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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.

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Author response: Thank you for your detailed comments on our manuscript. We believe we have addressed each request. Below, we provide a point-by-point reply to each comment. Please note that the other reviewer 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.

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 moved the ICESat paragraph from the introduction to the discussion where it provides an example for how our dataset can be used (c.f. Deems et al., 2013; Smith et al., 2018).

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: We did not measure LAP concentration directly; therefore, we were cautious in noting that we infer LAP influence. Our intention is not to suggest absorption by LAPs is surprising. However, it is also important to acknowledge that prior studies reported on albedo and/or reflectance, whereas the results referenced here relate to transmittance at >12 cm depth below the ice surface and therefore provide additional context regarding light absorption by LAPs within the ice volume, rather than on or very near the ice surface.

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. We acknowledge the need to distinguish our results from those of Ackermann et al. (2006). There are two main distinctions: 1) compressed glacial ice at >800 m depth has no granular structure, and 2) absorptivity of compressed glacial ice at >800 m depth is controlled by factors that are only partly relevant to the ice sheet surface (dust concentrations from past millennia). The Ackermann et al. (2006) results demonstrate that scattering at >800 m depth is mainly controlled by clathrates, indicating that air bubbles are also of little importance. An earlier study, closely related to the Ackerman study, concludes: "Scattering . . . at ice–ice boundaries . . . will be of minor importance" (Price and Bergström, 1997). Consequently, the two main factors that control light scattering near the ice sheet surface (granularity and air bubbles) are of minor importance to the light scattering results presented by Ackermann et al. (2006).

Although we agree that Fig. 16 of Ackermann et al. (2006) compares relatively well with our results, it is important to acknowledge that this is at least in part incidental, and different physical mechanisms are at play in both cases. It is not our intention to suggest that Ackermann's results are irrelevant to our study or that our study is incomparable to that study. Rather, the underlying physical mechanisms that control light attenuation are different in both cases. As such, we have removed claims of "first" throughout the paper and we added additional context for the difference between our study and the Ackermann et al. (2006) study in the relevant discussion.

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: We removed Fig. 4 and we point to Fig. 8 and 9 where we previously pointed to Fig. 4, as requested. We added a new paragraph that concludes the Discussion section focused on Fig. 9.

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 originally included the spectrally averaged value because large-scale models often need a single value for the visible and a single value for the infrared, or one single broadband value (Briegleb and Light, 2007; Liston and Winther, 2005). We now report spectral $\chi(\lambda)$ values in addition to the average value, as requested by another reviewer.

The reason we distinguish χ and i_o is because i_o is defined in such a way that it partitions the absorbed solar flux, or the net solar flux divergence, whereas we use χ to partition the downward flux. Grenfell and Maykut (1977) use their albedo measurements and modeling to extend their albedo and extinction coefficients across the solar spectrum and thereby to calculate the net flux divergence. Overall, our main goal with this part of the paper is to communicate the idea that a surface scattering layer is present on the ice sheet, and that this concept is well-developed in the sea ice literature but is conspicuously absent from the glaciological literature. To that end, we felt that a single χ value was sufficient to communicate that message.

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

Author reply: We moved these sections and Fig. 7 to the methods, as requested.

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

Author reply: This has been corrected throughout the manuscript, as requested.

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:

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.

Liston, G. E., Bruland, O., Elvehøy, H. and Sand, K.: Below-surface ice melt on the coastal Antarctic ice sheet, Journal of Glaciology, 45(150), 273–285, doi:10.3189/002214399793377130, 1999.

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 k_att values by the ratio of solid ice density to measured (sample) density: k_i=_i/ k_att where is measured ice density, _i is solid ice density (917 kg m-3), k_i is k_att in units of (inverse) “solid-ice equivalent thickness” [m-1] and k_att is in units of (inverse) in-situ ice thickness.

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: We now call it the asymmetry factor and we defined the single scattering albedo, as requested. For completeness, 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: The equation we use is derived equivalently from the Eddington approximation or the two-stream derivation given in Bohren, (1987). 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.

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

Author reply: We changed “relative irradiance” to “transmittance” in the figure caption and we added the units for standard deviation, as requested.

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 $\sim$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 was measured to 900 nm (and is plotted in this range in Fig. 2c).

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

Author reply: We added a definition for albedo and explained how we calculate it and how we use it.

The text reads: "The ice surface albedo was estimated as the ratio of the 2 m background upwelling spectral irradiance to the downwelling spectral irradiance. These irradiance data were smoothed with the same 1 nm interpolation filter described [for the in-ice irradiance measurements]. The ice surface albedo is presented in Sect. 4 to qualitatively discuss the in-ice irradiance measurements and the k_att (λ) estimates."

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

Author reply: 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: Line 230 in the discussion paper is a comment that grain size dominates absorption beyond ~530 nm. Line 229 is a comment that LAPs dominate absorption at shorter wavelengths. We are not sure which of these two comments this request is aimed at, but we added the following references that address both comments (He et al., 2017; Libois et al., 2013, 2014):

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: We have made these corrections, 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. The revised text reads: "the maximum difference found was 0.2% for values of ω at wavelengths greater than 800 nm."

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 >1 order 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: We added "near-surface", as requested.

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

Author reply: We removed this sentence at the request of another reviewer and replaced it with a summary of the scattering and absorption coefficient values that we quantify.

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Please also note the supplement to this comment:
https://tc.copernicus.org/preprints/tc-2020-53/tc-2020-53-AC1-supplement.pdf

Interactive comment on The Cryosphere Discuss., https://doi.org/10.5194/tc-2020-53, 2020.

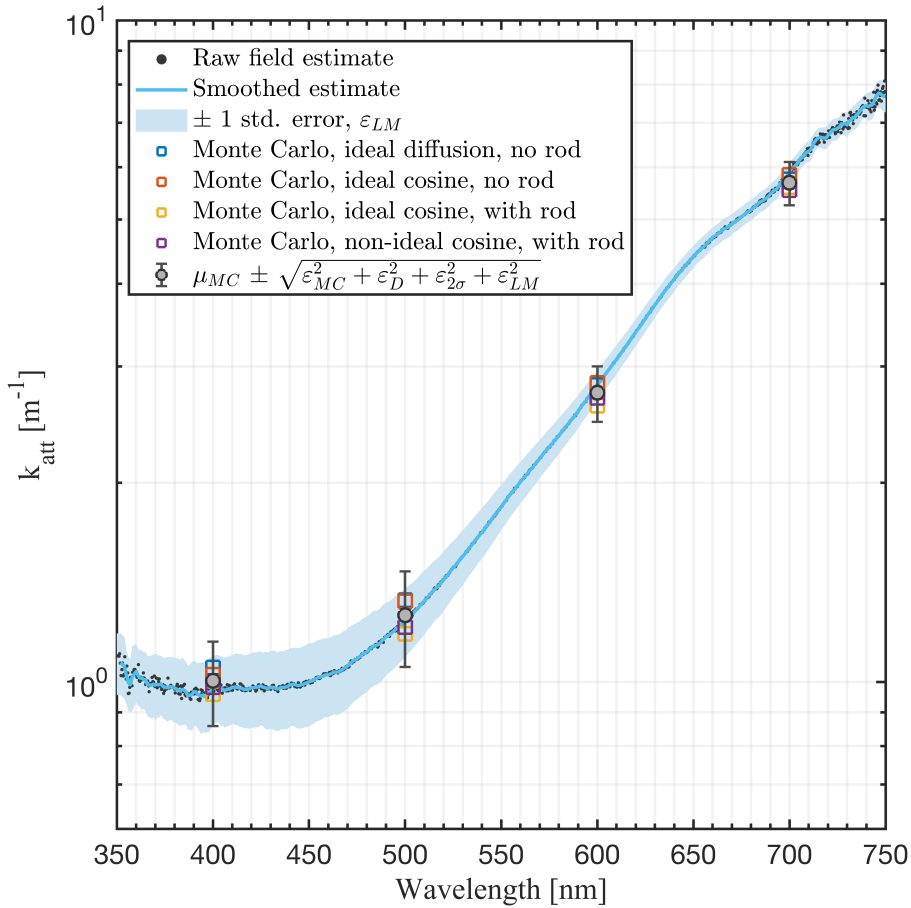

Fig. 1. Attenuation coefficient k_att spectra from measurements of light transmission collected on 20 July, 2018, compared with average k_att values from four simulations with a 3-dimensional Monte Carlo radi

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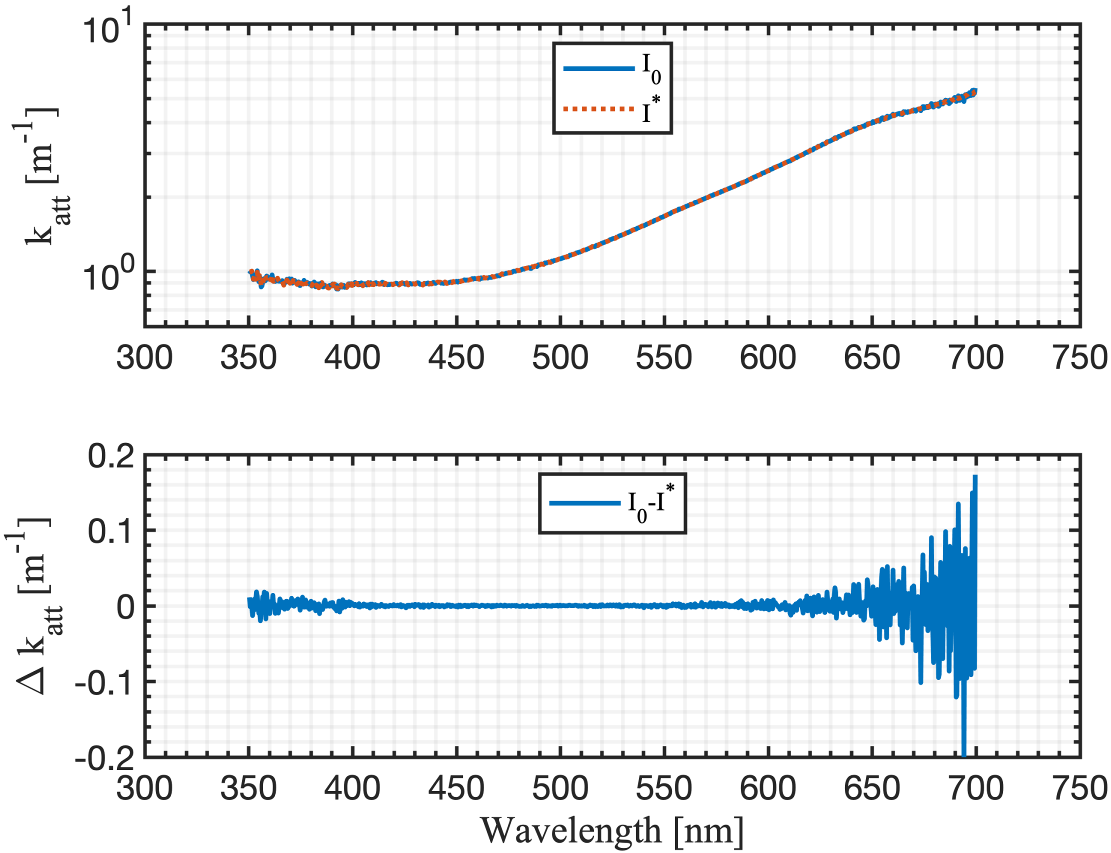

Fig. 2.

Interactive comment

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