Review of „A Novel Synergistic Approach Using Altimeter Backscatter for an Improved Radiometer Thin Sea Ice Thickness Retrieval" by Stefan Hendricks

In their paper the investigate way to improve the synergy between SMOS L-Band radiometer and CryoSat-2 Ku-Band altimeter data to estimate sea ice thickness in the Arctic. The current state of the art is to merge individually derived sea ice thickness estimates from radiometry and altimetry based on their uncertainties and spatial correlation using an optimal interpolation scheme (ESA CS2SMOS).

The authors develop and test an alternative approach, where sea ice emissivity relevant for the L-Band radiometer retrieval are derived from CryoSat-2 observations. The authors then Compare with their own SMOS-only and official ESA SMOS sea ice thickness and find improvement compared to a set of reference data sets.

My Background

I am the current maintainer of the ESA CryoSat-2/SMOS dataset. My background is mostly on radar altimetry and other methods to determine sea ice thickness with geophysical sensors. My review will therefore mostly focus on the backscatter/emissivity relationship and the reference data and less on the emission modeling.

General Comments

The authors have developed a complex retrieval chain that includes many parametrizations and algorithm choices. I find that the derivation of L-Band emissivity from Ku-Band backscatter coefficient requires a set of simplifications and parametrizations that may not be justified for real-life sea ice conditions.

My main concern comes from the fact that the RF-synergy retrieval algorithms results in thin ice (< 1m) data in multi-year sea ice areas where there is certainly no thin ice (upper right panel in Figure 7). The authors correctly state that their algorithm should only to be considered valid for first year sea ice, but first it does not generate confidence in the RF-synergy algorithms maturity if it results in thin ice thicknesses where neither of the two source datasets would indicate any and second, some artefacts seem to be located in areas first-year seas. As far as I can see the thin ice is introduced mainly by the backscatter derived emissivity and I have detailed this concern in the specific comments section.

But I also had problems following some of the authors logic, because I found the manuscript lacking clarity at least in some sections. I have marked relevant passages in the attached document.

And as my last general comment, I would strongly recommend to compare the RF-synergy data not against the ESA SMOS, but against the ESA CS2SMOS, as this would be a fairer comparison.

Specific Comment: Backscatter – Emissivity Relationship

The authors make the main argument that the Ku-Band backscatter is mainly a function of the dielectic contrast of the snow/ice interface as well as surface roughness. The authors than continue to use the term surface roughness, however there are two different roughness parameters to consider. There is the interface roughness and the large scale sea ice surface roughness, e.g. the surface height standard deviation. Both are relevant parameters CryoSat-2 waveform shape and most importantly, peak power.

The authors are not clear of their definition to as “surface roughness” (I assume it is interface roughness).

The authors also need to make the assumption that that Ku-Band backscatter is solely from snow/ice interface and there is plenty of evidence from field data that this is not the case. It is therefore questionable if dielectric properties of the sea ice surface have a reliable relationship to radar backscatter. Also one issue I did not see discussed, is whether the dielectic properties derived from surface backscatter are applicable for the whole sea ice column.

Specific Comments: Matching SMRT and CryoSat-2 sigma0

The authors derive the radar backscatter coefficient from the CryoSat-2 waveforms peak power, which is standard practice. But they then face the challenge that backscatter coefficient from SMRT and CryoSat-2 data have a different value range. The authors have developed a scheme to bridge SMRT and CryoSat-2 data and in this part of the manuscript I have the most comments:

Influencing factors on waveform peak power

The peak power of the waveform is determined by the type of surface (specular or diffuse) and their properties. For example, even for a simple homogeneous sea surface, the large scale surface roughness (here defined as surface height standard deviation) changes the waveforms peak power. As an example, I have used the SAMOSA+ waveform model to generate a set of waveforms with the same mean square slope value, but different significant wave height values (linear relationship with surface height standard deviation). Changes in surface roughness is not negligible for first year sea ice, especially in sea ice areas that undergo dynamic thickening due to convergent sea ice motion.

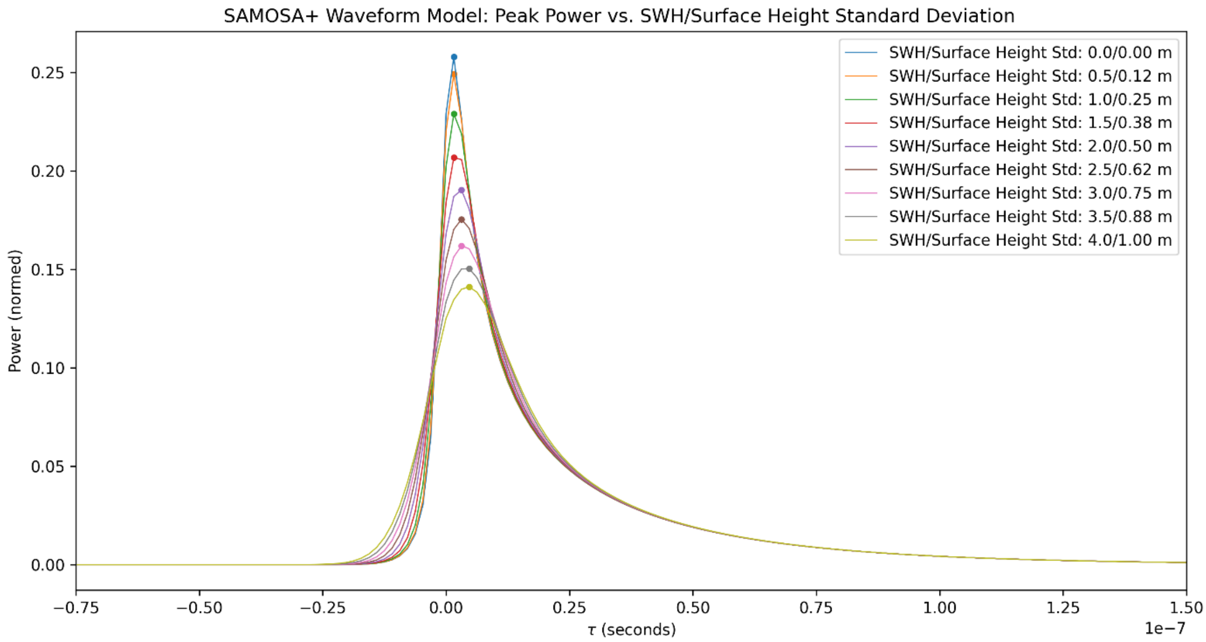

Figure 1:Results from SAMOSA+ waveform model (https://github.com/pysiral/samosa-waveform-model) for fixed interface roughness (nu = 1/mean square slope = 5.0e4) and different surface height standard deviations (implemented in SAMOSA+ via significant wave height). The peak power, and thus the sigma_0 estimate, depends not only on interface roughness but also on the surface height standard deviation.

Also, small patches of young sea ice or leads, can easily dominate a waveforms global maximum, even if located in the off-nadir. This effect may bias the results of the backscatter/emissivity relationship.

Is there any filter for the backscatter values, e.g. only sea ice waveforms? Or all values within a sea ice mask used?

Applicability of SMRT

The authors state that the SMRT altimetry module was "validated in Larue et al., 2021". But this is a paper for land ice, which is a rather different physical environment from sea ice surfaces which are characterized by a strong backscatter heterogeneity. Therefore the term "validated" does is not warranted.

The waveforms simulated are also CryoSat-2 low resolution mode (LRM). For sea ice, CryoSat-2 operates in SAR and partly SARin mode, but there is the pseudo-LRM mode available. What do the authors use here, pseudo-LRM for consistency with SMRT?

Specific Comment: CryoSat-2 backscatter scaling

It is not clear to me how the scaling of CryoSat-2 backscatter works and this need to be better described. Do the authors implement the scaling by mean and standard deviation as in Formular 5?

And if yes, does that not mean that the same CryoSat-2 sigma_0 value can be transformed to a range of scaled values, because the mean and standard deviation of the sample space is different? And what is the sample space, the 15-day window? Sigma_0 should have a mean trend as sea ice ages and get rougher. Scaling by mean also carries the risk of removing this trend?

Also the authors state in the conclusion "*Furthermore, the influence of the roughness in the radar altimeter backscatter has been effectively diminished with the CS2 data treatment, i.e. the scaling and the re-projection into to lower-resolution radiometer grid*". Again it is not clear what the authors mean by "surface roughness", but I cannot follow this logic either way. Both interface roughness and surface roughness are a function of sea ice stage of development and not just a random variations around an arbitrary mean state.

I could see that a seasonal correction factor between SMRT and CryoSat-2 data being a viable option. Also the absolute CryoSat-2 sigma_0 value could be used as filter for the validity of the backscatter/emissivity relationship. But I am extremely sceptical about the scaling approach, assuming that I have understood it correctly.

Specific Comment: Spatial coverage between SMOS and CryoSat-2

How do the authors deal with gap-filling? 15 days of CryoSat-2 at a resolution of 12.5 km does not provide coverage.

Specific Comments: Maturity of validation exercise (especially Ship-EM data)

The authors state in the conclusions that "*Regarding the theoretical framework, the statistical link between Ku-band backscatter and L-band permittivity derived from SMRT simulations is validated by the improved retrieval performance.*" Therefore, the validation exercise is of particular improvement for this paper.

The validation exercise however is based on limited set two parameters (mean absolute error and correlation coefficient), which in my experience do not necessarily tell the full story. For example, one can get rather good correlation factors even if the magnitude of the variation is not the same. And for the mean difference does not tell if thickness are systematically over- or underestimated. I therefore strongly adding at least the bias (mean difference).

The authors also state "*Given its nature, it allows to continuously monitor the thickness while navigating through the ice, delivering highly valuable dataset for validating remote sensing products*" when describing ship-EM data. I recommend caution here. First of all, the SIMS results include snow depth, thus a good match is not necessarily wanted, but the data may also be compromised by representation error, if the ship preferably choses thin ice areas for easier navigation or it speed may dependent on ice thickness. Was this the case? Also, the early cruise report which the authors cite, mentions substantial calibration issues with the SIMS. Was there any quality control and post-processing done on the data?

Conclusions

In summary, I cannot recommend this paper for publication and instead advise major revisions. The author can resolve some of issue by clarifying details, but I also feel that the emissivity needs an uncertainty estimation giving the list of potential issues with backscatter/emissivity relationship and the unclear treatment of the CryoSat-2 backscatter values.

Best Regards,

Stefan Hendricks