

Atmos. Meas. Tech. Discuss., referee comment RC1
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Comment on amt-2021-398

Anonymous Referee #1

Referee comment on "Airborne measurements of directional reflectivity over the Arctic marginal sea ice zone" by Sebastian Becker et al., Atmos. Meas. Tech. Discuss., <https://doi.org/10.5194/amt-2021-398-RC1>, 2022

Review of of "Airborne measurements of directional reflectivity over the marginal sea ice zone" by Becker et al.

The authors present an interesting case study of directional surface reflectance in the marginal sea ice zone, a zone of pronounced inhomogeneity of extreme surface brightness differences in the solar spectral range. Especially the satellite remote sensing community would very much profit from improved surface reflectivity models in this region. To this end the manuscript explores a few approaches how such a parameterization could work, considering factors like the inhomogeneity of potentially unresolved sea ice cover, the gradual impact of the sunglint on the open ocean patches, or the dampening of wind induced waves in the open water between ice floes. The study is well structured and written. It deserves publication after some minor improvements.

In some places the authors seem to discuss findings which are not too surprising and well known. In these places discussion could be slightly shortened and additional literature could be referenced. This is the case in the discussion of the sunglint shape and the Cox and Munk parameterization as well as the impact of "horizontal photon transport".

Minor issues:

Abstract: Please state somewhere in the abstract that this is a case study for one SZA and a 20-minute data set. For some time, I expected more after reading the abstract.

l.45 – Please name the difference between BRDF and HRDF. BRDF – direct illumination only. HRDF – including diffuse light.

L62 and I.63 – “goniometer” should be “spectrogoniometer” or “goniospectrometer”. Otherwise, it is just for measuring angles.

Eq.3 – Why did you omit the "d" in “dFi” e.g.? It is still incremental in i and r, isn't it?

I.151 – “inter-calibrated”. With which instrument? Obviously SMART, but please state it.

I.156 - Why does error propagation of two relative errors around 4% for the quotient HDRF not lead to the sum of relative errors 8% as textbooks teach? Please explain your derivation in the manuscript.

I.159 – Please add the original publication “Mayer and Kylling” to the reference here.

I.180: Please extend $c = I_{red}/I_{blue} = 0.95$ here.

Fig. 3 – There is “sun glint” around all ice edges? And “sea ice” around the sun glint? How relevant is this? How large is the error? Please discuss.

Fig. 5 – It would be interesting to see variations of this distribution for changes in thresholds as discussed in the text. Think about adding them.

I.215 – A standard deviation of 0.6 for a basis value of 0.11 is really large. Please discuss.

Fig.6 – What about white lines in the dark part of the image?

Fig.6 – The figure b is quite confusing at a first glance (while it is understandable reading the related paragraphs in the main text). Please add some more details to the caption. Questions that pop up without it: How can sea ice fraction be a function of reflection angle? How can sun glint fraction and sea ice fraction add up to more than 1?

I.245 and I.249 - "already ... 2002" does sound awkward here, as it is obviously correctly considered in the Cox and Munk publication which was based on observations as well. The impact of "reflected skylight" and the need to remove it for their parameterization is discussed there. Please mention.

L256 – Maybe 0.5 m/s - if you could have simulated it - would have provided an even better match.

I.261 – Do you have "large SZA"? 60 deg doesn't sound too large. Apart from that I have the impression that your results with all their additional uncertainties regarding mix of ice fractions, limited accuracy of glint identification, and true vs effective wind speeds are not suited to analyze the limitations of a theoretical parameterization (Cox and Munk). You could shorten this a bit.

I.285/286 – This seems like a quasi-constant offset because of the nearby dark open ocean influence. Please state this here.

L296 – "However, the optical ..." - Why "however"? I was confused first. I was expecting a snow grain size for Goyens, but got one for Carlsen. Please check wording and improve if possible.

I.311 – "horizontal photon transport mentioned above" – I agree, but why not putting this into clearer words. "the nearby dark ocean surface", "the reduced diffuse light due to the nearby dark ocean" ...

I.355 – I do not understand what you neglect here? The sea ice fraction is central part of Fig.10c and not neglected. Please adjust wording.

L358-369 – I don't understand what you want to tell the reader in this paragraph? The measured sunglint HDRF is affected by inhomogeneity of your sparse measurements (as you stated before) and the limitations of your glint masking (which you hardly mention). The simulated ocean HDRF depend on your arbitrary choice of parameterization of effective wind speed in Eq.5. In addition, a better match might be achievable, if you could include smaller effective wind speeds ... leading to even more frequent specular reflection moments. All these uncertainties lead to the fact that you hardly can compare the two HDRF in any detail. Please clarify what your conclusion is. Maybe shorten this part.

I.382 – "with the irregular distribution of sea ice and open ocean in the MIZ" – and the limitations of your sunglint mask. Please mention.

I.384 – "horizontal photon transport" - Please reference other literature discussing this. It is well known for quite some time that not only local albedo affects the sky brightness. And it is rather not Schaefer et al 2015 who found that first. An older example would be

Richiazzi and Gautier 1998, but I'm sure there are others ...

l.405ff – I'm missing some general outlook what this is good for. What are the limitations of your results? What is needed for it to become a useful method, e.g., for the satellite community (collecting data for different SZA, different wind speeds)? Do you intend to collect such a data set?

Language/ typos:

l.35 – "by about 13 km per decade". Please add "13 km in width".

Fig. 1 caption – "... dots point at ..." -> better "...dots label the ..." . I had to read the sentence several times, because "dots" and "points" seemed the same word and somehow merged in my mind.

L326: "open open"

L389: "variety" -> "variability"?

l.392: "holds" -> "holds true"?

Literature:

Ricchiuzzi, P., and C. Gautier, 1998: Investigation of the effect of surface heterogeneity and topography on the radiation environment of Palmer Station, Antarctica, with a hybrid 3-D radiative transfer model. *J. Geophys. Res.-Atmos.*, 103(D6), 6161-6176.