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The Cryosphere Discussions

Interactive comment on "A linear model to derive melt pond depth from hyperspectral data" *by* Marcel König and Natascha Oppelt

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Dear Anonymous Referee 1,

thanks a lot for your thorough evaluation and helpful comments on our manuscript. Please find our answers to your questions and our suggestions for integration of your improvements in the following paragraphs. For reasons of clarity referee comments are italic and answers are blue.

Best regards
Marcel König

The authors make the assumption that the reflectance spectra of light and dark bare

ice can serve as proxies for the reflectance spectra of the pond floors of light and dark ponds. I think this likely not true. Melting bare ice typically develops a surface scattering layer, which is likely not present where the ice is ponded. If I follow the arguments made by the authors later in the manuscript, it may turn out that their model is very insensitive to this assumption, which is good. But, I do think the assumption merits some discussion when it is first presented (section 2.1.1).

In fact, the bare ice surfaces did not consist of the typical surface scattering layer (compare Figure 1 A and B). We are going to add this information to Section 2.1.1.

It appears that there are 49 data points used in the validation of this model. That sounds like a large number, but I am concerned that they all come from only 3 distinct ponds. It is not stated whether the site was first-year or multiyear ice. Presumably, unless the pond floors were rafted ice, the optical properties of the pond floors for each pond were likely homogeneous? I suspect there is considerable variability in pond floor properties beyond what was sampled in these three ponds.

Unfortunately, we did not collect ice cores from the test site to measure salinity profiles. Yet, ice thickness under the bright ponds was $\geq$ 0.9 m while it was $\leq$ 0.5 m below the dark ponds. From the patterns of nearby ridges, we presume that the bright ponds were located on old ice – possibly second or multiyear ice – and that the dark ponds were located on a refrozen lead. We can add this information to Section 2.
While the bottom texture was similar in the bright ponds, variability was higher in the dark pond. Line 232 is pointing to the differences in texture. However, you are right that the dataset covers only a portion of the possible variability in bottom texture and optical properties. We are going to add a sentence to Section 4.2.

I wonder if it would be useful to compare the spectra shown in Fig. 5 with the spectra shown in Fig. 4 of Light et al. (2015; https://doi.org/10.1002/2015JC011163)? Eyeball comparison suggests the albedos in that study are spectrally flatter than the reflectance

spectra shown here.

We also thought about using spectra from other studies. Yet, we are afraid that the different measurement techniques and corresponding units (irradiance vs. remote sensing reflectance) hamper a direct comparison.

In the discussion (line 206) the authors declare the "universality" of this approach. I would argue that data from 3 melt ponds (all observed on the same day) likely does not show convincing universality! Also, the fact that the model is only valid for solar angles between 58.9 and 61 degrees makes it not truly universal.

You are right "universality" is probably a little overstated. We are going to change it to "applicability". The model has been tested on solar angles between 58.9 and 61 degrees but was calibrated for zenith angles between 0 and 90 degrees.

Minor points: I think it would be helpful to include "sea ice" in the title. This study is specific to ponds on sea ice, and it may not be applicable to other types of ponds (e.g., on glaciers, ice shelves).

Thank you for this valid comment. We suggest changing the title to "A linear model to derive melt pond depth on Arctic sea ice from hyperspectral data".

Line 7: "vertical melt pond evolution" is not clear. Do the authors mean "melt pond deepening"?

Yes. We are going to use "pond deepening" instead.

Line 10: "slope of the log-scaled remote sensing reflectance..." Isn't it really the "slope of the log-scaled reflectance as a function of depth"?

You are right. We are going to change it to "slope of the log-scaled reflectance at 710 nm as a function of depth".

Line 24: It is not clear what is meant here by "open"? Do the authors mean "ponds with no ice at their surface"?

Yes. You are right that open may also be understood as to melt through. Although "The reflected optical signal from melt ponds with ice-free surface ..." may be the more accurate formulation.

Line 49 (and other places as well, line 55, etc.): sampling rate < 1 nm? The unit "nm" is a length, not a rate?

We are going to replace "rate" by "interval".

Line 54: "...within the scope of a goniometer experiment"– OK, so goniometer is a tool used in that experiment, but it would be helpful to give that experiment a more general name– maybe "...within the scope of an angle-resolving BRDF experiment"?

We will change it as suggested.

Line 69: ". . . negligible standard deviation" of what?

The standard deviation of the 30 spectra of one measurement. We are going to add this information.

Line 85: Please give the time of the measurements in local solar time. I understand UTC is useful for syncing across datasets, but local solar time is essential for interpreting optical measurements.

We will change all time designations to local time.

Line 96: "expert knowledge" would help if this could be more specific.

We suggest ". . . that does not require knowledge about specific ice characteristics".

Line 102: "spectral libraries of melt pond spectra" redundant wording

Right. We are going to change it to "libraries of melt pond spectra".

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line 107: this means there are no constituents contributing to the absorption or scattering, but the absorption of the pure water is still accounted for?

Yes.

Line 131: It should be noted that the Beer-Lamber law applies strictly to media that have no multiple scattering, in one-dimensional domains, in regions not affected by boundary conditions. The ponds in this study should be good candidates for the application of this law, but strictly speaking, it is an approximation to a full radiative transfer treatment.

We suggest adding the following statement to the paragraph: "Here we assume that multiple scattering in melt water and (multiple) reflections at the pond surface, bottom and sidewalls can be neglected to approximate the radiative transfer."

Figure 6: The relationships shown here are just confirmation that the model used (Eqs 4, 5) satisfy Beer's Law, yes?

Yes, but they also visualize the data processing and illustrate the linear correlation (C).

Line 239 (numerous other instances throughout manuscript): data "are" not "is"

Thanks, we are going to change this.

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line 245: "widely independent"? This needs to be clarified. Is "independent" sufficient?

Due to the RMSE of 1.88 cm when fitting the model to the Ocean Optics spectral library, we believe that an influence cannot be entirely avoided (compare lines 245ff).

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