The Cryosphere
Discussions

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Interactive comment on "Quantification of the radiative impact of light-absorbing particles during two contrasted snow seasons at Col du Lautaret (2058 m a.s.l., French Alps)" *by* François Tuzet et al.

Anonymous Referee #2

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The manuscript by Tuzet et al. illustrates an interesting dataset of two years of measurements and modeling at the Col du Lautaret experimental site. The site is quite unique and the analysis of those data represents for sure a step forward in the snow science. The manuscript fits well the aim and scope of TC, but I found it a little hasty in some sections. The BC measurements are unprecedented in the Alps, but the presentation should be modified by comparing the concentrations measured in this manuscript with other publications on this topic. It's also important to present the data with the same units (e.g. ppb or ppm) of other studies, so data can be compared. I suggest to

present dust concentration in ppm and BC concentration in ppb, and directly compare these concentration with other measurements in other mountain chains or ice sheets. I think that some further work is needed before publication in TC.

Some specific comments below.

pg1 ln1. the abstract is way too long. I suggest to shorten it.

pg3 ln32. add "is" between "concentration" and "determined"

pg4 ln18. those mentioned are not "chemical techniques"

pg4 ln21. add more details regarding the radiative impact of dust on snow

pg5 ln10. I suggest to add some discussion also on the paper by Niwano et al. 2012 that made use of SMAP model

ref: Niwano, M., Aoki, T., Kuchiki, K., Hosaka, M., and Kodama, Y. (2012), Snow Metamorphism and Albedo Process (SMAP) model for climate studies: Model validation using meteorological and snow impurity data measured at Sapporo, Japan, J. Geophys. Res., 117, F03008, doi:10.1029/2011JF002239.

Section 2 "Materials". this section includes also several methods. I don't understand why the authors separated material and methods in two sections. I suggest to merge them and to harmonize the content.

pg7 ln25. How did you measure the slope/aspect? What are the uncertainties in these measurements? How these uncertainties impact on the albedo correction?

Section 3. I suggest to add more details on the retrieval methods. The reader is continuosly addressed to other papers from the same group.

pg13 ln1. the RF calculation is here strongly dependent on the simulations. A more useful (and replicable) RF estimation would make use only of Autosolex data. Please add this discussion here or later in the manuscript.

Equation 6. I think it should be E_pristine - E_lap

Section 4. from this section I'm missing a comparison between Autosolex, Solalb and simulated spectral albedo

pg14 ln10. "extreme dust deposition". We still don't know the (climatic) average of dust deposition on snow in the Alps. I suggest to replace "extreme" with "strong".

pg14 ln24. I don't see this regression in the manuscript. it should be added.

Figure 4. Figure 4 is a bit puzzling to me. Units are missing from the axes. From this plot I learn that for rbC <10 (ppb?) all possible values of EC are found in experimental data. Dust color coding is totaly useless since it does not add any information to this presentation. More explaination is needed in the text. Authors may also evaluate to delete this plot and find a better way to present these data.

pg15 ln1. "are lower than 50 gˆ-1 eqBC". I thing that ng is missing from the unit.

Figure 3. SSA variability is not particularly clear. Data are very scattered during the accumulation period. This is due to bad retrieval caused by atmospheric variability? the accumulation period of 2018 shows overall higher SSA values with respect to 2017, why? Please describe here or in the discussion section. Always on Figure 3: revise the label in order to present all data in the plot. In fig3a, the label is missing the autosolex measurements. Fig. 3a also shows an increase of LAP concentration during late April 2017. I suggest to present in the manuscript also the prescribed BC and dust depositions simulated by the model for the two years investigated.

pg15 ln 32. Why 65 mˆ2/kg has been selected as a upper bound for SSA?

Section 4.4. A comparison between TARTES and Autosolex could be interesting here.

pg16 ln22. RF values found in this study should be compared with other studies already published.

pg16 ln33. This is strange. The first year featured higher surface concentration of LAPs

and a stronger shortening of the snow season. Here the authors should try a process-based interpretation of their data. It was BC from the atmosphere? possible input from biomass burning or other emissions? Are there undetected dust events? Giving a look to the albedo spectra may help in the interpetation of LAPs concentration since dust and BC have a different impact on the spectra.

pg17 ln26. please add some references to the last sentence.

pg19 ln15. What is a "numerical outcropping"?

Figure 6. in this figure we only see modeled data. It would be interesting to add also retrieval from autosolex data.

Figure 7. Not particularly informative. I suggest to remove it, and to present average numbers in the text.

Figure 8. not easily understandable. I suggest to think a better way to present these interesting data

Figure A2. Here I don't understand why slope is changing sign during the season. It is very odd and makes me question the slope and aspect retrieval developed by the authors. Are those data somehow validated? It would be also informative to plot the slope-aspect of the underling terrain.

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