Response to RC3:

General comments:

a) the paper by Tuzet et al. proposes a very interesting integration of a snow model (CROCUS) with a radiative transfer model (TARTES) to estimate the impact of LAIs on the snow pack evolution in the French Alps. The authors calculate the direct and indirect radiative forcing and come up with an estimated earlier snow melt of about one week in 2014. The paper is well written and the messages are clear, it represents definitivey an advance in the study of LAIs on snow in Europe. There are only some issues to be resolved before final publication in TC.

The author are grateful to the referee for this review and interest in the manuscript, the issues highlighted are addressed in the point by point response hereafter.

b) I was quite impressed by the high concentration of BC estimated by the authors. In Figure 4, points represent the BC concentration estimated from measured spectral albedo (Dumont et al. 2017). I suggest to explicit it in the legend, otherwise the reader may think that they are the actual measured concentration of BC. To me, these concentrations are very high (more than 10ˆ3 ppb), for example Khan et al. 2017 found similar values next to an active coal mine in the Arctic.

The concentrations estimated from measured spectral albedo (Dumont et al. 2017) are BC equivalent concentrations. They include all type of LAIs such as mineral dust, organic debris, organic carbon. This could explain the high concentrations found. Flanner et al. (2007) report concentrations of BC in the Alps up to 800ng/g, so accounting for the other types of LAIs (especially mineral dust and organic debris) it is not unrealistic to have this BC equivalent concentration. Note also that there is also a high concentration of plant debris in Col de Porte snow due to the nearby forest.

The label “Measured” has been replaced by “based on measured albedo” as it was already for near-surface SSA. This is also explained page 10 lines 21-22 in the manuscript.

c) A possible BC overestimation may lead to erroneous conclusions on the impact on snowpack dynamics. To present these data, the authors should validate the BC estimation from spectra, showing a quantitative correlation between estimated and measured BC concentration at Col de Porte. The only comparison provided regards the snow profile from 11 February 2014 (which is before the two dust events). From these plots, it is clear that the model is strongly overestimating the BC concentration (and underestimating dust).

Possible sources of BC overestimation are discussed in section 5.1. However considering that the model simulates reasonably well the BC equivalent content (ie. meaning correct radiative forcing), we believe that even if BC is overestimated and dust underestimated, more accurate LAI simulated content will not improve the results in terms of snow melt rate. See also responses and modifications to the comment d) below.

d) From this plot one may conclude that there is very little BC in Col de Porte. Furthermore, since both BC and MD impact the albedo in visible wavelengths, decoupling their effect from spectral data is still an open issue in the remote sensing of LAIs in snow (see for example Warren 2013 JGR). In my opinion, the estimation of BC from (hyper)spectral data should be always coupled with a validation scheme.

Unfortunately, only one measurements of BC at Col de Porte has been performed this year. This issue is already discussed in Dumont et al., (2017). A discussion point has been added in the paper: page 15 –line 29

The upper panel of Figure 4 points out that C5 improves the simulated late season near-surface impurity concentrations compared to all other configurations.

However, in order to test this hypothesis a more detailed evaluation of the LAI (BC and dust) contents in snow should be performed using direct measurements of LAI and not LAI content estimated from (hyper)spectral measurements (e.g. Warren, 2013) which are uncertain for low impurity content (Dumont et al., 2017) but is beyond the scope of the present study. ”

e) The problem here may be hidden also in the spatial scale (as ackowledged in Section 5.1). ALADIN-climate works on a very coarse scale (50km) and the AWS used for this study provide a point measurement. It is understandable that the match is not perfect in simulated variables, but since the paper is focused on the impact of LAIs on snowpack evolution, I would ask: there was any BC in/on snow? If not, I would propose to strongly cut the discussion on BC and postpone it to a future paper in which actual BC measurements are provided.

See responses to comments c) and d). The discussion on BC in snow has been kept in the revised version of the manuscript since it highlights the limitations of the modeling chain and of the evaluation dataset.

f) Another question on BC: where does it come from? It is plausible that it comes all from air contamination in Grenoble? Is there any atmospheric inversion that leads to the accumulation of BC in the lower atmosphere? Is ALADIN-climate able to reproduce it?

Winter atmospheric inversions are indeed commonly observed in Grenoble. Considering the coarse scale of ALADIN-Cimate, these events can not be represented correctly .

The response m) of the specific comment RC1 for a more detailed response and subsequent modification in the paper further addresses this topic.

g) In the discussion section, the authors state that snowmelt advances 6-9 days due to LAIs deposition. This was due to BC or dust? If they ran the CROCUS simulations separately for the two impurities, it should be possible to estimate the partition of the impact. I would expect that most of the advanced snowmelt was due to the two big Saharan events in February and April 2014.

In order to address this question, additional simulations with BC only or dust only have been performed. The results show that for C2, C3 and C4 BC is responsible for most of the radiative impact whereas for C5 half of the radiative impact originates from dust. However, since we are not able to accurately evaluate the simulated BC and dust contents separately (see responses to comments c) and d)), we decided not to include these results in the paper.

These limitations have been however underlined page 18 – line 19 : For example, a direct evaluation of the dust and BC contents is required to quantify more precisely their respective part in the shortening of the snow season.

h) If this is not true, maybe the overestimation of surface BC concentration may lead to erroneous conclusions. From an environmental/ climate perspective it is very important to understand if some anthropogenic activity (e.g. BC emission from fossil fuel combustion) is involved in snow darkening in the European Alps.

An overestimation of surface BC concentration may lead to an overestimation of the melt rate or may be compensated by an underestimation of the mineral dust concentration. We do not have enough chemical measurements at Col de Porte to accurately conclude on the partition between mineral dust and BC relative impacts. However if ALADIN-Climate deposition fluxes are correct, at least half of the impact comes from BC (cf response f) above).

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Specific comments:

i) pg3 line5: add some references here for the different type of impurities.

References for the different types of impurities have been added.

Page 3 Line 5 has been modified accordingly: such as mineral dust (Painter et al. 2010), black carbon (BC) from combustion sources (Flanner et al. 2007), volcanic ash (Conway et al. 1996), soil organics (Takeuchi 2002), algae, and other biological organisms and constituents (Cook et al. 2017)

j) pg3 line26: actually the estimated advance was higher, please check the correct number in the referenced paper(s).

Painter et al. (2013) indeed pointed out that the shift in total melt-out due to dust radiative forcing can be up to 50 days.

The reference Page 3 Line26 has been modified accordingly: can advance total melt-out by up to 50 days

k) pg5 line12: replace "they" with "the author" (it was a single-author paper)

Done

l) pg9 line22: replace "gaz" with "gas"

Done

m) pg11 line11: please consider a reference to Varga et al. 2014, which also documents the Saharan events

This has been included in the introduction
Page 4 Line 1: dust outbreaks, are very sporadic events mostly occurring from April to August (Varga et al. 2014)

n) pg17 line17: this is important, since Saharan dust particle diameter is usually 6-7microns. Assuming a Rayleigh scattering may lead to underestimate the impact of dust on snow. In any case, since you measured dust concentration with a Coulter counter, it would be useful to provide the measured mean diameter of dust particles from the profile of 11 February.

The Coulter counter measurements indeed provide information on dust particles diameter. Assuming dust particles to be spheres, we calculate their volume and compute a volume-weighted size distribution of dust particles. Figure 5 below presents this size distribution of dust particles according to their volume contribution which has a mode around 3 micrometers .

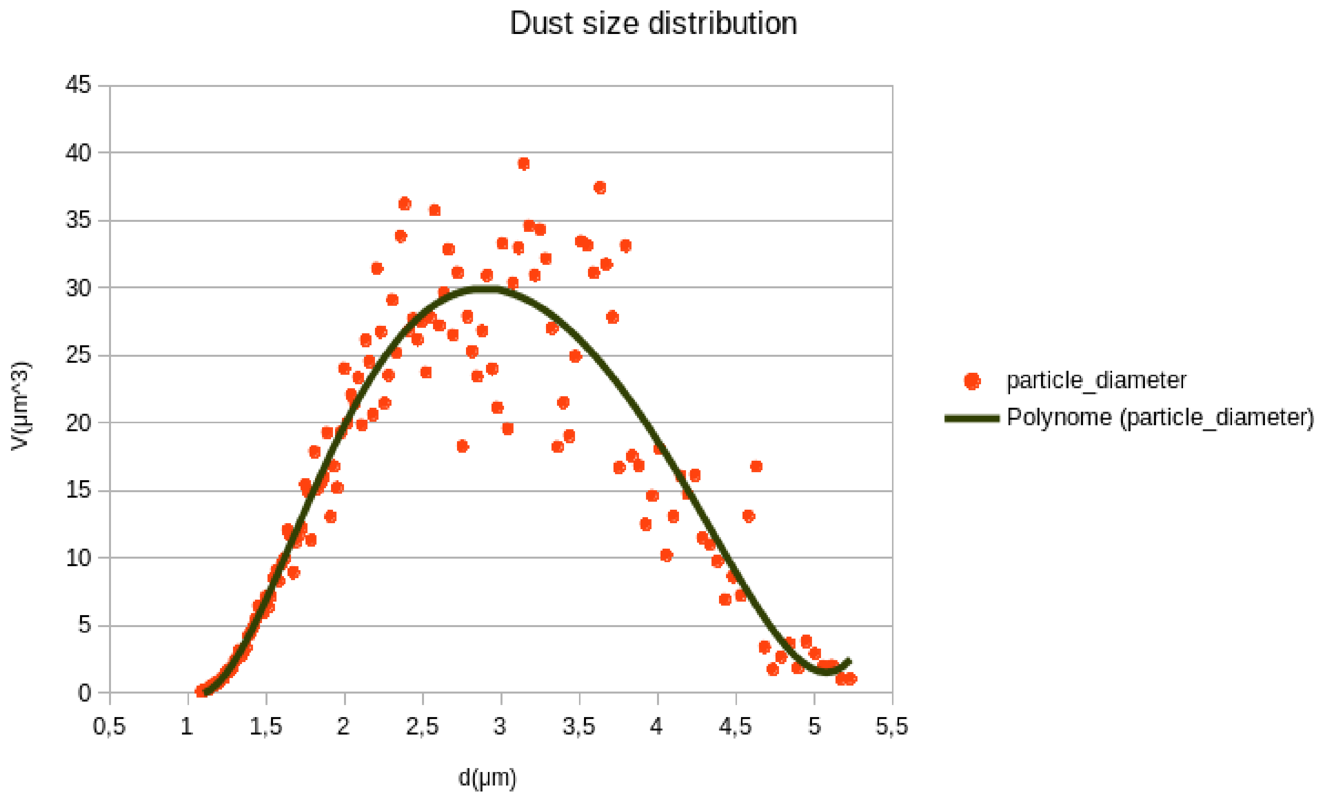

Figure 5: Dust particles diameter distribution according to their volume contribution, obtained from the Coulter counter measurements performed on the 11 February 2013 at Col de Porte.

Page 17 Line 17 has been modified accordingly: This theory is acceptable in the case of BC but may not perfectly apply to dust, depending on its volume size distribution, and may lead to an underestimation of dust radiative impacts. Coulter measurements show that the average diameter according to their volume contribution for our dust is 2.8 µm, which indeed suggest that dust radiative impact can be over-estimated in this study.

o) pg 19 line1: this is very interesting, last year a report was published in the journal "Neve e Valanghe" on this topic. You can find it here (http://www.aineva.it/pubblica/neve88/nv88_5.pdf), unfortunately it is available only in italian.

The authors are grateful for this reference, in the future the authors consider using the recent developments in Crocus to investigate the link between Saharan dust outbreaks and snow stability.

References:

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