1 Response to Anonymous Referee #1

Received and published: 30 May 2020

Author reply: Thank you for your detailed comments on our manuscript. Below, please find the original reviewer concern/question in italic typeface followed by our response in regular typeface.

Please note: Response to Anonymous Referee #2 is included within this document, after this reponse.

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.

Author reply: We added a new subsection within the 'Methods' devoted to instrument and measurement uncertainties. Specifically, we address:

- Dark noise sensitivity
- Cross-calibration of the two spectrometers
- Non-ideal cosine response of the RCR diffuser
- Absorption interference by the PVC detector rod

To address the last two points, we developed a 3-dimensional Monte Carlo radiative transfer model described in the attached Supplementary Material.

Based on analysis of field datasets and Monte Carlo simulation, we estimate a combined statistical and systematic uncertainty on our k_{att} values that is <20% for wavelengths between 350–450 nm and as low as ~5% for wavelengths >450 nm.

Reviewer comment: *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.*

Author reply: As requested, the second paragraph of Section 3.5 was moved to the 'Methods' (in this case, we added a new subheading 'Ice thickness and density').

As requested, Figure 3 (revised Figure 5) now contains both vertical and horizontal error bars representing uncertainty in depth and transmittance. These new estimates of uncertainty in both depth and transmittance are used in the revised analysis to obtain an unbiased estimate of the slope (k_{att}) and associated standard errors of the linear relationship between depth and transmittance using Maximum Likelihood Estimation, rather than Ordinary Least Squares (York et al., 2004).

Reviewer comment: *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.*

Author reply: We moved the detailed descriptions from the figure captions to the main text, as requested.

Reviewer comment: *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).*

Author reply: We removed the claim "First" from the title and throughout the text, to reduce perceptions of overselling the work. We also removed the reference to ICESat-2. The new title reads: "Spectral attenuation coefficients from measurements of light transmission in bare ice on the Greenland Ice Sheet".

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.*

Author reply: As requested, we added values for near-surface glacier ice air bubble and grain-size radii, which are of the order 10^{-2}–10^{-3} m, or 10^{-1}–10^{-3} m in terms of optically-equivalent grain size (Dadic et al., 2013).

• *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 nonspherical.*

Author reply: Thank for these suggestions. We added a discussion of these important studies to the Introduction, as requested:

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/tc10-2541-2016, 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?*

Author reply: We did not perform optical measurements to test this. Our Monte Carlo simulations quantify the impact of scattering and absorption by the rod, which include the RCR as a scattering surface with the same optical properties as the rod (Sect. S1).

• *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.*

Author reply: The 44 Hz corresponds to 0.0228 s integration time per scan. We agree this is confusing. As requested, the units for integration time are now reported in seconds:

"Spectral irradiance was recorded at 1 Hz frequency. Each recorded measurement is a 20-scan average with 0.0228 s integration time per scan, yielding 0.4 s total integration time per irradiance measurement."

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

- *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?*

Author reply: We enlarged the figure and added a horizontal line at the ice surface, which is also labeled on the figure in text as "ice surface". We did not add the dashed lines, they detract from the clarity of the figure.

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

Author reply: As requested, we expanded our description of the weather situation in the revised manuscript in the new 'Methods' subsection 2.2 'Weather Conditions'. The conditions were overcast with light rain on 20 July and overcast with partial cloud cover during some periods of the experiment on 21 July.

Results

• *Figure 2*

- *It would be interesting to include the surface downwelling irradiance at z_0 into Figure 2a for comparison.*

  Author reply: As requested, we added the surface downwelling irradiance to Figure 2a (revised Fig. 4a).

- *The unit for the standard deviation is missing in Figure 2b.*

  Author reply: The standard deviation is now shown as shaded uncertainty bounds on the plotted values.

- *Figure caption: instead of naming it relative irradiance for the first time, I would name it Transmittance like in the rest of the manuscript.*

  Author reply: As requested, we changed relative irradiance to transmittance.

• *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.*

Author reply: As requested, we moved this statement to the main text in Section 3.1.

• *P8 L216: Albedo is mentioned for the first time. Please explain at this point how it was calculated from the surface measurements.*

Author reply: As requested, we added the definition of albedo and how we calculated it to the Methods Sect. 2.4.

• *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.*

Author reply: As requested, we added the uncorrected (contaminated) snow values to Fig. 9 (comparison of k_{att} spectra from literature review). We removed the figure that this comment references (Fig. 4b) because it is redundant with Fig. 8 (comparison of k_{abs} spectra, revised Fig. 12), as requested by reviewer 2.

• *P9 L252: The derivation of 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.*

Author reply: We removed this method altogether. Rather than compute a best-fit spectral value, we use the 0-12 cm effective attenuation coefficient in a piece-wise Bouguer model to demonstrate the impact of the near-surface attenuation, see below for further detail.

• *Again P9 L252: I have a general question to the -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 , and indeed you mention at P9 L253, 'weighted equally at all depths and all ' indicating that you derived for each individual wavelength separately. Is this the case? If so, please state already in Equation (7) that is dependent on (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 = 15 % to all wavelengths. Please clarify this in the text. The same applies to P12 L354.*

Author reply: As requested, we clarified these issues in the revised manuscript. Specifically, we use the effective attenuation coefficient for the 0-12 cm layer to demonstrate the spectral variation in near-surface attenuation. In addition, we report values for the i_0 parameter using the formal definition given by Grenfell and Maykut, (1977). This parameter is a broadband value. It is simply the fraction of irradiance absorbed in the upper 10 cm of ice, weighted by the incoming irradiance (integrated over a given spectral range). This is described clearly in the revised methods. The reason we report this value is because several large-scale models apply simple radiative transfer parameterizations that use i_0 to put the correct amount of absorbed radiation in the upper grid cell of the vertical ice column (Briegleb and Light, 2007; Liston and Winther, 2005). With our reported values, any user will be able to compute their own i_0 for any other ice thickness or spectral region.

• *Figure 5: The second empirical model you use, applying $I(z_0) = I(z_{12cm})$, 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 would be such that (1) = I_{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: $I_z = I_{12cm}$ exp [k(z 12 cm)]*

Author reply: It is correct that the formula in Fig. 5b needs to include the z-12cm offset. However, we rewrote this equation in piecewise form. Note that the new form is functionally identical to the one used previously:

$$I_{\mathrm{z}}(\lambda) = I_0(\lambda)\exp\left[-\int_0^{\mathrm{z}'} k'(\lambda)\,\mathrm{dz} - \int_{\mathrm{z}'}^{\mathrm{z}} k_{\mathrm{att}}(\lambda)\,\mathrm{dz}\right]$$

• *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)?*

Author reply: As requested, we clarified how the effective k_{att} values are computed (see below) and we corrected the caption of Fig. 6 to point to the correct equation, as requested.

• *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.*

Author reply: As requested, we added a new equation that shows exactly how these values are calculated.

• *P10 L280: Please clarify in more detail how the external diffuse specular reflectivity for a flat ice surface was calculated in this case.*

Author reply: The external diffuse specular reflectivity for a flat ice surface was calculated using Eq. 20–24 of Briegleb and Light, (2007). The formulas are described in more detail Section S1.2.2 of the attached supplementary document that describes the Monte Carlo model, where we use the same formulas to calculate the reflectivity of the PVC detector rod.

• *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.*

Author reply: As requested, we moved the second paragraph of Section 3.5 to the revised Methods Section 2.3: "Ice thickness and density".

• *Figure 9: I suggest including an additional subsection that compares the kabs 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.*

Author reply: As requested, we added an in-depth discussion of the k_{abs} values and compare them with the previous estimates shown in Fig. 9 (revised Fig. 12). The revised Discussion section takes on a narrative format without dedicated subsections as each paragraph is logically linked, therefore we did not add a dedicated subsection.

• 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.

Author reply: As requested, we replaced the last sentence with the new values of attenuation and absorption coefficients.

• Data availability: please provide the doi of the published dataset in Pangaea.

Author reply: The URL to the dataset is printed below. Please note that the dataset is considered "in review" as the parent work (this manuscript) remains in review. As per the policies of the data repository the DOI will remain unregistered until the work is published.

https://doi.pangaea.de/10.1594/PANGAEA.913508

Technical corrections

• P2 Eq. (1): please indicate the spectral dependence already within the equation.

Author reply: We added the spectral dependence to the equation.

• P2 L47: please make sure the exponents are not split up at a line break.

Author reply: Thank you for noting this; we replaced all units with non-breaking hyphens.

• 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'.

Author reply: We made the requested corrections.

• P2 L62: The authors should consider to give $k_{abs,ice}$ a separate, numbered equation.

Author reply: As requested, we added a separate numbered equation.

• P5 L139: The equation for the spectral transmittance should become a separate equation instead of an in-text equation.

Author reply: As requested, we made this a separate numbered equation.

• P6 L179: do you mean Warren and Brandt (2008)?

Author reply: Yes, thank you, we corrected this.

• *P7 L183: Equation (2) does not give a direct relation to calculate k_{att}(). The authors should consider providing a separate equation for this purpose.*

Author reply: As requested, we added a dedicated 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.*

  Author reply: The data shown in Fig. 3a and 3b are transmittance plotted on log-scale axes.

- *Legend for black line: 350 nm are stated in the text is '351 nm' a typing error?*

  Author reply: Yes, this typing error has been corrected.

• *P8 L220: the superscript 1 belongs to the unit of k_{att}, please keep it in one line.*

Author reply: We replaced all units with non-breaking hyphens.

• *P8 L234: reference should be to Figure 4a not Figure 5.*

Author reply: As noted above, we removed this figure because it is redundant with Fig. 8 and 9 (revised Fig. 10 and 12) as requested by reviewer 2.

• *P8 L236: 4 μm instead of 4 um.*

Author reply: We corrected the symbol as requested.

• *P9 L252: missing closing bracket after Eq. (7)*

Author reply: We corrected this as requested.

• *Figure 6: add y-axis title to Fig. 6b.*

Author reply: We added the y-axis, as requested.

• *P9 L273-274: don't split the unit on different lines.*

Author reply: We have replaced all units with non-breaking hyphens.

• *P10 L281: I think this should be a reference to Fig. 3b.*

Author reply: We corrected this reference to Fig. 3b (revised Fig. 5b)

• *P10 L287: I guess you don't mean ω?*

Author reply: Thank you, this typo has been corrected. We intended to say "values of ω at wavelengths >800 nm".

• *P11 L310: to six previously published [...], not seven*

Author reply: Thank you. We added an additional published value. Seven is now correct.

• *Bibliography: please add how to access the Mätzler (2002) and Perovich (1996) references.*

Author reply: These URLs have been added to the references. They are:
Mätzler: https://boris.unibe.ch/146550/
Perovich: https://apps.dtic.mil/dtic/tr/fulltext/u2/a310586.pdf

• *Appendix 1, P29 L693: the minus '-' in the unit is missing*

Author reply: Thank you, this has been corrected.

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2 Response to Anonymous Referee #2

Author reply: Thank you for your detailed comments on our manuscript. Please note that Reviewer 1 requested a comprehensive instrumental and measurement uncertainty analysis. Please see the attached supplementary document that describes the Monte Carlo radiative transfer simulations that we performed for this purpose. Please also see our response to Reviewer 1 for more information.

Below, please find the original reviewer concern/question in italic typeface followed by our response in regular typeface.

This study presents attenuation flux coefficients using spectral irradiance measurements of bare glacial ice in western Greenland. These coefficients are compared with theory and other data sets. The authors conclude that attenuation is enhanced due to a semi-granular near-surface ice layer and by light absorbing impurities at their measurement location. As attenuation flux coefficients for glacial ice are scarce, the data set presented in this study is therefore an important addition for the scientific community. The manuscript is generally well written with clear figures. The following issues should be addressed before publication:

General comments:

1) The title and part of the introduction (line 81-91) looked a bit strange to me. I got the impression that a significant part of the study is a about the ICEsat satellite, while it is only briefly discussed in Sect. 4.3. Therefore, I would suggest to shorten the title and leave out the ICEsat part and shorten the part of the introduction about ICEsat to take away the confusion.

Author reply: As requested, we shortened the title and removed the ICESat part. We retained the ICESat paragraph in the Introduction to demonstrate the broader motivation for our study. We report salient results in the Results section but we removed the ICESat paragraph from the revised Discussion and only briefly touch on the relevant results. We hope this satisfies your request while maintaining the broader impact intent of the submitted version.

2) The Kangerlussuaq region is well known to have a high LAP concentration (e.g., Wientjes et al., 2011; Tedstone et al., 2020). Therefore, it is not surprising that impurities impact the results. The authors, however, state on various occasions in the manuscript that there might be LAPs involved, almost like it is a new finding (e.g., line 331-333: "Comparison with the spectral coefficient for pure ice (Figure 4c) suggests the discrepancy we find is likely due to LAPs present in the measured volume, which appear to disproportionately enhance energy absorption near the ice surface" or on line 385-386: "This suggest light absorbing particles enhance visible light absorption and reduce optical penetration depth at our field site"). I think that the manuscript would benefit if more literature is used to determine if the results are in agreement with the observed LAP concentration for this region.

Author reply: As requested, we strengthened the literature review regarding LAP concentrations. We paid special attention to this request and, as a result, the Discussion section was reorganized. Briefly, our investigation revealed that the ice we measured was optically quite pure, with equivalent black carbon concentrations comparable to pre-industrial values. We infer that the ice is most likely of Pleistocene provenance, which (for this sector of the ice sheet) is generally less dusty than Holocene ice associated with the 'dark zone'. This is discussed in detail in the revised Discussion.

3) The authors state that no asymptotic flux attenuation coefficients are available for glacial ice (e.g., line 72-73), but Ackermann et al. (2006) (which is cited in this manuscript) reported absorption coefficients for glacial ice in Antarctica. Although it is true that Ackermann et al. (2006) measured deep glacial ice in Antarctica while the authors measured bare glacial ice in Greenland, for some cases it compares relatively well with the results presented in the manuscript, as you have shown in Fig. 9. Furthermore, Ackermann et al. (2006) show the absorption coefficient for 532 nm (Fig. 16 of that paper), which does not seem to not match the statement on line 89-91. I would like these issues discussed on the relevant places in this study or an explanation why the authors think it is not comparable (For example, on line 72-73, line 89-91, line 226-229, line 305-307, Sect. 4.3, Fig. 4b, Fig. 9).

Author reply: We removed the claim "first" everywhere to avoid confusion. As with the prior comment, we paid special attention to this request and the Discussion section was reorganized to accommodate a detailed new comparison with the results of Ackermann et al. (2006).

4) Figure 4b should be replaced by Fig. 9, as Fig 4b seems redundant. Furthermore, a discussion in more detail about Fig. 9 is desirable. On one hand it shows that the results of this study are in agreement with AMANDA 1755 m, and support the claim that impurities are an important factor (which is mentioned on various places in the manuscript, like on line 226-229 or line 282-283). However, on the other hand it shows that the difference with the pure-ice estimate of Picard et al. 2016 is very small. This is confusing for me. I also think that Fig. 4a and Fig. 8 can be merged.

Author reply: As requested, we removed Fig. 4b and we point to Fig. 9 (revised Fig. 12) where we previously pointed to Fig. 4b. We added values for contaminated snow (Warren's Layer B) to revised Fig. 10, as requested by Reviewer 1. In addition, we added a detailed discussion of Fig. 9 (revised Fig. 12) that compares our findings with those of AMANDA and Picard et al. (2016).

With regard to the stated confusion, the Picard et al. (2016) pure ice estimate is in fact an estimate of ice absorptivity in the presence of trace impurities. They argue that even if conservative assumptions are made about the impurity concentration of the snowpack they measured, they would still find higher absorptivity values in the blue-green than the Warren et al. (2006) values, meaning the Warren values are unreasonably low. They conclude by recommending that their values be used in lieu of Warren's values, a recommendation that has been adopted in the literature (Tuzet et al., 2019). The point we make is that the ice we measured likely had higher LAP concentration than the snowpack they measured, yet our absorptivity values are lower than theirs. This suggests their values are biased high, and cannot be considered a better estimate of pure ice absorptivity than the values reported by Warren et al. (2006). Rather, it seems sensible to consider their values, and our values (and the AMANDA values) as estimates of in-situ ice absorptivity in the presence of realistic LAP concentrations, whereas Warren's values are reasonable estimates of pure-ice absorptivity. We conclude with this recommendation.

5) Most Figures are barely introduced in the manuscript, while a highly detailed description is provided in the caption. I would suggest to move some of the caption to the main text.

Author reply: We addressed this throughout the manuscript, as requested.

6) It would have been better for the χ term that is introduced in Sect. 2.5 to be wavelength dependent, as attenuation in the surface layer strongly depends on wavelength (e.g., Fig. 6 and 7 of (Grenfell and Maykut, 1977)). This maybe could explain the increasing difference with wavelength for the 12 cm depth fit in Fig. 5c. As the differences are still rather small, I do not think that it is necessary to adjust the results to a wavelength dependent χ, but I think that the manuscript would benefit if the authors state the uncertainty that arises because of this choice. Also, I do not understand line 195-197. Isn't χ now practically the same as i0 due to the spectral integration?

Author reply: We removed the best-fit χ method altogether. Rather than compute a best-fit value, we use the 0-12 cm effective attenuation coefficient in a piece-wise Bouguer model to demonstrate the impact of the near-surface attenuation. In addition, we report values for the i_0 parameter using the formal definition given by Grenfell and Maykut, (1977). This parameter is a broadband value. It is simply the fraction of irradiance absorbed in the upper 10 cm of ice, weighted by the incoming irradiance (integrated over a given spectral range). This is described clearly in the revised methods. The reason we report this value is because several large-scale models apply simple radiative transfer parameterizations that use i_0 to put the correct amount of absorbed radiation in the upper grid cell of the vertical ice column (Briegleb and Light, 2007; Liston and Winther, 2005). With our reported values, any user will be able to compute their own i_0 for any other ice thickness or spectral region.

7) Figure 7, lines 268 – 275 and Sect. 3.5, except for line 285-287, should be moved to the methods.

Author reply: As requested, we moved these sections and Fig. 7 (revised Fig. 3) to the methods.

8) Use the abbreviation 'Fig.' when referring to a figure in running text, unless it is at the beginning of the sentence.

Author reply: As requested, this has been corrected throughout the manuscript.

Minor comments:
Line 65: Change "size > wavelength" to "size larger than wavelength"

Author reply: This has been corrected, as requested.

Line 66-69: Add references to this statement.

Author reply: We added the following references to this statement:

Grenfell, T. C. and Warren, S. G.: Representation of a nonspherical ice particle by a collection of independent spheres for scattering and absorption of radiation, J. Geophys. Res., 104(D24), 31697–31709, https://doi.org/10.1029/1999JD900496, 1999.

Brandt, R. E. and Warren, S. G.: Solar-heating rates and temperature profiles in Antarctic snow and ice, Journal of Glaciology, 39(131), 99–110, doi:10.3189/S0022143000015756, 1993.

Wiscombe, W. J. and Warren, S. G.: A Model for the Spectral Albedo of Snow. I: Pure Snow, J. Atmos. Sci., 37(12), 2712–2733, doi:10.1175/1520-0469(1980)037<2712:AMFTSA>2.0.CO;2, 1980.

For all equations: Use punctuation at the end of the equation, as the equation is part of a sentence.

Author reply: This has been corrected, as requested.

Line 148-149: What do you mean with "Solid ice-equivalent values?"

Author reply: "Solid ice-equivalent values" refers to normalization of the values by the ratio of solid ice density to measured (sample) density. A strict application of Bouguer's Law would have the medium be homogeneous. In the original manuscript, we fit our k_{att} values to the measured depths and then converted k_{att} to 'solid ice equivalent'. In the revised manuscript, we first convert the measured ice thicknesses to 'solid ice equivalent' thickness, and then fit the linear models. Note that for our medium, the density is relatively constant, and we show that our results are insensitive to this decision. This is described in the revised manuscript in Sect. 2.3.

Line 154: g is usually assumed to be independent of wavelength. Also call it the asymmetry factor and define the single-scattering albedo.

Author reply: As requested, we now call it the asymmetry factor and we defined the single scattering albedo with a dedicated equation. We retained the wavelength dependence of g.

Eq 5: Are you sure that Schuster, 1905 is the right reference for this equation? Libois et al. (2013) and Tuzet et al. (2019, cited in this manuscript) describe it relatively well. They also use the Delta-Eddington method, which should be mentioned in the manuscript.

Author reply: Schuster, (1905) is usually credited with the asymptotic two-stream solution (Mishchenko, 2013). We cite Tuzet et al. (2019) and Libois et al. (2013) in the manuscript. In the original analysis, we inverted the Eddington approximation to obtain the theoretical attenuation coefficients. In the revised analysis, we invert the delta-Eddington approximation, and we cite Joseph et al. (1976).

Fig. 2.: Please change "Relative irradiance" to "transmittance" and add the units for the standard deviation.

Author reply: As requested, we changed "relative irradiance" to "transmittance" in the figure caption. The standard deviation plot was removed; the standard deviation is now shown as shaded bounds on the plotted transmittance.

Fig. 3: Do the authors have any idea why k_att becomes increasingly smaller and very small around 850-900 nm, which does not seem to be in agreement with Warren et al. 2006? I know that in the manuscript it is stated that beyond 700 nm the flux is small and the results become less reliable, but it seems to be odd.

Author reply: The values beyond ~700 nm are inaccurate. We show them to help the reader understand why we restrict our k_{att} values to the range 350–700 nm, whereas transmittance is reported to 900 nm (and is plotted in this range in revised Fig. 3b).

Line 216: Define the albedo, as e.g. the surface reflectivity of solar radiation .

Author reply: As requested, we added a definition for albedo and explained how we calculate it.

Line 218: Please use more recent references, e.g. Gardner and Sharp (2010), and/or He and Flanner (2020).

Author reply: As requested, we added both of these references. Thank you for alerting us to the review by He and Flanner (2020), it was helpful.

Line 230: Add more references.

Author reply: As requested, we added the following references:

He, C., Takano, Y., Liou, K.-N., Yang, P., Li, Q. and Chen, F.: Impact of Snow Grain Shape and Black Carbon–Snow Internal Mixing on Snow Optical Properties: Parameterizations for Climate Models, J. Climate, 30(24), 10019–10036, doi:10.1175/JCLI-D-17-0300.1, 2017.

Libois, Q., Picard, G., France, J. L., Arnaud, L., Dumont, M., Carmagnola, C. M. and King, M. D.: Influence of grain shape on light penetration in snow, The Cryosphere, 7(6), 1803–1818, doi:10.5194/tc-7-1803-2013, 2013.

Libois, Q., Picard, G., Dumont, M., Arnaud, L., Sergent, C., Pougatch, E., Sudul, M. and Vial, D.: Experimental determination of the absorption enhancement parameter of snow, Journal of Glaciology, 60(222), 714–724, doi:10.3189/2014JoG14J015, 2014.

Line 241. Do the authors mean Eq. 17 instead of Eq. 16 of Warren et al. (2006)?

Author reply: Yes, thank you, we corrected this.

Fig.6b: Please put k_att(0-12 cm) / k_att on the y-axis and remove the legend.

Author reply: Corrected, as requested.

Line 268: I assume the authors mean with "The field measurements" your observations, and not from Grenfell and Maykut (1977)? Please clarify.

Author reply: Yes, we are referring to our measurements. We clarified this in the revised text.

Line 287: "omega > 800 nm". 800 nm does not make sense, as omega is defined in this manuscript as the single-scattering albedo.

Author reply: This typo has been corrected.

Line 323-324: "The comparison demonstrates the tremendous variation in k_att values". The term 'tremendous' is a bit overexaggerated. Besides, the differences are not that large if the absorption coefficient is compared to glacial ice or pure ice (Fig. 9).

Author reply: We revised the text as follows: "The comparison demonstrates that k_{att} values vary by nearly two orders of magnitude at visible wavelengths due to differences in ice structure and composition"

Line 374: Change "to modelling light attenuation in glacier ice" to "to modelling light attenuation in near-surface glacier ice".

Author reply: Changed, as requested.

Line 394-397: This is a bit vague, please reformulate.

Author reply: As requested, this paragraph has been reformulated.

References:

Briegleb, B. P. and Light, B.: A Delta-Eddington Mutiple Scattering Parameterization for Solar Radiation in the Sea Ice Component of the Community Climate System Model, Technical Note, National Center for Atmospheric Research, Boulder, Colorado. http://dx.doi.org/10.5065/D6B27S71, last access: 18 February 2019, 2007.

Ertürk, H. and Howell, J. R.: Monte Carlo Methods for Radiative Transfer, in Handbook of Thermal Science and Engineering, edited by F. A. Kulacki, pp. 1–43, Springer International Publishing, Cham, https://doi.org/10.1007/978-3-319-32003-8_57-1, , 2017.

Gordon, H. R.: Ship perturbation of irradiance measurements at sea 1: Monte Carlo simulations, Appl. Opt., 24(23), 4172, https://doi.org/10.1364/AO.24.004172, 1985.

Grenfell, T. C. and Maykut, G. A.: The Optical Properties of Ice and Snow in the Arctic Basin*, Journal of Glaciology, 18(80), 445–463, https://doi.org/10.3189/S0022143000021122, 1977.

van de Hulst, H. C.: Multiple light scattering: tables, formulas, and applications, Academic Press, New York., 1980.

Joseph, J. H., Wiscombe, W. J. and Weinman, J. A.: The Delta-Eddington Approximation for Radiative Flux Transfer, J. Atmos. Sci., 33(12), 2452–2459, https://doi.org/10.1175/1520-0469(1976)033<2452:TDEAFR>2.0.CO;2, 1976.

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Mishchenko, M. I.: 125 years of radiative transfer: Enduring triumphs and persisting misconceptions, pp. 11–18, Dahlem Cube, Free University, Berlin, https://doi.org/10.1063/1.4804696, , 2013.

Modest, M. F.: Radiative heat transfer, Third Edition., Academic Press, New York., 2013.

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Picard, G., Libois, Q. and Arnaud, L.: Refinement of the ice absorption spectrum in the visible using radiance profile measurements in Antarctic snow, The Cryosphere, 10(6), 2655–2672, https://doi.org/10.5194/tc-10-2655-2016, 2016.

Schuster, A.: Radiation through a foggy atmosphere, The Astrophysical Journal, XX1(1), 1–22, 1905.

Tuzet, F., Dumont, M., Arnaud, L., Voisin, D., Lamare, M., Larue, F., Revuelto, J. and Picard, G.: Influence of light-absorbing particles on snow spectral irradiance profiles, The Cryosphere, 13(8), 2169–2187, https://doi.org/10.5194/tc-13-2169-2019, 2019.

Wang, L., Jacques, S. L. and Zheng, L.: MCML—Monte Carlo modeling of light transport in multi-layered tissues, Computer Methods and Programs in Biomedicine, 47(2), 131–146, https://doi.org/10.1016/0169-2607(95)01640-F, 1995.

Warren, S. G., Brandt, R. E. and Grenfell, T. C.: Visible and near-ultraviolet absorption spectrum of ice from transmission of solar radiation into snow, Appl. Opt., AO, 45(21), 5320–5334, https://doi.org/10.1364/AO.45.005320, 2006.

Zhang, X., Qiu, J., Li, X., Zhao, J. and Liu, L.: Complex refractive indices measurements of polymers in visible and near-infrared bands, Appl. Opt., 59(8), 2337, https://doi.org/10.1364/AO.383831, 2020.