

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2021-398

Anonymous Referee #2

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-RC2>, 2022

Dear Authors,

I greatly appreciated reading your manuscript and enjoyed learning about your method development and findings. The manuscript demonstrates a fine piece of scientific work and a beneficial contribution to the remote sensing community and to our understanding of the Arctic radiation budget. I would be glad if it gets eventually published. However, in the current state the study includes two major - but easy to solve - issues that need to be fixed before.

In general:

The study applies an existing method to derive optical properties of surfaces from aerial image data and extends it innovatively by taking the composition of two surface type classes into account. This is motivated by the increasing extent of the Arctic MIZ, which is composed of sea ice and open ocean. For both surfaces individually, studies on the HDRF investigated here are available, but the mixture of both has so far remained unexplored. The study is built on remote sensing methods for HDRF, mainly developed by co-authors in previous studies, and is extended by taking a coverage fraction into account and analyzing open ocean HDRF in the MIZ to fill this gap. The conclusion reads frankly that this is only the first step on a way to better understand optical properties of the MIZ, yet it already provides a conclusive picture and is worth a scientific publication as such.

- Used methods are well described and the argumentation line to the results is consistent.
- Language-wise the manuscript is easy to read with only few mistakes.
- Citations are well used to give sufficient background to methods and to compare results.

- Regarding your figures: please avoid the usage of rainbow colormaps, as they are misleading the perception (see e.g. Borland and Taylor 2007: DOI 10.1109/MCG.2007.323435). Apart from that, all graphs are important and helpful to understand your methods and results.
- The study focuses mainly on the ability to mix and unmix the HDRF of two very different surface types. After reading your title I expected a study that particularly concentrates on the MIZ, and thus, on its distinct features and properties and their impact on reflectivity. You discuss only sea ice fraction and note its effect on horizontal photon transport and wave attenuation. Properties of the MIZ like a distinct floe size distribution, the effect of ice edges etc. remain underexplored or are even actively neglected. Improving this could substantially extend the scientific outcome regarding the MIZ.

Major issues of the present manuscript are:

- There is a fundamental mismatch between your definition of the MIZ (ice fraction between 15% and 80%) and the ice fraction in your study area (83%) which actually contradicts your claim to have measured in the MIZ. That needs to be discussed in the manuscript.
- Following your method description, you use the HDRF first as a threshold to separate surface classes, create your sea ice mask and derive a coverage fraction. Subsequently, you use the averaged HDRF of both classes and their fraction to derive a reconstructed MIZ HDRF, similar to a weighted mean. You then compare that to the arithmetic mean.
 - First thought: if the ice fraction would be homogeneous in all directions, the only difference between reconstructed MIZ HDRF and mean MIZ HDRF would be some reclassified pixels in the area of sunglint or ice, slightly changing the weights. Apart from that, the average would be mathematically identical and thus rather insufficient for comparison. So your discussion of errors induced by differences in ice fraction across all directions is correct, but most probably that will remain the only source of error between reconstructed and mean MIZ HDRF even in larger data sets.
 - Second thought: Using the HDRF to classify pixels/directions still seems reasonable, given that you only have this one optical data set. Which, in addition, is also very limited in size. Nevertheless, it would be important for the validity of the method to at least separate training and test data, e.g., by splitting your flight into subsets. You can still give the overall mean from all images as a result, but the evaluation needs separate datasets for training and testing.
 - This needs to be addressed and discussed in the text. Otherwise, this dual use and mathematical dependency makes your results vulnerable, since a comparison to independent data is lacking so far.

Specific comments (and few typos):

Title: Fine, but at the latest in the abstract, the limited data basis of a 20min flight at only one location should be addressed.

Abstract: Please note that your study is based on only one flight, otherwise readers expect a greater data basis (as mentioned). The abstract also lacks concrete results like the deviations in the open ocean that you found. The last sentence should not be stated as such, since you have not accounted for all special conditions of the MIZ in your study (as previously mentioned).

19: sea ice–albedo feedback (long dash “en dash”)

28f: “mixture of sea ice and open ocean, e.g., [...] melt ponds.” Needs reformulation

37: “is dominated by freshly..”

37: Why therefore? Do you refer here to sentence 36f?

40f: That statement needs a reference.

61: the camera does not provide “instantaneous reflectance observations”. Only in combination with a SMART or similar you can derive reflectance.

78ff: High resolution could be added as a motivation for the camera here

81ff: It is a matter of taste, but it has become more common to formulate the scientific questions at the end of your introduction and not to give an extended table of contents, since the overall structure of your manuscript is clear anyways.

101: why do you use different notations HDRF and R_{HDRF} ?

101: was introduced

101f: why however?

111: I would recommend omitting “(Norway)” here, since Svalbard is a known geolocation

and the political affiliation to Norway is regulated in a somewhat more complex manner

111: 19 flights each makes me wonder why you only used one? That needs to be included here.

Figure 1: "dots point" -> "dots indicate/show/..."

116: "cloudless" -> "clear sky"

131: "served as reference in this study."

148: It is great that you give error estimates for each single component of your workflow, but partially, it remains unclear how you retrieve them exactly. Like 4.2% here.

151: What do you mean by "the camera was inter-calibrated"?

153f: Would be good to briefly put the red channel in relation to the spectrum here and discuss what impact you expect on the results. Inaccuracies in other channels are not a good argument to use the red channel, the data set could simply be inappropriate.

168: On which basis did you select your 138 images from 200(?) captured during 20min.? This must be added here, otherwise all distribution functions are difficult to evaluate, since it must be assumed that they originate from a subjective subset.

174f: Why do you exclude data between both modes, most likely from "ice floe edges". Isn't that a major feature of the MIZ? Can you detect any impact of the floe size distribution in your data? 3% doesn't sound much for this dataset, but what about others with a higher fraction?

180: add the ratio here $c = \text{red}/\text{blue}$

183: You state "the mask is capable to separate...". When I look at Fig 3, it doesn't seem so: A significant portion of the sunglint area is masked as ice, all ice edges that you claimed to omit are marked as sunglint. That needs to be better discussed.

187f: "The sea ice fraction refers to the portion of the total horizontal surface that is covered by ice."

Figure 5: What do you mean here by "all images". Sounds like you did not only use your 138 images but more. Why?

200ff: This sentence is not self-explanatory. What do you mean by extreme cases, how do you derive an uncertainty of 4%?

215f: I think there are several physical explanations and not only a statistical reason. What about wave induced heterogeneity in the sunglint area resolved by the high-resolution images or ice edges classified as sunglint in all different directions?

Figure 7: It would be helpful to briefly mention the SZA again in the caption.

256f: Why didn't you include smaller wind speeds to eventually match your observations? Like 0.3, 0.5, ...?

265ff: What about horizontal photon transport from ice edges below the water surface? Or could it be also caused by "blooming" effects on the CMOS sensor?

Figure 8: The wording with "missing here" is misleading, rather use something like "but without the ..." or similar.

310f: If you do a highly simplified budget here, photon loss doesn't seem to have an impact: the open ocean is 0.09 too bright on an area fraction of 17% in your comparison. That equals a darkening of the ice by less than 0.02. Your following sentence referring to the variability is much more important. Given the time of the year I would expect some bare ice surfaces (which I think are also visible in your example images) and thus, an HDRF between snow covered ice and bare ice which you perfectly match.

327: open open

Figure 10: Why not include v_{eff} as legend in (d)? This would make the results clearer.

355: I needed some double reading here until I understood, that 10d shows your main results for v_{eff} . Would be good to mention it already here.

361: in in

361-365: It is difficult to follow you here, please consider rephrasing.

398ff: This statement is difficult to prove, as the impact of the distribution was not evaluated in this study.