatter. The model is described in detail in the references: Fetterer and in Ulander and Carlström.

P10 L240: How do they calculate the noise floor?

We have reformulated this sentence. There is no noise in our simulations, so the noise-floor is defined as zero backscatter.

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

Here surface scattering is interface scattering from any interface air/snow, icy layers in the snow, snow/ice interface. This has been clarified in the revised version.

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.

We did conduct a simulation study (described in Tonboe, 2017 in the reference list) where different thresholds were tested and the choice of threshold did not change the sensitivity to snow depth. The text describing the choice of re-tracker has been elaborated.
All the radar altimeter processing centers are using a correction for radar penetration in the snow (eq. 4). That means that all the re-trackers are sensitive to snow and our simulations with the 1/2 power re-tracker are simply confirming that.

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.
I sort of agreed with this reviewer here when I first reviewed the manuscript back in the summer, but I think the revised sentence reads more clearly and accurately now.

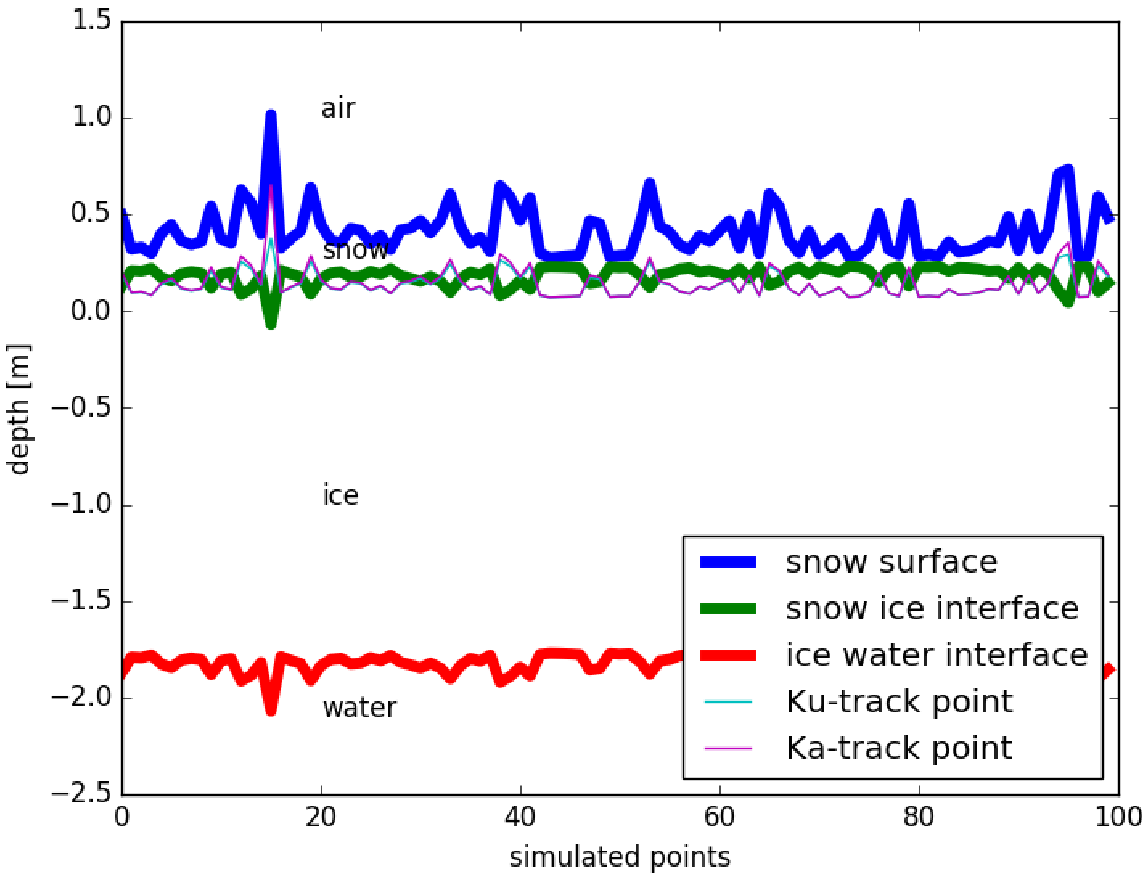
The simulations show that it is not the snow depth itself but rather the snow grain size/ scattering magnitude which is creating the Ka- and Ku-band track-point difference. Of course the scattering magnitude could be a function of snow depth, but also a lot of other things.
We have reformulated this sentence so that this is clearer.

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.

Ok, thanks.

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.

We tried the reviewer's suggestion; however we were not happy with the outcome. We have included a figure showing the air/snow, snow/ice and ice/water interfaces together with the Ka- and Ku-band track points. The track points are close to the snow ice interface as expected but still sensitive to snow. This looks like a transect but it is only the first 100 points in the RRDP. This is a bit confusing, and therefore not included it in the revised MS.



The figure is showing the air/snow, snow/ice and ice/water interfaces together with the simulated Ka- and Ku-band track points for the first 100 RRDP points.

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.

Fig. 6 has been revised and described better in the text and caption. The black crosses in Figure 6 still appear quite faint to see … can they be made bold and thick like they are in Figure 5?

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

Good point, we have formulated this statement differently in the revised manuscript.

— Technical comments —
P2 L36: "UCL" -> "CPOM"

Done

P10 L255: ": : :**Ka and Ku** waveform: : :"

Done

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

Done

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

Done

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

Done