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The Cryosphere
Discussions

Interactive comment on "A multi-layer physically-based snowpack model simulating direct and indirect radiative impacts of light-absorbing impurities in snow" *by* Francois Tuzet et al.

Anonymous Referee #3

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General comments:

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.

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. 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). 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. 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. 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?

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 snomelt was due to the two big Saharan events in February and April 2014. 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.

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Specific comments:

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

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

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

pg9 line22: replace "gaz" with "gas"

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

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.

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.

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References:

Khan, A. L., H. Dierssen, J. P. Schwarz, C. Schmitt, A. Chlus, M. Hermanson, T. H. Painter, and D. M. McKnight (2017), Impacts of coal dust from an active mine on the spectral reflectance of Arctic surface snow in Svalbard, Norway, J. Geophys. Res. Atmos., 122, 1767–1778, doi:10.1002/2016JD025757.

Varga, G., Cserháti, C., Kovács, J., Szeberényi, J. and Bradák, B.: Unusual Saharan dust events in the Carpathian Basin (Central Europe) in 2013 and early 2014, Weather, 69(11), 309–313, doi:10.1002/wea.2334, 2014.

Warren, S. G. (2013). Can black carbon in snow be detected by remote sensing? Journal of Geophysical Research: Atmospheres, 118(2), 779–786. doi:10.1029/2012JD018476

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