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Interactive comment on "First spectral measurements of light attenuation in Greenland Ice Sheet bare ice suggest shallower subsurface radiative heating and ICESat-2 penetration depth in the ablation zone" *by* Matthew G. Cooper et al.

Anonymous Referee #1

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General comments

Within the manuscript, spectral measurements of light attenuation in Greenland Ice Sheet bare ice are presented. For this purpose, the authors employed spectral irradiance measurements between 350-900 nm wavelength at the surface and at four different depths below the ice surface and calculated the spectral transmittance within the ice. From this, spectral flux attenuation and absorption coefficients are derived and compared to previous studies.

The manuscript is clearly structured and the figures are of good quality, which helps to convey the arguments of the authors. The measurements of in-ice spectral transmittance are very valuable, and the author's efforts to put them into context and to identify possible future applications should be acknowledged. However, there are some aspects that need further focus in my opinion. After some general comments, the more specific comments and suggestions for technical corrections follow below.

In my point of view, the most pressing aspect is the lack of accounting for measurement uncertainties within the manuscript. So far, only statistical variations along the 30 measured irradiance spectra are considered, which need to be clearly separated from instrument uncertainties. However, the latter are not mentioned at all within the manuscript.

I suggest to include a new subsection within the 'Methods'-section that is devoted to instrument and measurement uncertainties. I understand the instrument is calibrated for irradiance measurements, but instrument errors such as the influence of dark and stray light, the wavelength calibration of the spectrometer, the non-ideal cosine response of the RCR diffuser, and the uncertainty of the absolute calibration (to get calibrated irradiance measurements) need to be quantified. Furthermore, as the transmittance is measured calculating the ratio of measured irradiance from two different instruments (the one in the ice, and the permanent one at the surface), differences between the two instruments need to be quantified (e.g., by means of a cross-calibration with the same light source). These instrumental errors need to be put in relation to uncertainties regarding the measurement setup (e.g., is the cosine receptor really in close contact with the ice during the in-ice irradiance measurements?) and the statistical variations as deduced from the 30 subsequently measured spectra.

This uncertainty analysis should eventually lead to vertical uncertainty bars in Figure 3, together with the horizontal uncertainty bars stemming from the depth measurements with the ruler (this second paragraph of Section 3.5 should also be moved to the new uncertainty subsection in Section 'Methods'). The errors in the linear regression to derive the flux attenuation coefficient should consider both depth and transmittance uncertainty estimates and, eventually, should lead to an uncertainty range attributed to each k_{att} value. A thorough treatment of the measurement uncertainties will definitely increase the value of the measurements for further applications.

Another remark is more structural and concerns the introduction of figures in the text: Some figures need to be described and explained in more detail within the text already. So far, some figures are mentioned for the first time in the text in brackets after an interpretation of the figure is done already. In contrast, the figure captions explain the figures in a lot of detail. I suggest providing the information of the figure captions already within the text of the manuscript.

It is definitely fair to point out that the presented measurements are a valuable contribution to the field as (to my knowledge) such experimental values do not exist for glacier ice. However, the authors should consider to not oversell their work in mentioning it many times throughout the manuscript (e.g, the title, and on Page 2 Line 41, P3 L72, P3 L89,...). In a somewhat related issue, the title reads a bit 'bulky' and would benefit from being shortened in my opinion. However, this is of course only something to consider for the authors. In addition, some shortcomings in explaining the applied methodology should be fixed in order to increase reading comprehension and reproducibility of the measurements (see specific comments below).

Specific comments

Introduction

- Page 2 Line 46: at this point of the introduction, it is helpful to give some values for typical ranges of air bubble and ice grain sizes.

- P3 L66: While it is true that analytical models typically assume spherical scatterers to calculate the inherent optical properties, the introduction should also point at different approaches and mention efforts by Kokhanovsky and Zege (2004) as well as Malinka (2014) and Malinka et al. (2016) which provide analytical solutions for single-scattering properties but model snow and ice grains as non-spherical.
  Kokhanovsky, A. A. and Zege, E. P.: Scattering optics of snow, Appl. Opt., 43, 1589–1602,
  https://doi.org/10.1364/AO.43.001589, 2004.
  Malinka, A.: Light scattering in porous materials: Geometrical optics and stereological approach, J. Quant. Spectrosc. Radiat. Transf., 141,3514–23, https://doi.org/10.1016/j.jqsrt.2014.02.022„ 2014.
  Malinka, A., Zege, E., Heygster, G., and Istomina, L.: Reflective properties of white sea ice and snow, Cryosphere, 10, 2541–2557,https://doi.org/10.5194/tc-10-2541-2016, 2016.

Methods

- How did you make sure the cosine receptor is in direct contact with the ice to avoid another ice-air interface? Shimming the ruler underneath the PVC tube seems to help, but have you done any testing in that regard?

- P4 L123: This sentence reads a bit confusing to me and needs to be reformulated. Also: I don't understand the integration time given in Hertz - I think a conversion to an actual time period is useful here.

- I have some suggestions for Figure 1 that would help the reader in my opinion:

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- the photograph of the measurement setup within Figure 1 should be enlarged (and maybe put to the right of the schematic) as, right now, it is a bit small.
- a horizontal line should clearly mark the ice surface, as due to the color gradient it is hard to distinguish from the end of the schematic.
- Can you indicate the other vertical positions of the transmittance measurements maybe with some dashed horizontal lines and then also draw an arrow indicating that the measurements were conducted from the bottom to the top?

- P5 L134: The weather situation during the measurements needs to be specified with respect to clouds, temperature, etc.

Results

- Figure 2:
  - It would be interesting to include the surface downwelling irradiance at z_0 into Figure 2a for comparison.
  - The unit for the standard deviation is missing in Figure 2b.
  - Figure caption: instead of naming it relative irradiance for the first time, I would name it Transmittance like in the rest of the manuscript.
- Figure 2b is not discussed in Section 3.1. Instead of stating in the Figure caption of Fig. 2b that the standard deviation is below 1 W m-2 nm-1 at all wavelengths, I would move this statement to the main text.
- P8 L216: Albedo is mentioned for the first time. Please explain at this point how it was calculated from the surface measurements.

- P8 L231-...: Warren et al. (2006) also show the snow transmission measurements before removing the absorption by impurities - wouldn't using these measurements lead to a similar discrepancy between the theory and field estimate for clean snow than for the glacier ice at smaller wavelengths? I suggest to use the uncorrected measurements for snow as well within Figure 4a, so that it is consistent with the glacier ice measurements.

- P9 L252: The derivation of χ as the root-mean-squared difference between measured and predicted transmitted irradiance should already be explained in the respective Section 2.5 in the Methods.

- Again P9 L252: I have a general question to the χ-value you applied. As you state in the caption of Figure 5, 'the spectral dependence [of the relative error] suggests a contribution of absorption to near-surface attenuation enhancement'. This calls for a spectral value of χ, and indeed you mention at P9 L253, 'weighted equally at all depths and all λ' indicating that you derived χ for each individual wavelength separately. Is this the case? If so, please state already in Equation (7) that χ is dependent on λ (which is the main difference to i_0 in my understanding). However, I get confused with the last sentence of the paragraph on P9 L253: it reads as if you apply only one value of $\chi = 15\,\%$ to all wavelengths. Please clarify this in the text. The same applies to P12 L354.

- Figure 5: The second empirical model you use, applying $I(z_0) = I(z_{\mathrm{12cm}})$, seems to perform best as it is only applied in the isotropic region of transmittance. Looking at the formulas in Figs. 5b and 5c for I_{z}, one could think the best possible solution for χ would be such that $(1-\chi) = I_{\mathrm{12cm}}$. Which is not possible in my understanding as the exponential part of the equation (namely the 'z') is different. To avoid this confusion, the equation in Fig. 5b needs to be adapted accordingly: $I_{\mathrm{z}} = I_{\mathrm{12cm}} \exp\left[-k(z - 12\,\mathrm{cm})\right]$

- P9 L256: it is unclear to the reader how the effective k_{att}-values are derived using a finite-difference solution to Eq. (2) (also: contradictory, in the caption of Figure 6 it says Eq. 1)?

- P9 L260: please state how exactly the effective k_{att}-values were combined for calculation of the effective penetration depth, e.g. give an equation for that.
- P10 L280: Please clarify in more detail how the external diffuse specular reflectivity for a flat ice surface was calculated in this case.
- Section 3.5: The title 'Uncertainty analysis' is misleading. It is true that the second paragraph of this section is a valuable uncertainty estimate, that should already be part of an 'Uncertainty analysis' subsection of the Section 'Methods' (compare general comment). The first paragraph of 3.5 is well-placed at this point of the manuscript, but I suggest to rename the subsection to e.g. 'Influence of ice density' after moving the second paragraph to the 'Methods'-section.
- Figure 9: I suggest including an additional subsection that compares the k_{abs}-values of this study with previous estimates. The first time the authors mention Figure 9 is in the 'Suggestions for further work' part, which definitely undersells this comparison. This is also a very specific case for what I was mentioning in the 'General comments' section: the Figure is mostly described and discussed in the Figure caption and not in the text at all. This should be changed.
- P13 L394: The 'Further work ...'-part of the last sentence is not useful in my opinion, as Section 4.4 already gives suggestions for future studies. I would end the 'Conclusions' section with the new values of attenuation and absorption coefficients that are provided in this study.
- Data availability: please provide the doi of the published dataset in Pangaea.

Technical corrections

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- P2 Eq. (1): please indicate the spectral dependence already within the equation.
- P2 L47: please make sure the exponents are not split up at a linebreak.
- P2 L61: as you specify the spectral dependence of m, you should include it also for m_{re} and m_{im}. In addition, naming them the real part and imaginary part of the complex index of refraction seems more appropriate than denoting them 'real and imaginary index'.
- P2 L62: The authors should consider to give $k_{\text{abs,ice}}$ a separate, numbered equation.
- P5 L139: The equation for the spectral transmittance should become a separate equation instead of an in-text equation.
- P6 L179: do you mean Warren and Brandt (2008)?
- P7 L183: Equation (2) does not give a direct relation to calculate $k_{\text{att}}(\lambda)$. The authors should consider providing a separate equation for this purpose.
- Figure 3:
  - The y-axes of Figs. 3a and 3b don't show the Transmittance T but its logarithm ln T - please adjust the axes titles accordingly.
  - Legend for black line: 350 nm are stated in the text - is '351 nm' a typing error?
- P8 L220: the superscript 1 belongs to the unit of k_{att}, please keep it in one line.
- P8 L234: reference should be to Figure 4a not Figure 5.
- P8 L236: 4 µm instead of 4 um.

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- P9 L252: missing closing bracket after Eq. (7)
- Figure 6: add y-axis title to Fig. 6b.
- P9 L273-274: don’t split the unit on different lines.
- P10 L281: I think this should be a reference to Fig. 3b.
- P10 L287: I guess you don’t mean ω?
- P11 L310: to six previously published [...], not seven
- Bibliography: please add how to access the Mätzler (2002) and Perovich (1996) references.
- Appendix 1, P29 L693: the minus ’-’ in the unit is missing

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Discussion paper