Replies to reviewer comments

Please find our answers to the reviewer's overall minor comments in blue font under the respective comments.
We thank you very much for the constructive reviews and the interest in publishing our work in The Cryosphere.

Anonymous Referee #2
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This manuscript presents an interesting and potentially very useful new design for measuring the light field within sea ice. Making use of relatively inexpensive light sensors, the authors sacrifice some degree of accuracy in the measurements for the ability to have many, closely spaced instruments on the light string, which can be left behind after deployment. The low cost and ease of deployment will also allow for installation of the strings at many locations, similarly to the thermistor strings used as inexpensive mass balance buoys.
We thank you very much for the positive evaluation of our novel measurement concept.

The concept is well presented and the first results are analyzed and interpreted in a way that shows the instrument's strengths and weaknesses. I feel it warrants publication in The Cryosphere. The user community would likely benefit from a supplement that gives more technical details of the design.
Most technical details of the design are given in in sections 2.1-2.3. Thus it is unclear to us what you are referring to. This publication does not aim to publish completely documented open source hardware including PCB drawings and parts lists. However we are confident that any interested user with access to the respective facilities can easily built is own version of this chain. Also the manufacturer has a fairly open policy regarding making their designs available.

One of the results the authors present that is important for the ability to use the side-looking irradiance measurements in more traditional applications is that, at least with diffuse incidence, the sideward looking irradiance is proportional to the scalar irradiance, a very useful quantity for biology in the ice and ocean. This result is relied upon as a justification for the unusual orientation of the irradiance sensors on the string. While it may well be an accurate and useful result, I felt the presentation and examination of the modelling this result is based on is the weakest part of the paper and should be improved before final publication. If this tool becomes broadly used, this result will be widely used and cited, so it should be well founded here. Beyond that my comments below are mostly minor.
We included further discussion of the applicability of the main underlying assumption into the manuscript. Of course this equivalency can only be valid for light fields that are close to isotropic. Particularly for more directional light fields above the ice, in thin ice or just underneath the ice surface this equivalency does not hold up.
Nevertheless we are convinced that sideward looking irradiance data can provide crucial insights also in these situations.

Regarding the modelling and interpretation of the model results:

1. It appears there was no attempt to run the model for different wavelengths, given that the absorption coefficient is fixed. I feel it would be worth confirming that the factor-of-four difference to spherical irradiance holds at other wavelengths
Indeed, the light field modelling was only conducted in a monochromatic/broadband fashion, so it is not specific to any wavelength per se. Light transfer in sea ice is mostly governed by scattering, which is nearly wavelength independent, so we do not see a reason for spectrally resolved modelling.

2. How were the absorption and scattering coefficients and asymmetry parameter(a/b/g in §2.5) chosen? No references or reasons are given for the chosen values. Based on the estimates from Light et al (2008, 10.1029/2006JC003977), b=250 m-1seems low for the surface scattering layer (SSL), though b=25 m-1 is in their range for interior ice. Warren et al (2006, 10.1364/AO.45.005320) give much lower values of a for pure ice. Is a=0.15 based on measurements of sea ice? Finally, g=0.9 is very low according to Light et al, who argued that the value they used, 0.94, "is probably too low for the majority of actual g values appropriate for sea ice."
Parameters were chosen to roughly fit the observed albedo and transmittance. As this modelling is aimed at the understanding of the relationship between scalar and sideward planar irradiance and not inverse retrieval of ice IOP, we do not try to exactly match parameters for a direct comparison of the chain data. We upgraded our statement in section and added literature references "These parameters were chosen guided by values previously used in the literature [Ehn et al., 2008b; Light et al., 2008; Petrich et al., 2012] and adjusted so that they resulted in calculated ice albedo and transmittance values very similar to our observations.."
Essential for this calculation is the effective scattering coefficient b' = b*(1-g) [e.g. Petrich et al 2012]. For numerical robustness and speed reasons it is common practice to run simulations at a lower g with appropriately lowered b.

3. The model has isotropic downwelling radiance above the ice, which is a reasonable approximation under thick clouds, which admittedly describes the high Arctic most of the summer. Still, it would be useful to see if and at what depths this relationship holds with a direct beam, something which will be azimuthally dependent and potentially spectrally dependent.
This relationship (as any relation between scalar/planar irradiance and radiance) can of course only hold true for strongly diffuse light fields, such as present in the asymptotic regime or under overcast skies. We added the following statement to the Limitations section making this more clear: "However these equivalencies can of course only be valid for undirected diffuse and azimuthally homogenous light fields. While such diffuse light fields are prevalent in most in and under-ice scenarios, more directional light fields can occur during cloud-free conditions particularly above the ice surface, within the first layers and if a surface scattering layer is absent."

4. Scattering obviously dominates in the SSL, so it might not matter, but was the density used to account for the fact that the air in the SSL reduces the absorption since only40-50% of the path is ice?
As stated in section 2.5 we use a homogeneous plane parallel model and thus did not explicitly include air volume. As you noticed, Scattering is by far the dominant parameter, while the choice of ice absorption coefficient has very minor impact on the results.

5. Showing some comparisons of the modelled normalized irradiance and extinction coefficient with the observed values would give readers more confidence in relying on the model result giving the proportionality between sideward and scalar irradiance.
This comparison can be easily made by the reader when comparing figures 5 and 9. The main idea behind our modelling was not a perfect parameter retrieval and data assimilation for that case, but a general model geometry roughly guided by our scenario. As we did not try to exactly reproduce measured results we do not present measurements and model in the same graph. Making the two fit in this one scenario is purely a matter of tweaking enough parameters and thus -from our viewpoint- does not provide any more scientific insight. To make this clear, we added the following statement in section 2.5 "The goal of this modelling analysis is the general evaluation of the sidewark-looking sensor orientation and not an exact reproduction of the deployment situation, thus we did not further tune the optical parameters to a perfect fit to the observations."

Other comments:

6. Line 77: Was the transparent heat shrink checked to make sure it was spectrallyneutral, or could it have altered the response curves shown in Figure 2?
We thank you very much for pointing out this issue. Following your suggestion, we performed additional experiments towards the characterization of the spectral transmittance of the heat shrink. Unfortunately heat shrink is a cheap industrial product, that is poorly controlled in terms of optical stability. We thus added the following statement to section 4.2: "Part of this uncertainty could also originate from the poorly characterised spectral transmittance of the heat shrink covering the sensors. Manufacturing differences and material aging make it difficult to precisely account for spectral transmittance of the heat shrink. A lab experiment revealed highest heat shrink transmittance for the blue channel, with 3% relative reduction in the red, 6% in the green and 21% in the clear channel."

7. Line 79: 48 sensors spaced 5 cm apart gives a total chain length (from first sensor to the last) of 235 cm.
Corrected accordingly

8. Line 95: Presumably one of the 'eight's should be 'six'.
Thank you for catching this. The sections are six sensors long. We corrected this accordingly.

9. Line 155: I would write 'channel' after 'clear' since 'in the clear' is an expression.
Corrected accordingly

10. While the boundary effects at the bottom of the ice are explained in section 3.1, the increase in irradiance between 0.2 and 0.3 m, just below the SSL, is not mentioned. It should at least be pointed out and possible reasons for it discussed. Is it an effect of the hole, or are the lower sensors seeing more of a nearby bare area or pond? It could also be a real effect of the boundary. The light will refract on entering the ice from the air in the SSL, meaning it will wind up being more downward directed just below the surface, with very little light travelling nearly horizontally (since that would be reflected at the ice surface), so perhaps a side view a few cm below the air(SSL)-ice boundary is actually darker than after some scattering in the ice. I'm not

sure you can come with a definitive answer for why the measured radiance increases in that layer, but it shouldn't be ignored.
We thank you for pointing out this gap in our explanations. We are uncertain what exactly causes this effect, so we added the following statement including your suggestions: "Another notable feature of the profile is an increase between 0.2 and 0.3m. It could result from locally enhanced scattering, an effect of the sampling hole, an effect of the adjacent pond or refraction at the lower boundary of the scattering layer or the water-line."

11. Line 195: Figure 5 doesn't show any data after the 14 September snowfall, so it is illustrating exponential decay within the SSL.
We rephrased this paragraph to be more accurate.

12. The paragraph starting at line 215 should address why the clear channel has lower bulk extinction than any of the others, rather than being between the R and the G/B channels.
We thank you for pointing out this mystery! This effect does not appear during our recent spring 2020 deployments and is much milder during the fall 2020 deployment. It could thus be related either to melt pond vicinity or instrumental issues. To highlight that, we added the following statement: "It remains unclear to us why this prototype shows lower attenuation in the clear channel than the green and blue channels, instead of the expected values between the red and the blue and green channels. This effect did not occur in the deployments during spring 2020 (not described here), and thus seems to either be related to instrument uncertainties or the influence of close by melt ponds."

13. I would re-write line 225-226 to read 'with attenuation coefficients for the clear channel rising from 0.8 m-1 to 1.0 m-1 and for the red from 1.1 m-1 to 1.3 m-1.'
Corrected accordingly

14. Figure 3 is not referenced in the text.
We added the missing reference to the description of the deployment.

15. Figure 4: I would specify in the caption that it is the natural logarithm of sensor count that is plotted.
We added the word 'natural'.

16. I think Figure 6 would be more readable with a logarithmic scale on the color axis,or separate color bars put together to allow seeing the variation within the ice. The inclusion of Figure 7 allows for seeing details at the surface.
We respectfully disagree. Purpose of figure 6 is an overview about the values encountered throughout the ice column. Artificially blowing up small differences within the ice would distort the perception of relative importance of different layers. While differences inside the ice are interesting, they are also far less significant.

17. Since the focus of Figure 11 is the colour of the light, it might work better to use a constant brightness. When I look at it, on screen or paper, the colour information is largely drowned out by the brightness variations.
As both reviewers did not seem to understand this figure well and we did not manage to generate a clearer representation, we deleted this figure and rephrase the respective section accordingly: "The four spectral bands of the light sensor chain also

allow a simple assessment of light color and spectral changes over time. Our first results suggest that there is potential to detect at least transient high concentrations of in-ice algae by this light sensor chain, either in RGB plots or simple band ratios similar to remote sensing algorithms.”