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 #4
Received and published: after deadline

General comments:
This paper demonstrates new measurements of light transmission through sea ice, essentially taking existing methodology for such measurements and mounting the equipment on an ROV to gather measurements over a greater area. Overall the paper is not yet suitable for publication. While hearing about the method is technically interesting, the methodology itself is nothing more than a natural next step combination of existing technologies.
There is novel development in technique here that could merit a "methods" paper and there does not appear to be a scientific question posed or answered by collection and analysis of the data. Therefore, this paper reads more as a cruise report than a scientific paper. The writing needs to be substantially shortened and focused to the topic at hand light transmission through the sea ice.
Unless analysis of large scale spatial variability is to be made, for example, discussions of the cruise track, which get so detailed as to discuss Atlantic and Pacific water masses (wholly irrelevant to transmission through ice) should be removed.
We do indeed see the merit of a "methods" paper for this application. In particular, experiences with respect to operations under sea ice are novel and have not been described yet. We do expect various similar studies in the coming years using this and similar approaches to investigate the under-ice environment, in particular with respect to optical properties. We have considered publishing all the presented aspects as part of a analyses and results manuscript of these data, but found that two manuscripts, one technical and one scientific are more appropriate. See also comments to Reviewer #3.
However, we agree that improvements on the focus of the manuscript are needed. Comments on these aspects are given above. This also includes the suggested reduction in presented details

A data analysis and discussion section must be added between 4.4 and the conclusions. Perhaps transmission could be linked to the observed surface properties? Perhaps if positioning error is too great, statistical analysis could be conducted on the transmission data instead? Basically ask "Why did you go collect this data and what did you hope to do with it?" then answer that question! I'd be very excited to read that paper. The current report should be published as metadata with the online data report and then an analysis paper written.
The main focus of this paper was to communicate the methodology for such observations (see above). Though carrying optical sensors onboard a ROV is "just" the natural next step, we do see enough advances which need to be documented for the future. This has also been acknowledged by reviewer #1 and reviewer #3.
The description of the cruise track is indeed too extensive (see above). Though water-masses obviously don't have a major impact on sea-ice optical-properties, we see a strong influence in light availability under the ice due to different optical properties of the water column. Furthermore ROV operations were strongly influenced by the visibility under the ice which was mostly dependent on the different water mass regimes.

Specific comments:
Statements like "most comprehensive" are not often untrue, but do not add any content to the paper and should be removed. If this is the first, best, biggest, or fastest of anything, your reader can conclude this by comparison to the rest of their knowledge and the literature. A scientific paper is not a sales pitch. This can be found in the introduction, when the author describes his own work in lines 22-25, and throughout the remainder of the paper. I recommend removing virtually every instance of the words "most" (39 of them), "best" (just 1), and "particular(ly)" (12) from the paper.
We agree to the finding that such formulation were used too frequently, however not alike a "sales pitch". We modified various sections of the manuscript accordingly.

I understand the intention, but the second sentence of the introduction is awkward and not quite correct. Shortwave absorption is not the predominant factor controlling snow metamorphism. The melt and formation of sea ice are controlled by many factors aside from shortwave albedo.
This is indeed right. We will rephrase that in a revised version.

Is 100m from the ship really enough to remove the possibility for shadowing? The Polarstern is about 25-30 m high I believe, so in low sun angles near the pole I would expect there to be potential shadowing near the launch site.
This aspect is a good addition and will be included. Indeed the geometry would give possibilities for shadowing. However, we were able to avoid shadowing by careful station planning. In addition, weather conditions during ice stations were mostly overcast with an isotropic field of incoming radiation, not producing detectable shadowing.

In your methodology there is no discussion of how you correct the data, particularly the radiance data, for sensor inclination. In your photograph (figure 2) it is apparent that the Radiance sensor is not even parallel to the irradiance sensor. Where is your inclination data being collected? How are you correcting for errors in inclination, particularly when pointing errors of the radiance sensor may result in the sensor being pointed at ice of a substantially different type. I do not see the inclination data in the Pangaea archive. Is the data already in some way corrected?
Small inaccuracies in mounting angles of the spectral radiometers are irrelevant compared to the 3D motion of the ROV in the water, even during single measurements. To increase data quality dives were anyways conducted in a depth close to the ice underside. Due to this strategy the slight differences in mounting angle are negligible, as the overlap of sensor footprints is good enough comparing to the error in positioning and the movement during the different integration times of each sensor mentioned in Section 2.4. At edges, differences can occur due to the placement distance of the sensors, but uncertainties should be mostly compensated by the large number of measurements. All data were filtered for outliers. We will add these comments also into the revised version of the manuscript.
Issues on pitch and roll errors will be included in the manuscript as well (see also comments to Reviewer #1)

Also, I do not see compass direction on most of your site maps. This, combined with solar angle/time of day will be needed to understand transmittance/shadowing due to adjacent features due to low solar angles of the Arctic.
Compass roses cannot be given due to floe rotation of the free-floating pack ice. The majority of measurements were carried out under overcast conditions, where shadowing and direct illumination are not present. In the few cases of clear-sky conditions, shadowing was taken into account during mission planning.

I cannot support your method for calculating extinction coefficients by measuring a depth profile of radiance or irradiance (3622). As discussed in Frey et al 2011 (one of your citations) the expanding field of view with increasing depth allows a sensor to “see” adjacent ponds or bare ice, greatly altering the apparent extinction coefficient. Your assertion that the observed curves matched an exponential decay with R2 less than 0.9 is not sufficient to overcome this. As Frey et al show, the character of the vertical profile of transmittance may very well match an exponential decay, particularly when starting beneath ponded ice, but this apparent extinction will not be equivalent to true extinction in the water. Considering water in the central arctic is typically quite clear, you will likely have more accurate data if you do not make such “corrections.” They should be removed from the data.
The method of extracting the extinction-coefficients from depth-profiles has certainly some disadvantages, too. But we came to the conclusion that it is the only practicable method during field observations like ours. Even the paper by Frey et al (2011) uses exactly this method to estimate the extinction-coefficient (see page 4 top right of their paper). The only difference is that they use a

water layer further down (15-25m) instead of the topmost 8 meters. Both layers are similarly affected by the geometric effect. Our own calculations (not included here) using a model similar to the one in Frey et al, show that the differences between true and apparent extinction coefficient are rather small. Furthermore this effect was taken into account in mission planning and depth profiles conducted at suitable places with least possible variability. A similar method of depth-correction was applied by Ehn et al. (2011). Instead of measured extinction coefficients they used a simple model to combine different literature values which has its own drawbacks.
We disagree with the suggestion to drop the depth-correction as our data shows that the correction is at least one (if not two) orders of magnitude more important than the suggested inclination-correction. A measurement of a sea ice transmittance of X % frequently had to be corrected to X+2% (absolutely adding 2%) whereas an inclination correction would account to X*(1+-0.03) % (relative change of 3%). Furthermore the water under the ice can be pretty turbid especially in the Atlantic influenced water masses. We added this geometric error to the discussion of depth-correction errors in paragraph 4.3.

3625 This is quite minor, but you list Pi as 3.1415S to four digits, it should be 3.1416 (3.14159265)
Thank you for noticing this typo, which originates from truncating the number instead of correct rounding. We changed it in the text.