

Replies on the interactive comments on

“Mapping radiation transfer through sea ice using a remotely operated vehicle (ROV)”

by M. Nicolaus and C. Katlein

General comments to all reviewers

First of all, we would like all four anonymous reviewers for their positive and constructive comments on our manuscript. We highly appreciate the work they put into revising our manuscript. Please find our replies to all reviewer comments below. In general, we agree to the main critics that the manuscript reads in parts too much like a field report with too many details and that it does not become clear enough that we present a mostly technical manuscript. The focus of the manuscript is indeed the description of advances in under-ice measurements of solar radiation through the combination of ROV technology and spectral radiometers. Both components have made significant technical progress during the last years that allows this kind of measurements. In a revised version of the manuscript, we will put more focus on these aspects. In order to do so, we will modify the following (major) aspects, as well as those (minor) aspects listed with respect to each reviewer comment below.

- The abstract will more focus on the technical aspects, highlighting the advances through the combination of ROV and spectral radiation measurements. We will highlight the areal coverage as well as the experiences from this study in order to allow collecting efficiently large data sets, how to process and analyze them and give a short outlook on further developments needed.
- The introduction will also cover the aspect of heat fluxes and long-wave radiation, which are missing in the current version. Also advances in ROV technology are not mentioned in the current version of the manuscript.
- The measurement section will be shortened by details about the cruise, which are less important for the methodology. Those details become more important for following analyses, when results need to be put into perspective with ice conditions etc.
- It is obviously right, that the symbols used for different variables are used inconsistently. This will be corrected
- Sections 2.6 (spectral data processing) will be moved before section 2.5 (additional measurements.) The data processing section will be extended by comments on pitch and roll issues and highlight the improvements that are made compared to data processing in Nicolaus et al. (2010, CRST).
- Sections 3.2 (transmission through sea ice) and 3.3 (repeated transects) will be merged into one section in order to distil the findings on data quality and measurement progress.
- The discussion will be shortened by the (incomplete) analyses of the data set with respect to the role of FYI and MYI for light transmission. Now, we are able to include a new reference to more advanced analyses of the presented data set focusing on this aspect (Nicolaus et al., GRL accepted).
- According to the abstract, also the conclusions will be edited to highlight the technical advances.

Anonymous Referee #3

Received and published: 25 October 2012

General Comments:

This manuscript describes measurements of light transmittance through sea ice covers sampled at multiple stations across the Central Arctic. Transects of light transmittance and "transflectance" were recorded over variable paths at each station. This paper describes a new methodology for collecting transmittance data beneath sea ice and presents a data set that was carefully collected and appears to be of very high quality.

I think the methodology, as described, is quite useful. The data set, perhaps less so, as it is of limited use. It is difficult to extract scientific advance from this analysis as there is not a full complement of attending physical property characterization. Indeed the data analysis in this paper is reduced for a better presentation of the method. We see that the dataset has some drawbacks in regards to a full physical characterization of the sea-ice surface. Still we see great potential in the data set as it describes all physical parameters widely used in sea-ice models and can be of high value to improve parameterizations for large scale estimates of solar radiation under sea-ice. These topics will be addressed in future work.

Please find our general comments above. We agree that we need to be more explicit on these aspects. In particular, we see a great potential in the resulting data set, in particular since it is made available online in parallel to this technical and descriptive publication and in parallel to an additional manuscript on differences of FYI and MYI (Nicolais et al, GRL accepted), based on the presented data.

We also added this suggestion to the possible applications of the dataset in section 4.4

Minor comments:

p 3619, line 17: why "(south)"?

The magnetic pole in vicinity to the geographic northpole is physically a magnetic south pole. We removed the bracket to avoid confusion.

p 3622, line 9 -10: this seems like a difficult measurement to make beneath a horizontally inhomogeneous ice cover— what happens when the deeper irradiance measurement includes light propagated through the edge of a neighboring melt pond (or ridge), whereas the shallower measurement does not include that feature in its field of view?

Seems it would be possible to under- (or over-) estimate extinction coefficients for the intervening water depending on the details of the spatial variability of the ice. Ideally, this measurement could be made more reliably with a narrow field of view radiance detector, but then it is difficult to correct irradiances.

Definitely, this method is measuring an apparent extinction coefficient, though we think that it is quite close to a geometry independent measurement. See also comments to Reviewer #4. Radiance data would indeed give better data, if it would be possible to measure in a true vertical profile. Any method without the usage of heavy weights will lead to horizontal displacement due to currents. Under a spatially varying ice cover this will result in bad data quality. Our radiance depth-profiles proved to be unusable for an extraction of the extinction coefficient.

p 3623, line 19 - 20: I think the authors would be wise to take care in using statements like "most comprehensive" and "most unique". These are difficult to substantiate.

We agree and edited some phrases accordingly

p 3624, line 1-2: Sentence beginning "Also, only the direct access..." Please re-write this sentence for clarity; I do not understand what it means as it is presently worded.

We mean the ability to actually work on the ice, which can be limited under some circumstances. We rephrased the sentence for better understanding.

p 3625, line 26 - 27: "it can be shown that light transmittance is generally lower for MYI than for FYI". Is this result corrected for differences in ice thickness? This is either an obvious statement that is tightly correlated to fact that selected MYI was likely thicker than selected FYI, or else it is an interesting result that is not substantiated by the limited data analysis that is presented.

In part it is due to the bigger thickness, the other part is caused by differences in the surface properties (scattering-layer/ melt-ponds). It is indeed an interesting result, which is presented in Nicolaus et al. (GRL accepted), as we wanted to separate this important finding from the extensive description of the method. By this both papers are easier to read as most readers will be either interested in the method or the Arctic wide interpretation of the results.

Are the data presented in Fig. 7 corrected for extinction by the intervening water?

Please state in the discussion of this figure.

Yes they are. We add it into the discussion of the figure