Answer to Anonymous Referee #1

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

The reviewer's initial comments are reported 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.

Summary comment:

This was a worthwhile project which deserves publication, but I found the paper difficult to read.

Major comments:

(1) Figure 4 compares two established methods for measuring black carbon. It shows, for example, that when rBC=5 ng/g, EC can be anywhere from 4 to 75 ng/g. This is disturbing and demands explanation. The very low rBC values from the SP2 make me skeptical. The authors cite Wendl et al 2014 (on page 8 line 11). However, the Wendl paper discusses the effect of using different nebulizers, so I'm not sure which one was used here. More detail should be given about how the measurements were done, including the expected sampling efficiency of the SP2 set-up.

We agree that only mentioning Wendl et al 2014 can be confusing, so we now explicitly mention the nebulizer used in our case (APEX Q), and reference Lim et al 2014, which describes further evaluation of this particular setup.

Discrepancies between rBC and EC measurements are well documented. We acknowledge that our observed discrepancies are larger than usually observed, and we added a short discussion on that in Section 4.1 p.19 l.23 : "This extra coagulation step is usually not implemented, which may explain why the EC/rBC ratios are higher in our case compared to previous studies. This calls for more systematic comparison of rBC vs EC measurements."

(2) In places the discussion of a figure disagrees with what the figure actually shows. For example, page 1 line 20 says "LAP concentration and SSA are correctly reproduced". But Figure 5 shows disagreements commonly a factor of 10 for LAP, and a factor of 2 for SSA.

This sentence of the abstract was indeed over-optimistic, it has been replaced as follows : p.1 l.15 : "The temporal variations of near-surface LAP concentration and SSA are most of the time correctly simulated."

Page 15 line 22-23, describing Figure 5a, says "temporal variations of near-surface AEC are correctly reproduced. But Figure 5a for 2018 shows poor agreement: e.g. for 2018-03 the red line is at _3

ng/g, but the black crosses (autosolex) show two clusters of values, one an order-of-magnitude higher at _50 ng/g, and one an order of- magnitude lower at _0.5 ng/g.

Indeed, even if the temporal variations of AEC concentrations are in general captured by the model, there are some periods for which the agreement is poor. This has been underlined.16 l.29 "meaning that temporal variations of near-surface AEC are generally well reproduced despite periods with lower agreement (e.g. March 2018)."

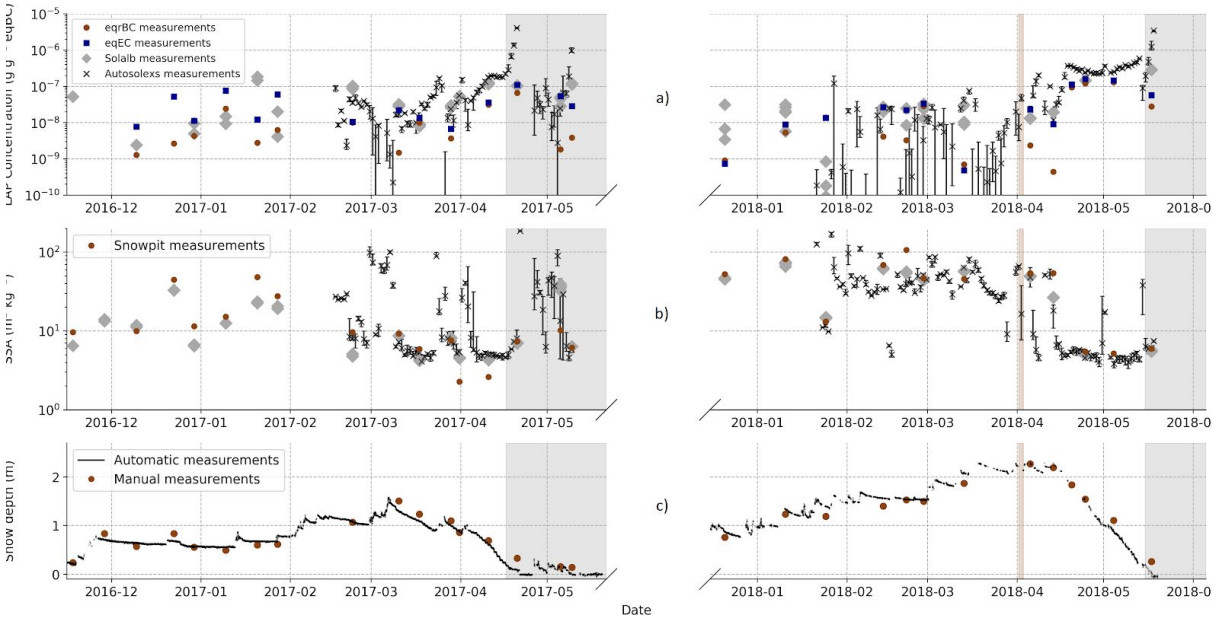
Page 15 line 22 gives the correlation r2=0.78. Was this correlation done linearly or logarithmically? If it was done linearly, the apparently good correlation will be the result of many points near zero (i.e. 10-9 not distinguished from 10-8) and a few points at the high end (10-6).

The correlations are done linearly, which have been indicated in the manuscript in Section 3.2. The logarithmic correlations are indeed lower because of the effect described by the reviewer. The following sentence has been added after the paragraph on AEC measurement description p.16 l.7 :

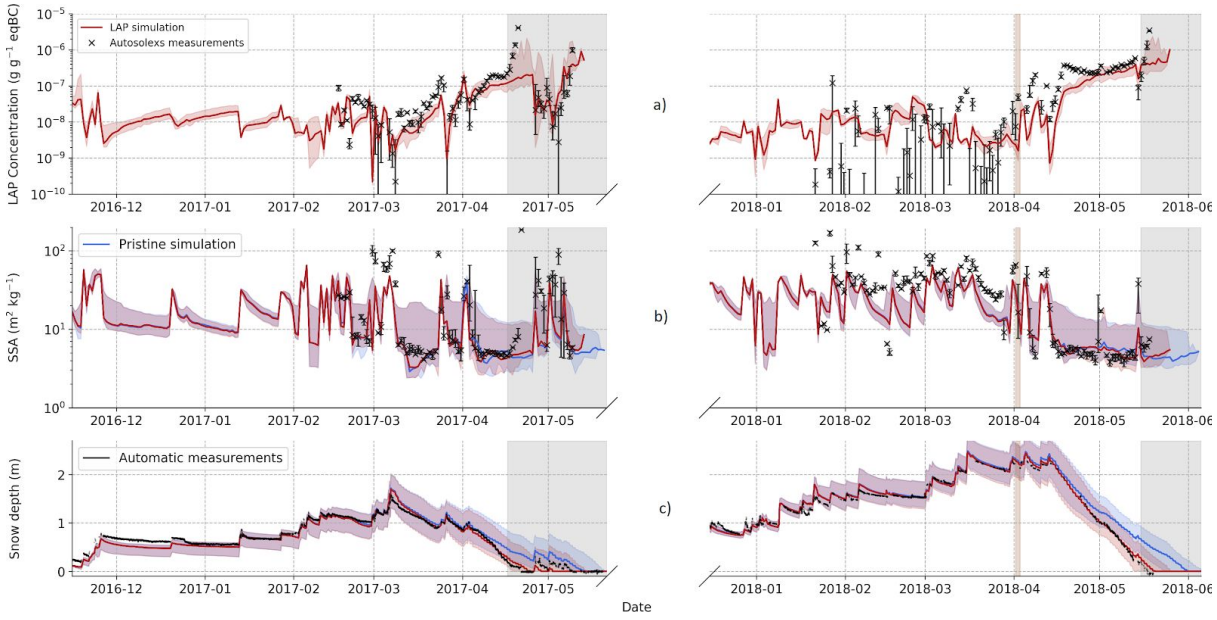
"It is noteworthy that all the Pearson correlation coefficients for AEC presented in Table \ref{tab:LAP} strongly decrease when the regressions are done logarithmically (not shown). The good linear correlation between the different AEC estimates mainly results from two clusters of points: a lower one, with points around 1 ng g^{-1} that are not distinguished from points around 10 ng g^{-1} and a higher one with points featuring concentrations higher than 50 ng g^{-1}. This result is not surprising, as AEC estimations from spectral albedo are expected to have a poor accuracy for low concentrations (< 10 ng g^{-1} approximately, e.g. Warren 2013), explaining the low values of logarithmic correlations."

In the discussion of Figure 5b, page 15 line 29 says "there is no significant bias between Crocus SSA and the measurements". This figure should be redrawn, plotting SSA on a log scale instead of linear. In the middle of March 2017, the difference is barely distinguishable, but with magnification I can see that the black crosses average _5.3 m2/kg and the red line _3.3 m2/kg, indicating a factor-of-1.6 disagreement (and corresponding to effective radii re of 600 and 1000 microns respectively). The same problem would result from changing the vertical axis to be linear in re, because that would shrink the high-SSA region. But there is no reason to prefer either SSA or re as the choice for showing area-to-mass ratio, so to be fair the axis should be in logSSA (or logre); both choices then give the same intervals.

The representation chosen in the original version of the manuscript was indeed clearer for high SSA than for lower ones. Figures 3 and 5 have been redrawn following reviewer's recommendation (they are presented below this comment) and we believe they now provide a clearer overview of the data for both low and high SSA. Moreover, the relative difference between simulated SSA and the black crosses is significant in the middle of March 2017. This is not a systematic bias as the one observed for high SSAs, but it is worth to be underlined in the description p17 l.2 : For SSA lower than 15 m2/kg there is no significant bias between Crocus SSA and the measurements, except for a short period in mid-March 2017



New Figure 3



New Figure 5

Page 16 line 12 states “the highest AECs estimated from Autosolexs are within the simulated concentrations by our ensemble”. I assume AEC means LAP concentration? If so than the statement is not true. If I am reading Figure 5a correctly, the highest AECs from Autosolex (black crosses) are above the red shading, e.g. 2017-03, 2018-03, 2018-04.

By this sentence, the authors meant that the seasonal maxima of Absorption Equivalent Concentration (AEC; at the end of the ablation phase) was close to the simulated AEC ensemble. However this was not clear and simulated AEC are in fact slightly underestimating autosolexs seasonal maximum. This sentence has been reformulated as follows and moved p.17 l.27

During the period with significant RF (April 2017, April and 2018 and May 2018), the AECs estimated from Autosolexs measurements are within or slightly above the concentrations simulated by our ensemble (Figure 5). This means that the simulated RF, presented here, are expected to be representative of Autosolexs measurements or slightly underestimated.

Minor comments:

p 7 line 11. “350 to 1050 nm” What is done to account for the rest of the solar spectrum, 300-350 and 1050-2800 nm?

The instrument only acquires signals in this range of wavelengths. For SSA and LAP content retrieval, this part of the solar spectrum is sufficient.

p 11 Eq.3. Instead of requiring the reader to consult Dumont 2017 (which in turn requires going back to Libois 2013 and Picaro 2016), it would help the reader if the authors would describe this equation briefly. For example, why the factor 32/3? And I think B has not been defined.

This Section has been modified in order to better explain the core of the spectral albedo model used in the retrieval method. However, the reader may have to read Dumont et al. 2017 in order to understand all technical information related to the retrieval method. Redefining the whole method here would be extremely long and is out of the scope of this study. Finally, the enhancement parameter B has been defined in Section 2.1 defined pg 7 line 5.

p 11 line 20. The definitions have been reversed. The density should be rho-ice, not n-ice.

This mistake has been corrected in the revised version of the manuscript.

p 12 Eq.4. It would be helpful to describe the psi-function. For example, approximately how much dust would be needed to have the albedo-lowering effect of 1 ng/g BC? Dang et al (JGR 2015) estimated a factor of _200 for Saharan dust.

In order to clarify the way the eqBC concentration is computed, the psi function (exactly reproduced from Tuzet et al.2019) has been added in the Appendix A1. As it can be interpreted from this figure, 1 ng/g BC has an albedo-lowering effect 250-290 times stronger than Saharan dust in the range of dust concentrations studied here. Of course, this strongly depends on the hypotheses on dust and BC MAE but gives an order of magnitude

The following sentence has been added in the manuscript p.14 l.9 : Appendix A1 illustrates the hypotheses of BC and dust MAE taken here as well as the psi function. More details about the computation of C_{eqBC} are given in Tuzet et al. 2019 .

p13 Eq.7. The numerator looks wrong. I think it should instead be E(indirect)- E(pristine).

The numerator is indeed E(indirect)- E(pristine), the mistake has been corrected in the manuscript. This does not affect the results, all the results of the manuscript were obtained with the correct formula.

p 14 line 2. Change “2c” to “2b”.

Done. p.x l.x, the opposite mistake was also corrected and “2b” became “2c” when mentioning air temperatures.

p 15 line 1. Change “50 g-1” to “50 ng g-1”.

Done.

p 15 line 26. “The dispersion . . . is quite low regarding the median value”. I don’t understand this phrase; perhaps “regarding” is the wrong word.

Indeed, what was meant here was “relative dispersion” and not “dispersion regarding the median value” which was an erroneous formulation. The sentence has been replaced by : “The relative dispersion of near-surface LAP concentrations in the ensemble is moderate, ...” p.16 l.32

p 16 line 17. How can snow cover lower the amount of incoming radiation?

In this sentence, snow cover has to be replaced by cloud cover. The mistake has been corrected p.17 l.22: “cloud cover that lowers the amount of incoming radiation”

p 25 line 32. This paper has now been published in JGR, so the citation can be updated.

The reference has been updated.

Figures 2a and 3c (showing snow depth, with gaps), disagree with Figure 5c (which has no gaps). 2a and 3c will be easier to read if the gaps are filled in.

The snow depths plotted on Figure 5c are exactly the same data as Figure 2a and 3c, the gaps seem to have been filled because of the superposition of the model data but it is only a visual artefact. The authors voluntarily decided to let missing data as they are in order not to plot hypothetical information at dates for which we have no measurements.

Figure 2a has two kinds of vertical blue dashed lines. What is the distinction between the bold lines and the faint lines?

As the ROS vertical lines are plotted with transparency, and as the width of the plotted lines are larger than 1 hour, the ROS events appear with bold lines when their duration is higher than 1 hour.

Figure 2 caption line 1. Reverse (b) and (c). Wind speed is (b).

Done.

Figure 3 caption line 3. “grey diamonds”. I don’t see any grey diamonds.

Indeed, the grey diamonds corresponding to solalb data were not plotted in the original version of the manuscript. This mistake has been corrected as follows :

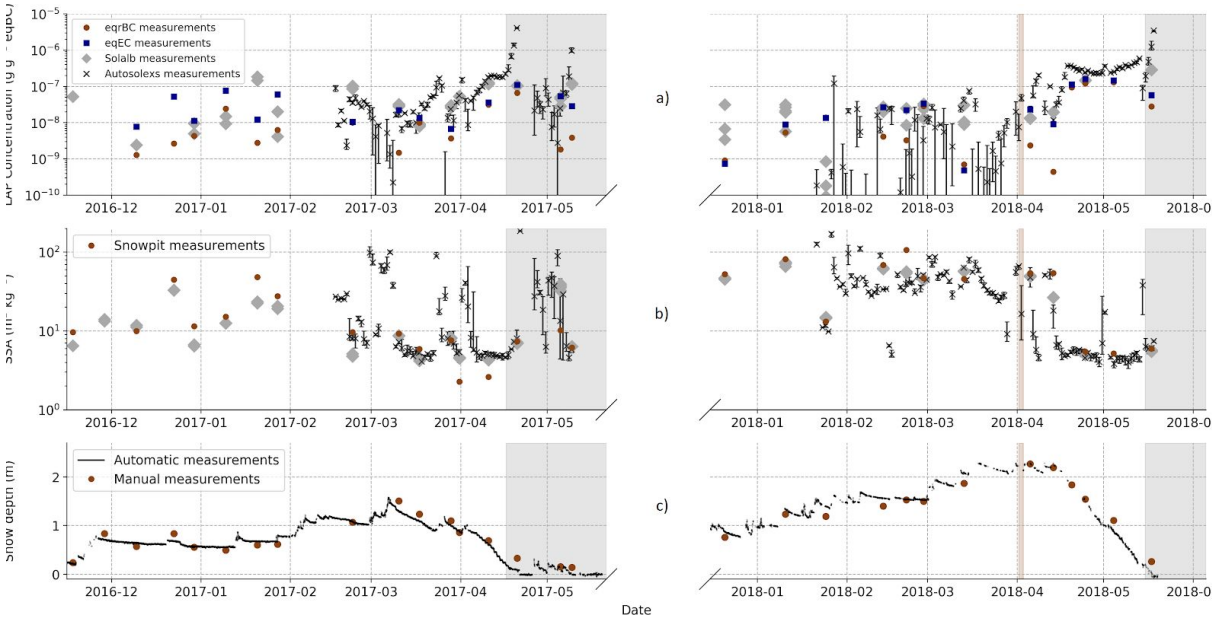


Figure 4. Give units for both horizontal and vertical axes.

Done

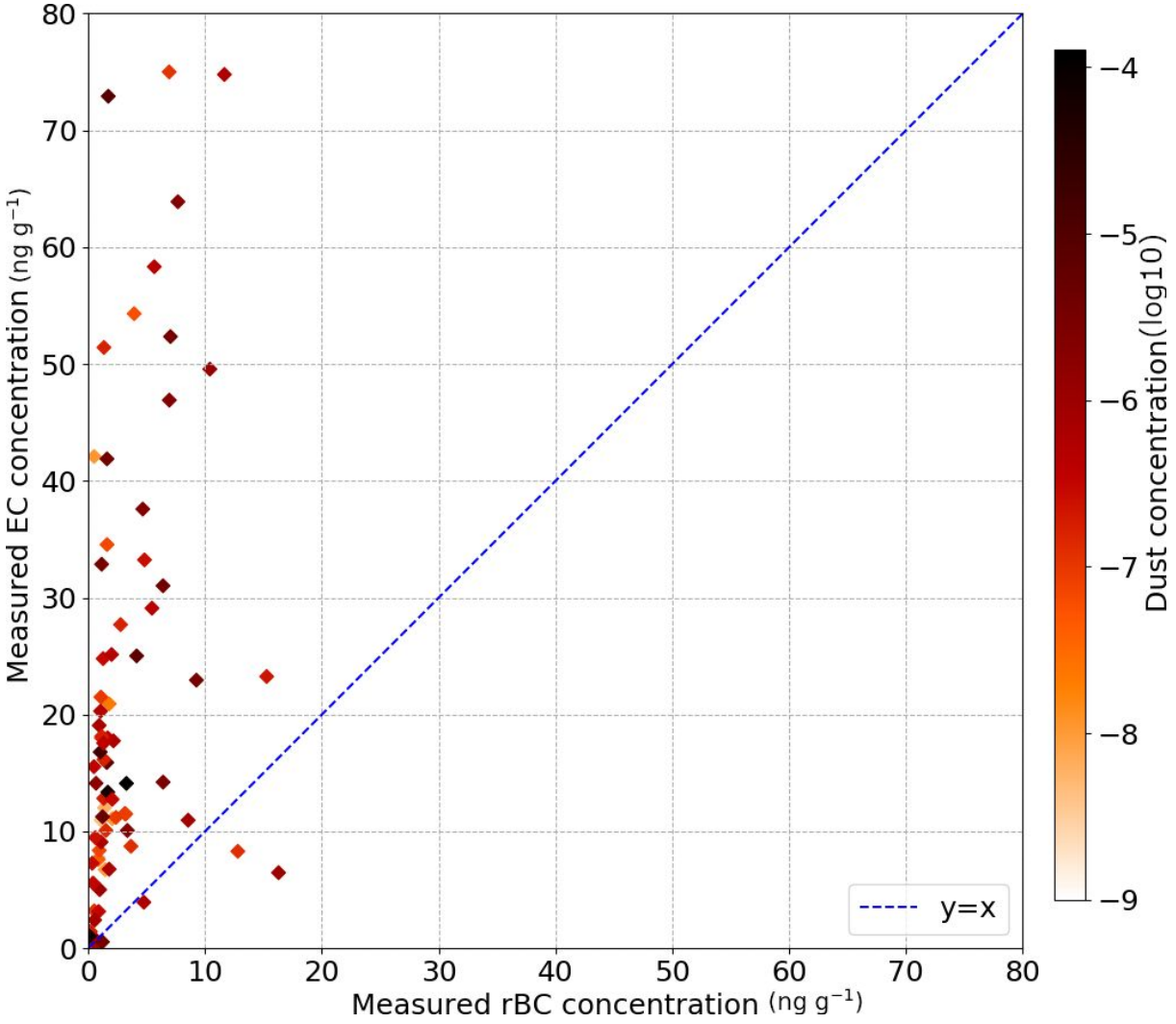


Figure 5. Do the ticks on the horizontal axis mean the beginning of the month or the middle of the month?

For all figures, the horizontal ticks represent the beginning of each month. The following sentence has been added at the end of the legend of Figure 2, which is the first figure to use these temporal ticks : The ticks on the abscissa axis correspond to the first day of each month.

Figure 5 caption line 1. Change “(b)” to “(a)”. Change “(c)” to “(b)”.

Done

Figure 5 caption is confusing. Line 1 says “measured and simulated near-surface LAP”. But no measurements of LAP are actually plotted here. What is plotted is not measured LAP, but rather LAP inferred from albedo.

Based on the terminology defined in the manuscript, LAP concentration has been replaced by AEC in the caption.

Figure 6. The horizontal grid lines for the right-hand plots (2018) differ from those in the left-hand plots (2017), indicating a different scale. But the vertical axis has a scale only on the left-hand plot. Add vertical-axis labels to the 2018 plots.

The yticks were indeed disturbingly positioned in Figure 8 of the original version of the manuscript. After verifying the code, the y-limits were the same on left-hand and right-hand plot so that the data presented already corresponded to the min and max values. The mistake has been corrected in the revised version of the manuscript as follows :

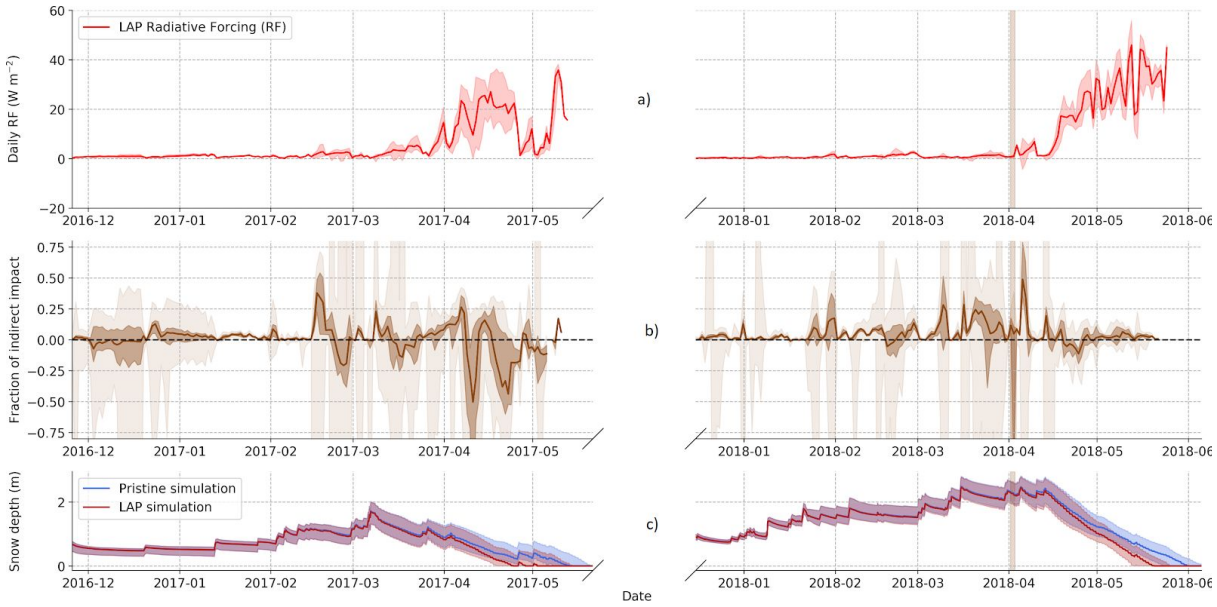


Figure 8 caption last line refers to “brown shading” for the major dust deposition event. I do not see the brown shading.

The extreme dust deposition occurred before the beginning of the selected period. The sentence referring to the brown shading has been removed.

References :

Dumont, M., Laurent, A., Picard, G., Libois, Q., Lejeune, Y., Nabat, P., ... & Morin, S. (2017). In situ continuous visible and near-infrared spectroscopy of an alpine snowpack. The Cryosphere, 11(3), 1091.

Tuzet, F., Dumont, M., Arnaud, L., Voisin, D., Lamare, M., Larue, F., ... & Picard, G. (2019). Influence of light-absorbing particles on snow spectral irradiance profiles. Cryosphere, 13(8).

Modifications of the original manuscript that were not suggested by the referees :

Following the comment of another member of the community, the authors realized that the term Radiative Forcing (RF) should not be used to describe the radiative impact of impurities in snow. The term Surface Radiative Effect would be preferable, however given the common use of the term RF in the community and the numerous acronyms already defined in the manuscript we decided not to change RF for SRE. This has been clarified just after Table 1 p.6 l.3 "Strictly speaking, what is called Radiative Forcing (RF) of LAPs in snow should better be called Surface Radiative Effect. However, given the common use of the term RF in the literature (e.g. Skiles et al. 2018) and the numerous acronyms already defined in this manuscript, we decided to keep the term RF for sake of simplicity. "

Skiles, S. M., Flanner, M., Cook, J. M., Dumont, M., & Painter, T. H. (2018). Radiative forcing by light-absorbing particles in snow. Nature Climate Change, 8(11), 964-971.