

First of all, we slightly modified the results in the new version of the manuscript. The retrieval method used in the last version indeed included a small regularization term to minimise the SSA difference between the retrieval and the measured value contrary to what is written from page 10 lines 18 to 21. As this term had a small weight the impacts on our results are small. However to remain consistent with the description of the method, the regularization term has been removed and all figures and paragraphs impacted have been modified.

The impacts on our results are a small improvement of LAP retrieval performances ( $r^2$  0.74→0.80, mainly due to one point shifting under the sensitivity threshold of 5ng/g) and a small reduction of SSA retrieval performances ( $r^2$  0.73→0.71). The conclusion of the paper remains unchanged.

#### Answer to Anonymous Referee #1 (Referee):

We would like to thank Anonymous Referee #1 for his extensive analysis of our manuscript which helps improving our paper. All the comments have been addressed and point by point response is provided below each comment.

The reviewer initial comments are written in black, our answer in blue and the corrections in the paper are highlighted in red. The line numbers which are used in the answers correspond to the new version of the manuscript.

#### 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 concentrations of dust ( $C_{\text{dust}}$ ) and black carbon ( $C_{\text{BC}}$ ) 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<sup>-1</sup>. However, disturbingly, there is a substantial systematic bias of ≈16 ng g<sup>-1</sup> 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 attractiveness 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.

Indeed, the definition of near infrared can be ambiguous. Here by near infrared we mean the wavelength range from the visible to ≈1.4 μm. The first sentence of the introduction has been modified in the revised manuscript:

Snow is a highly reflective medium in the wavelengths of the visible and of the near infrared (up to 1.4 μm, referred to as NIR in the following) where most of the solar energy is available (Warren, 1982).

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

Here it should be understood: “from the onset of the snow season”. It has been explicitly added in the manuscript p4 l.11:

both in winter and spring conditions from the onset of the snow season to the total melt-out of the snowpack.

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?

Following the protocol described in Picard et al. 2016, we systematically added a few millimeters of snow on the surface around the rod to cover the void space from direct sun beam. This avoid the risk of direct light penetration in the small space of air between the rod and the snowpack though we cannot fully exclude that some additional radiation is scattered in the hole. We modified the manuscript accordingly :

page 4 line 18 :‘taking precautions not to enlarge the hole. A few millimeters of snow was systematically added on the surface around the rod to shield the void space from direct sun beam in order to minimise the leak of additional solar radiation into the hole.’

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 ( $\text{m}^2 \text{g}^{-1}$ ). I can see that related information is given in Fig. 2, but it requires effort to do the conversion.

Indeed, this information is of importance and is not illustrated in the manuscript. The Figure 1 and his caption has been modified as follow to account for this remark:

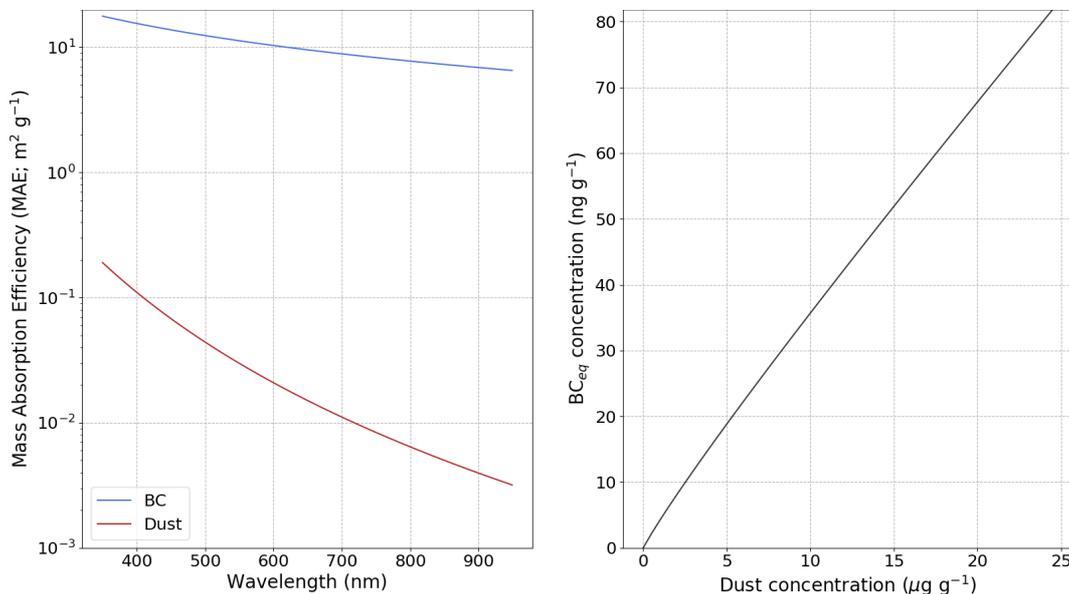


Figure 1. a) Mass Absorption Efficiency (MAE) values of BC and dust used in the present study as a function of wavelength. b) EqBC concentration corresponding to a given dust concentration using these MAE values and the methods described in section 3.1.

Consequently, all references to Figure 1 have been replaced by Figure 1 b) in the manuscript and the text p.6 l.13 has also been modified as follows:

It is noteworthy that the function  $\Psi$  has a strong dependence to the spectral distribution of the incident solar radiation and on the radiative transfer model parameters, mainly on the selected values of BC and dust Mass Absorption Efficiency (MAE). These MAE values are represented in Figure 1 a) and detailed in section 3.4.2.

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).

Indeed  $z$  is the depth increasing downwards and  $z_0$  is a reference depth.

The text p.6 l.28 has been modified as follows:

Following the radiative transfer theory in a homogeneous layer far from any interface, the intensity at a given wavelength  $\lambda$ ,  $I(z, \lambda)$ , decreases exponentially with depth. This writes:

$$I(z, \lambda) = I(z_0, \lambda) e^{-ke(\lambda)(z-z_0)},$$

where  $ke(\lambda)$  is the Asymptotic Flux Extinction Coefficient (AFEC) expressed in  $\text{m}^{-1}$ ,  $z$  is the depth increasing downwards and  $z_0$  is a reference depth.

Moreover, the values on y axis of Fig 3 and 6 have been noted positive to ensure consistency throughout the manuscript.

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

As explained in the manuscript, shallow snowpacks where ground is expected to have a significant contribution to the total energy absorption are discarded. For this we use the quantitative criterion that total snowdepth must be higher than 50cm (already indicated in the manuscript page 7 line 4). Nevertheless even for snowpack thicker than 50 cm, the near proximity of the ground (few centimeters) might impact the radiation field. In the present manuscript we did not established any quantitative criterion to discard these cases because the minimal ground-ZOI distance is of 18 centimeters which we consider too far to impact significantly our result. This has been explicitly added in the manuscript page 7 line 4

“Note that the minimum distance between the ZOI and the ground is of 18 cm, which we believe thick enough to prevent any significant disturbance of the measured signal due to the presence of the ground.”

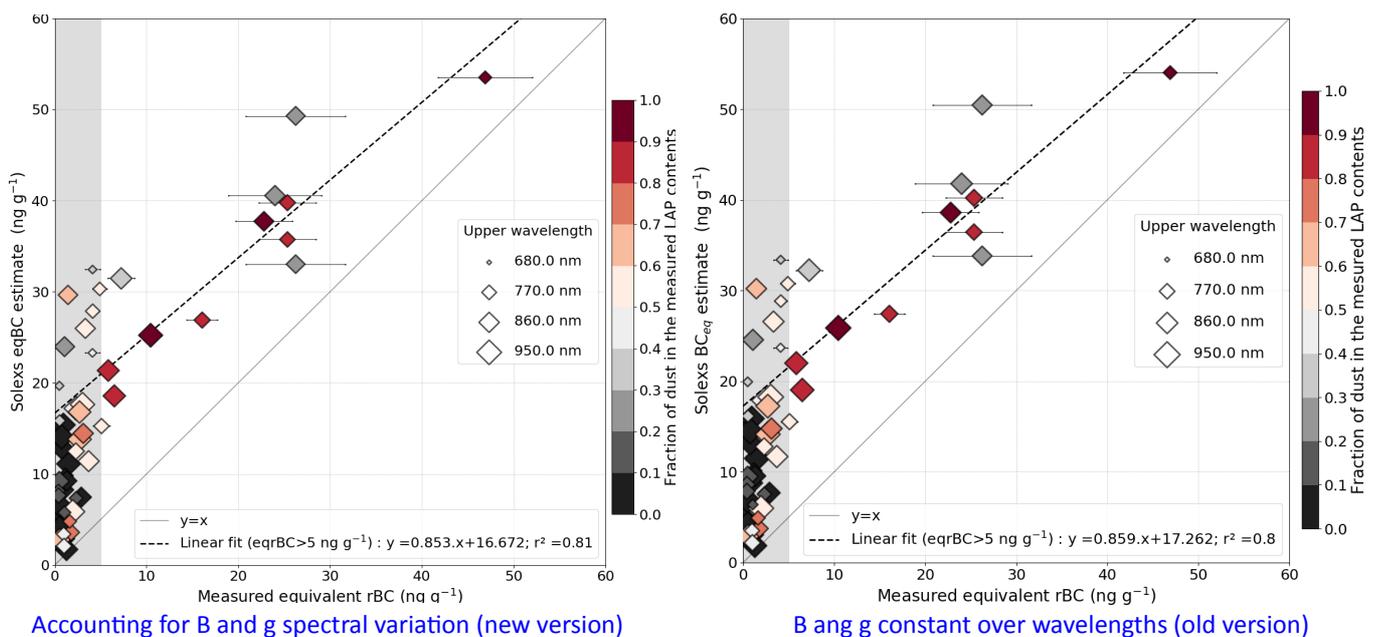
7. p. 8, Eq. (6): please define B (absorption enhancement parameter?).

Indeed B is the absorption enhancement parameter. It has been added in the manuscript p.8 l.19:

with  $\sigma_a(m^{-1})$  the absorption coefficient of snow due to ice and B the 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 Räsänen et al. (2015) for a few assumptions about snow grain shapes. (I think their 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  $\pm 5\%$  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.

The spectral variations of B and g were neglected in this study since we believed the impact was negligible. However the assumption was not clearly formulated in the manuscript and your remark pointed out the relevance to assess this impact on our method. As the implementation of B and g spectral variations in our algorithm was quite straightforward, we implemented B and g spectral variations as in Appendix F of Libois 2014 PhD, which are taken from Kokhanovsky (2004). As expected the impact on our retrieval is minor, which is illustrated on the two figures hereafter.



We decided to keep the spectral variations of B and g in the manuscript. It is now explained in the manuscript p.10 l.5.

The snow shape parameters B and g are constant over time and for all types of snow. These parameters have a small dependence to the wavelength  $\lambda$  implemented following Kokhanovsky (2004) and Appendix F of Libois (2014). This dependence is a function of the real part of ice refractive index  $r_i$  which is taken from Warren and Brandt 2008 and is written as follows:

$$B(\lambda) = B_0 + 0.4 (r_i(\lambda) - 1.3)$$

$$g(\lambda) = g_0 - 0.38 (r_i(\lambda) - 1.3)$$

The absorption enhancement parameter  $B_0$  is set to 1.6 and the asymmetry factor  $g_0$  is set to 0.85, considered to be good approximations to describe all type of snow (Libois et al 2014b). As the spectral dependence of  $B(\lambda)$  and  $g(\lambda)$  is small over the range of wavelength targeted by this study, they are referred as B and g for sake of simplicity.

9. p. 10, lines 20-21: "The wavelength range of the estimation and the  $\eta_{\text{mes}}$  do not impact the eqBC retrieval". Do you mean "...do not impact significantly"?

By this sentence we meant that neither the wavelength range used for the retrieval estimation nor the value of  $\eta_{\text{mes}}$  are found to be correlated with the accuracy of the retrieval.. Given the uncertainties associated to our method it is not possible to guarantee that there is absolutely no impact of these two parameters on the retrieval accuracy. Consequently the sentence has been modified in the manuscript p.11 l.9:

Neither the wavelength range used for the estimation nor the value of  $\eta_{\text{mes}}$  are found to be correlated with the accuracy of the retrieval.

10. p. 11, line 5: The range of dust MAE (0.071–0.127  $\text{m}^2 \text{g}^{-1}$  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  $\text{m}^2 \text{g}^{-1}$  (even though the maximum represents Sahel only).

Indeed the range of dust MAE is quite small compared to the maximum that can be found in the literature. However as you stated, these maximum are found for dust coming from regions that less likely to affect our study area. A paragraph clarifying this point has been added in the results p.11 l.24:

Caponi et al (2017) suggest that for dust particles smaller than 2.5  $\mu\text{m}$  (PM2.5), which is the major dust type in regard of measured size distribution, dust MAE at 407 nm is between 0.071 and 0.127  $\text{m}^2 \text{g}^{-1}$  (0.103 for Figure 8) for north Saharan dust. It should be noted that higher values of dust MAE can be found in the literature and in turn higher uncertainties associated to this parameter could be considered. However, these values corresponds to source regions that less likely affect our study area (e.g. up to 0.6  $\text{m}^2 \text{g}^{-1}$  for Sahel desert, Caponi et al. 2017).

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?

Under the hypothesis made in our study and following our method, this conclusion is valid for any range of B and g. Indeed, when looking at Equation 12

$$k_e(\lambda) \approx \sqrt{\frac{3(1-g)}{2} \rho^2 \text{SSA} \left( \frac{B \gamma_{\text{ice}}(\lambda)}{\rho_{\text{ice}}} + \sum_i \text{MAE}_i(\lambda) c_i \right)}$$

the impurity retrieval is totally independent of the value of g as SSA is let as a free parameter of our optimization scheme. Any change of can be fully compensated by a change in our SSA retrieval. In turn g variation have only an impact on our SSA retrieval and not on LAP retrieval. This has been clarified in the new version of the manuscript (page 12 line 21) :

"The variations of g do not impact LAP retrievals since SSA is left as a free parameter in our method and can counterbalance any variation of g (see Eq. 12). "

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

The correction has been added to the manuscript p.9 l.18 and p.12 l.23.:

“the refractive index of ice” has been replaced by “the imaginary part of the refractive index of ice”

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).

That is an interesting point and it is now mentioned in the manuscript p.13 l.8.

The estimated dust fraction is almost systematically lower than the measured dust fraction (12/14 points). This may either indicate that the relative absorption of dust versus BC used in this study could be improved or that there are systematic biases in dust or rBC measurements.

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

The mistake has been corrected and a sentence has been modified in the Section 3.4 to explicitly mention AART. p.8 l.11

The Asymptotic Approximation of the Radiative Transfer theory (AART; Kokhanovsky and Zege, 2004) for pure snow shows that for convex crystals :

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 closepacking. 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 interpretation 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?

The effect of the close packing as found by C. He et al. (2017) and the consequence on the extinction in snow under the presence of impurities are in line with our finding of the overestimation by the optical method. However, we have concerns about the findings in C. He et al. (2017) which do not discuss about a large corpus of work in the 90s by Mischenko et al., showing that in snow (very large particles compared to the wavelength) the close packing effect is negligible. These opposite conclusions and the absence of citation of this previous work make us conclude that this subject is still debated and needs to be confirmed. This is the reason why we prefer not to address this aspect in the present manuscript.

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.

The caption of Figure 1 b) has been modified to account for this remark:

b) EqBC concentration corresponding to a given dust concentration using these MAE values and the methods described in section 3.1

17. Fig. 2: I am puzzled about the numerical values here. Ice absorption coefficient reaches down to  $10^{-6} \text{ m}^{-1}$  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  $10^{-3} \text{ m}^{-1}$ , i.e., three orders of magnitude higher. Also, what is assumed about snow density here?

Figure 2 is supposed to represent  $\sigma_a$  for snow and  $\sigma_a$  for LAP. However, the legend and the caption implied that ice absorption ( $\gamma_{ice}$ ) was represented instead of snow absorption due to ice. Moreover the values actually represented were  $\sigma_a/\rho$ . This choice was made to be independent of the snow density value but the explanation was not in the manuscript. As this choice is inconsistent with the writing of the equations, the value represented are now  $\sigma_a$  and not  $\sigma_a/\rho$ . To this end, a density hypothesis of  $200 \text{ kg m}^{-3}$  for snow has been done and is specified in the caption. Moreover, the legend and the caption of the figure were modified to replace “ice absorption” by “snow absorption”.

Finally, in Picard et al. 2016 (The Cryosphere, 10, p. 2655–2672), the values represented are the one of pure ice absorption ( $\gamma_{ice}$ ) and not the one of snow absorption as in the present study, explaining the differences.

The caption and legend of Figure 2 have been modified as follows:

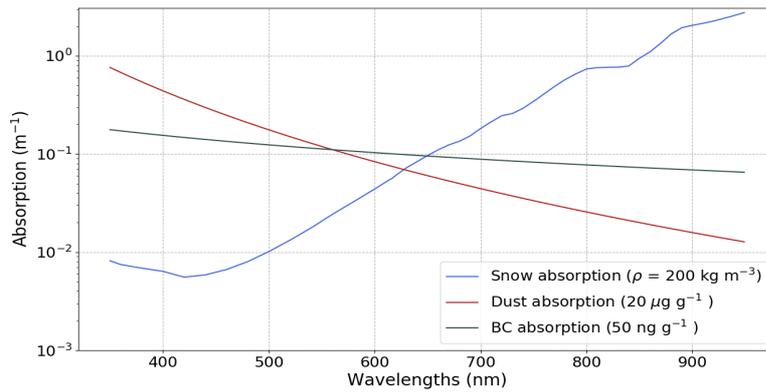


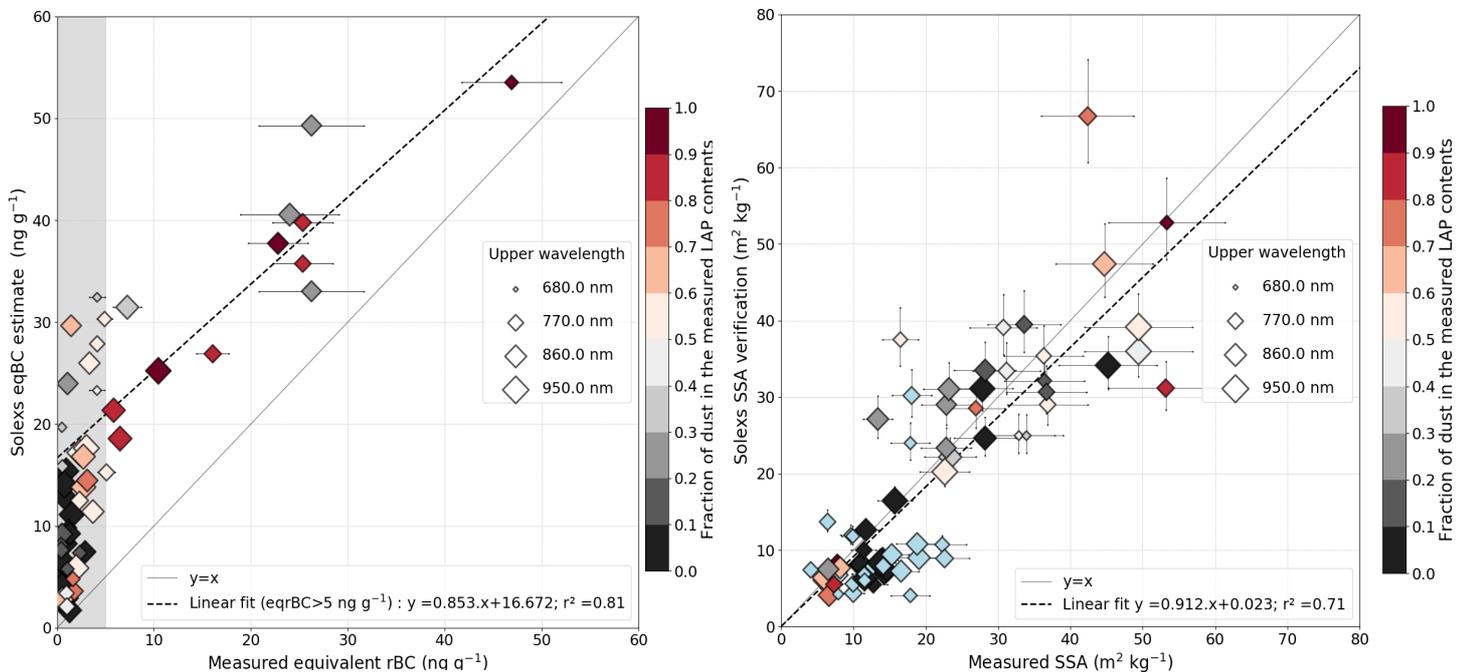
Figure 2. Spectral signature of the absorption coefficients  $\sigma_a$  for snow and different types of LAPs assuming a snow density of  $200 \text{ kg m}^{-3}$ .

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).

Thank you for this suggestion, the modification has been done. A different marker has also been used for each wavelength to facilitate black and white reading. Moreover, according to your comment 5 the y axis have been modified to have positive depth.

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?

This modification has been added to Figure 8 and 10 as follows:



### **Technical and language corrections :**

All the remarks of this section have been corrected in the manuscript; additional information can be found after the comment when necessary.

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.  
The verb comporting has been replaced by “with concomitant measurement” in caption of Figure 10 and 11 to be consistent with Figures 8 and 9.
10. In Fig. 12, x-axis label, “Mesured” should be “Measured”.

### **References:**

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