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Interactive comment on "Influence of light absorbing particles on snow spectral irradiance profiles" *by* François Tuzet et al.

Anonymous Referee #1

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General comments

This paper presents an original technique for estimating the concentration of light absorbing particles (LAP) in snow based on measurements of vertical profiles of spectral irradiance made at wavelengths from 350 to 950 nm.

The approach is based on values of the asymptotic flux extinction coefficient (AFEC) derived for homogeneous layers in snow. An optimization algorithm is developed that estimates the snow specific surface area (SSA) and concentratios of dust (c_{dust}) and black carbon (c_{BC}) based on the AFEC spectra. The inferred parameters are then compared with independent observations of SSA and equivalent-BC. It is shown that the estimated equivalent-BC concentration correlates quite well with that inferred from

chemical measurements, although only for concentrations larger than 5 ng g^{-1}. However, disturbingly, there is a substantial systematic bias of ≈16 ng g^{-1} between the equivalent-BC concentrations inferred from the algorithm and the chemical measurements. Various sources of error are discussed, but they are able to explain the bias only partially.

The proposed method would allow for a relatively fast measurement of the LAP concentrations (compared to chemical analyses of snow samples), but at this stage, its attractiviness is reduced by the presence of a systematic bias that is not properly understood. Also, the applicability of the method is limited to homogeneous layers, and it can only be used at depths where the radiation field in snow is diffuse, and sufficiently far away from the underlying ground. Therefore, rather than providing a method of measuring LAPs that is ready to use at its present state, I view the research reported in this article as a "closure experiment" that probes our understanding of both the radiative transfer in snow with LAPs and of the measurements. Clearly, this understanding is still less than perfect.

Even though the proposed method does not yet work fully satisfactorily, I think this is an innovative and interesting study. I therefore recommend its publication in the Cryosphere, subject to the minor comments listed below.

Specific comments

1. p. 2, line 2: Can you specify what you mean with "near infrared"? Different definitions exist. Wikipedia mentions both 1.4 μm and 2.5 μm, while in atmospheric radiative transfer literature, NIR usually extends to 4 μm. It is well known that snow albedo is generally quite low at wavelengths larger than ≈1.4 μm.

2. p. 4, line 11: do you mean the onset of the snow season, or onset of the snow melt

season?

3. p. 4, lines 17–18: “taking precautions not to enlarge the hole”. Even so, some light could leak deeper into snow through the small space of air between the rod and the snowpack. Did you eliminate this somehow?

4. p. 6, line 11: It would be helpful to show the spectral Mass Absorption Efficiencies (MAE) of BC and dust assumed in the computation of equivalent BC concentration in the conventional units ($m^2\,g^{-1}$). I can see that related information is given in Fig. 2, but it requires effort to do the conversion.

5. p. 6, Eq. (3): please define z and z_0. Presumably z is the depth, increasing downwards (even though in Figs. 3 and 7, the values are negative).

6. p. 6, line 27: Did you apply some quantitative criteria for the minimum distance between your zones-of-interest and ground?

7. p. 8, Eq. (6): please define B (absorption enhancement parameter?).

8. p. 9, Eq. (12): Strictly speaking, g and B also depend on wavelength. An example of this can be seen in Figure 6a,c of Räisänen et al. (2015) for a few assumptions about snow grain shapes. (I think their ξ parameter is equal to B in this study). For the wavelengths of interest for the present study (350-950 nm), $B(1-g)$ might vary up to $\sim \pm 5\%$ as a function of wavelength (with largest values at the short wavelengths). I guess this would be insignificant compared to other uncertainties associated with your approach, though.

REFERENCE:

*Räisänen, P., Kokhanovsky, A., Guyot, G., Jourdan, O., and Nousiainen, T.: Param-

eterization of single-scattering properties of snow, The Cryosphere, 9, 1277–1301, https://doi.org/10.5194/tc-9-1277-2015, 2015.

9. p. 10, lines 20-21: “The wavelength range of the estimation an the η_{mes} do not impact the eqBC retrieval”. Do you mean “...do not impact significantly”?

10. p. 11, line 5: The range of dust MAE (0.071–0.127 $m^2\,g^{-1}$ at 407 nm) seems a bit conservative, considering that you mention an order of magnitude uncertainty on p. 2, line 25, and that the whole range of Table 4 in Caponi et al. (2017) goes from 0.071 to 0.621 $m^2\,g^{-1}$ (even though the maximum represents Sahel only).

11. p. 11, line 28: “The variations of g do not impact LAP retrievals”. Again, “do not impact significantly”? At any rate, this seems surprising to me, especially when you first explain that while the ratio $B/(1-g)$ may be fixed at 10.7, variations of B and g could still have an impact as $B(1-g)$ may vary. What was the actual range of g and B considered when you arrived at this conclusion?

12. p. 11, line 29: Please specify that you mean the imaginary part of the ice refractive index (n_i).

13. p 12, section 4.4: Also mention that according to Fig. 12, the estimated dust fraction to LAP absorption is underestimated in almost all cases (this might tell something about errors in the spectral signature of BC vs. dust absorption, even if pursuing this issue further is not feasible here).

14. p. 15, line 6: I think AART should be mentioned already in the theory section 3.4.

15. p. 15, line 30: “Using Monte Carlo ray tracing on real micro-tomography snow samples.” I think it would be appropriate to mention here explicitly the concept of close-packing. In fact, a recent paper by He et al. (2017) suggests that close-packing of snow

may substantially enhance the albedo reduction caused by BC in snow (and hence the total absorption in snow). However, my intepretation of their paper is that this mainly happens because close-packing makes the effective snow grain size larger, or the SSA smaller, so that radiation penetrates deeper into snow (which is an effect that should be captured even by traditional 1D radiative transfer). What do you think?

REFERENCE:

He, C., Takano, Y., and Liou, K.-N. (2017), Close packing effects on clean and dirty snow albedo and associated climatic implications, Geophys. Res. Lett., 44, 3719–3727, doi:10.1002/2017GL072916.

16. Caption of Fig. 1: “B, g, LAP MAE” is quite cryptic because these parameters appear in the text much later than Fig. 1 is introduced. If you replaced this with “$B = 1.6$, $g = 0.85$, LAP MAEs defined in Sect. 3.4.2” it would already be much more explicit.

17. Fig. 2: I am puzzled about the numerical values here. Ice absorption coefficient reaches down to 10^{-6}m^{-1} at 390 nm. In Picard et al. 2016 (The Cryosphere, 10, p. 2655–2672), the lowest values for the IA2008 curve (which is probably too low) are slightly below 10^{-3} m^{-1}, i.e., three orders of magnitude higher. Also, what is assumed about snow density here?

18. In Figs. 3 and 7, it would be logical to switch the colors for 550 and 700 nm (as the wavelength for red light is ≈700 nm, and green light ≈550 nm).

19. In Fig. 8 and 10, can you include a scale showing how the size is related to the maximum wavelength of the AFEC estimation?

Technical and language corrections

1. p. 1, line 14: replace “dependence” with “sensitivity”.

2. p. 9, line 17: this should be “dust source regions”.

3. p. 9, line 19: replace “inferior to” with “smaller than”.

4. The order of figures differs from the order they are cited in the text. Fig. 5 is cited first time after Figs. 6 and 7, and Fig. 14 is cited first time before Figs. 12 and 13.

5. p. 12, line 13: replace “few number” with “small number”.

6. p. 13, line 27: this should be “abnormally”

7. In Figs. 2, 3b, 4, 6: There is a label missing on the lower left corner, and should be added so that the reader can interpret the scale accurately. (Hint: this is probably a round-off problem with your graphics software. But graphics software can be cheated: e.g., in Fig. 2, try to start the scale from 9.99$\times 10^{-7}$ instead of 10^{-6}!).

8. Fig. 3: Add units of depth (m) on the y-axis.

9. In the caption of Fig. 10, “comporting” sounds like a strange choice of verb.

10. In Fig. 12, x-axis label, “Mesured” should be “Measured”.

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