The Cryosphere Discuss.,
https://doi.org/10.5194/tc-2020-53-RC2, 2020

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 #2

Received and published: 23 July 2020

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.

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.

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).

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.

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.

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?

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

Minor comments:

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

Line 66-69: Add references to this statement.

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

of a sentence.

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

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

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.

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

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.

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

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

Line 230: Add more references.

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

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

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

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

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).

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

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

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