Response to: Interactive comment on "Saharan dust events in the European Alps: role on snowmelt and geochemical characterization" by Biagio Di Mauro et al.

Reviewer: Dr. Stanislav Kutuzov

Authors responses are in *italic*, Reviewer's comments are in **bold.** Line numbers refer to the track-changes version of the manuscript.

The study "Saharan dust events in the European Alps: role on snowmelt and geochemical characterization" by Biagio Di Mauro and co-authors is dedicated to a very important topic of impacts of mineral dust on melting of snow in mountainous region. This research is based on observations over 3 years in high-altitude site in European Alps, AWS data and modelling. Additionally authors present a novel and relatively simple technique to monitor dust occurrence on snow. Results of the geochemical analysis of snow sample from one of the dust deposition events were also presented and compared to the chemistry of "clean" snow. Paper is well written and contains a comprehensive description of the research together with substantial literature review, results and discussion. I recommend this manuscript for publication after a minor revision.

Dear Dr. Kutuzov, thank you for the positive evaluation of the manuscript. We have carefully considered each of the Reviewer's comments and suggestions. The Reviewer will find below the responses to general and specific comments.

As a general comment in my opinion text could be structured better. In many instances it goes beyond the topic and sometimes the discussion of the methods and results can be found all over the manuscript. Although it is important to mention relevant issues with the methods and data but it is expected that discussion of the results comes after the description of data and method. This complicates reading of the manuscript.

Authors did a great job reviewing a substantial number of previously published researches but the resulted introduction seems excessive and includes a number of repetitions. Some of the statements are repeated later as well. In some instances sentences located in different places are actually stating essentially similar findings and can be combined. I recommend shortening of the introduction and text generally by removing repetitions and information which is not directly related to the conducted research or discussion.

Thank you for this comment. Following your indications, we shortened the introduction and removed repetitions and information not directly linked to our research.

Some specific comments are listed below.

P.1 L.31-34 This was said in previous sentence. Listing of these feedbacks in abstract gives a wrong impression that all these feedbacks were assessed and evaluated here which is not the case. I suggest either to rephrase and generalise or simply to drop it.

We removed from the abstract the sentences regarding the specific feedback effects of the anticipated snowmelt induced by dust depositions. The sentence now reads:

"We conclude that the effect of the Saharan dust is to anticipate the snow melt-out dates, that is known to have a series of hydrological and phenological feedback effects"

P2. L21-26 This should be either shortened and moved to line 15 after "The alterations of the optical properties of snow are known . . .. (. . .. Painter et al,. 2012)." Or removed.

We moved this paragraph after line 15, and we shortened it. Now it reads:

"First estimations of the impact of dust on snow date back to the beginning of the last century: Jones (1913) estimated one month of anticipated snow melting due to dust deposition in the US. Drake (1981) estimated 4 days of advancement in the snow melt. These advances in snow melt-out dates have important implications on water supply operations (Painter et al., 2012)."

P.3 L9. This sentence is then repeated a number of times in the text. Please decide where you want to mention it and remove duplications. Bearing in mind that mineralogy of particles is actually out of the scope of this study.

We removed the sentence from line 7 to line 10. This topic is then addressed in the discussion in order to link the geochemical composition of dust with its radiative effect when deposited on snow.

P.3. L.11 This is a bit strong statement (fundamental reservoir). I'd recommend it to be rephrased. Temporal and fundamental do not sound particularly good together. Of course it plays a key role in redistribution and timing of runoff and many other aspects. And then the word fundamental is repeated several times later.

We replaced "fundamental" with "important".

P.3 L11-19 This paragraph should be moved after the effects of dust ecosystems. And the last sentence (L.35-36) can be placed here "Changes in snow falls and dust depositions are likely to occur more frequently in a warming climate."

We modified accordingly.

P3 L.20-36 Please check this paragraph. Order of sentences should be changed so that you first mention what has been done and then point to the knowledge gaps in the Alps.

We modified accordingly.

P.4 Fig. 1 The map should be enlarged and zoomed, font size adjusted. Preferably the geochemistry sample site should be included as well.

We modified accordingly. We also added an aerial view of the site, and the field of view of the Phenocam. Here the new Figure 1:

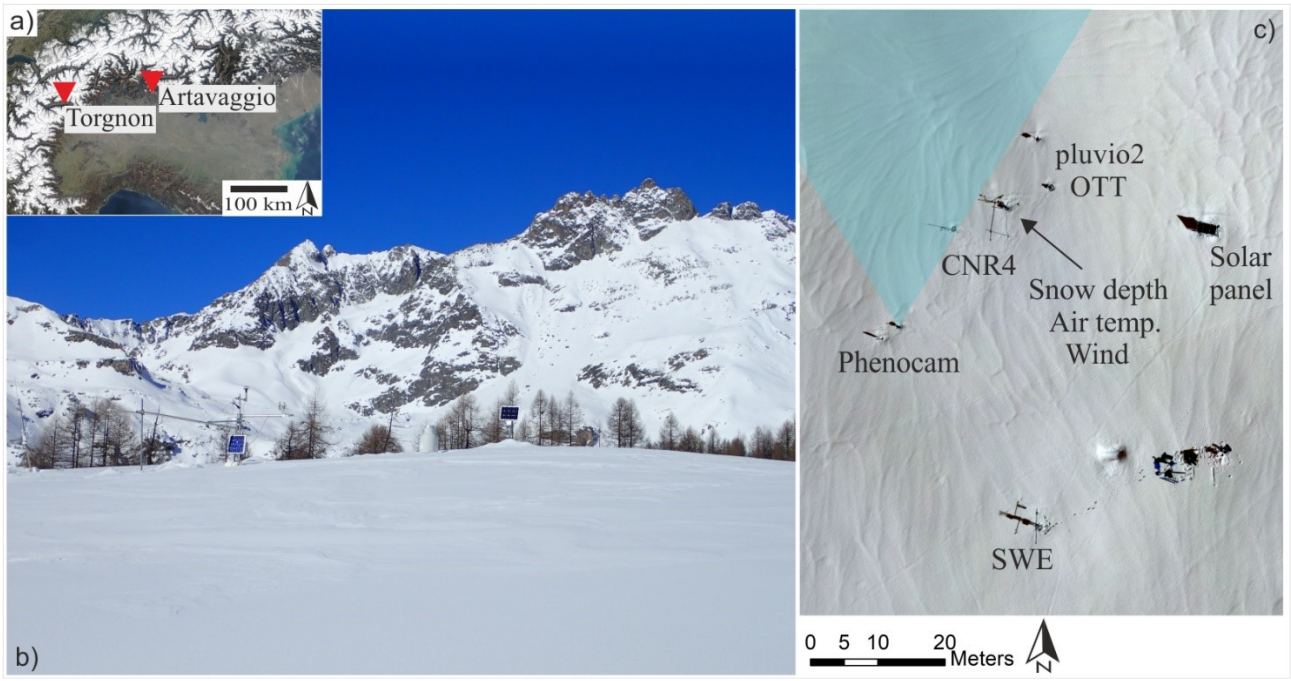

Figure 1: a) location of the experimental site of Torgnon (Aosta), and Artavaggio plains (Lecco) in the European Alps. b) a picture of the experimental site of Torgnon (2160 m a.s.l.). c) aerial view of the site in Torgnon with the location of different instruments installed. The field of view of the Phenocam is also represented with a blue shaded area.

P4 L18-P5 L2 This paragraph should be moved to the introduction.

We shortened the paragraph and we used it to introduce the use of digital images in the methodology section. The sentence now reads:

"*2.2 Digital images analysis*

In recent years digital images analysis was applied to monitor vegetation phenology (Julitta et al., 2014; Migliavacca et al., 2011; Richardson et al., 2007), landslides, glaciers (Jung et al., 2010) and snow (Corripio, 2010; Dumont et al., 2011; Hinkler et al., 2010; Parajka et al., 2012). Regarding the two latter, using digital cameras researchers successfully retrieved snow albedo and snow cover in alpine areas."

P.6 L5 Which instrument was used to measure diffuse shortwave radiation?

Diffuse radiation was measured with a BF3 sensor (Delta-T Devices Ltd, Cambridge, UK). We added this information in the text.

P6 L5 These instruments should be listed in site description.

We added this information in the site description section. Now it reads:

"Solid and liquid precipitations were measured with a pluvio2 OTT instrument."

We also added:

"Wind speed and direction were measured with a CSAT3 three-dimensional sonic anemometer (Campbell Scientific, Inc.)"

P.6 L16 Can you clarify how exactly samples were collected. Was it one sample at depth 20 or one sample for the 0-20 cm layer? What was the total depth of the snow pit? Is there a description/photograph to compare the snow pit with the results of the modelling and dust layers modelling?

Samples used in this paper were collected from six different snow pits placed at few meters from the AWS station. For each snow pit, we collected a surface samples at 0 cm, and three samples at depths equal to 20, 40, and 60 cm from the surface. The concentrations of dust among different snow pits were very similar, so we presented just an example in Fig. 5. Unfortunately, we don't have high quality photographs of the snow pits for this comparison.

Now the sentence reads:

"On April 6th 2016, a field campaign was organized to collect snow samples at the experimental site of Torgnon. Six snow pits were dug in different locations placed at few meters from the AWS station. For each snow pit, we collected a surface samples at 0 cm, and three samples at depths equal to 20, 40, and 60 cm from the surface".

P.6 L.25 and further. This information is a bit confusing as the plural used for samples of dust which were used to characterize the dust events and elemental input. But at the end of the section we see that there was only one dust (presumably originated in Sahara) sample analysed. This is important issue as it's quite difficult to justify how representative results of analysis of one sample are for other dust events. It should be clearly stated how many samples were analysed for this particular study. Representativeness of this site and samples together with possible dust pathways etc. should be discussed within the results and discussion section rather than here.

For the neutron activation analysis, we used two samples: one representative for clean snow and one for the dust event of February 2014 (see line 37 in page 7). We acknowledge that only one snow sample containing dust is not enough to provide a complete overview on the composition of Saharan dust in snow in the Alps, but our analysis may pave the way for a more exhaustive characterization of dust composition in the future.

We moved this paragraph to Section 3.3, we also added this sentence (pg 17 ln 31):

"For this reason, the dataset presented in this study can be considered representative for the main composition of long-range dust deposition on snow in the Alps."

And then in pg 18 ln 11:

"We acknowledge that only one snow sample containing dust is not enough to provide a complete overview on the composition of Saharan dust in snow in the Alps, but our analysis may pave the way for a more exhaustive characterization of dust composition in the future."

P.7 L9. "only" is subjective, for some sites this would be considered quite substantial sample.

We removed "only" from the sentence.

P.7 L18. So far there was no mention about this modelling. Probably it should be mentioned somehow in introduction.

In the Introduction, we now added:

"The timing and intensity of Saharan dust depositions were simulated using two independent models (ALADIN-Climate and NMMB/BSC-Dust)."

P.7 L27 3.1 Modelled dust depositions?

We changed the title of this paragraph to: "Modelled dust deposition events"

P.7 L36-38 repetition.

We removed these two sentences.

P.8 L2 this strong dust event? (singular?)

We replaced "these" with "this".

P.9 L4 than P9. L6 Can you please explain why Crocus model shows a 100 mm SWE in Dec 2013 while this was not observed. Solid precipitation is one of the input parameters isn't it?

The GMON sensor was installed in 2013. During the first weeks, we had some problems with the power supply, so data were not recorded. In the caption of Figure 3, we added:

"SWE data are missing in December 2013 because of problems with the power supply."

P10. L.14 This is just one possible explanation though quite doubtful as particles are still quite large to be washed out that simple. It would've been great to see the description (photograph) of the snow pit in 2016 and to see how it corresponds with modelled structure. Additional samples collected from these dust layers separately could've helped. Another interesting question is how the local mineral particles (rocks, soil, vegetation. . .) affect snow melting. The mass can be substantial in the snow pack, but of course it will not be modelled by dust deposition model.

We agree with this comment. Unfortunately, we don't have high quality photographs of the snow pit. During the last two years, we've been visiting regularly the site and collecting multiple samples of snow containing dust.

In the manuscript, we added:

"This deeper layer can be probably due to the eventual scavenging of small dust particles by meltwater, or to other undetected processes".

The effect of larger particles on the snow melting is discussed in the answer to the following comment.

P.10 L18 The tail in distribution most likely is due to input from local particles. Looking at the photograph there are many rocks and vegetation around the site and Coulter Counter analysis do not distinguish between particles of different nature. This is quite an important issue. If the total mass concentration of mineral particles considered, than highest input would be from the small number of large particles.

We cannot exclude that large particles of local origin can be deposited on snow (we acknowledged it in pg 12 ln 16). Recently, we installed different deposimeters for evaluating the input of local and remote particles to the snowpack. In the future, Crocus model could be also modified to account for larger particles of local origin. In the text (pg 12 ln 16), we added:

"A contribution of large particles of local origin cannot be excluded, and it may have a strong influence on snow melting. At the moment, we don't have enough data to decouple the effect of large and small particles on snow albedo"

P.12 L4 Can you please clarify a bit more how exactly BC data were used. Was it an input to Crocus model? How large was the impact compare to dust. Isn't it the largest source of uncertainty? Can the BC signal be separated from the natural dust? Later in the text you mostly discuss the influence of the impurities without specifying.

Black carbon (soot) fluxes was one of the inputs of Crocus model (see Section 2.2). For decoupling the effect of dust and black carbon, Crocus can be run taking in account dust and black carbon separately (see Tuzet et al. 2017). In our simulations, both impurity fluxes are considered. Since we don't have direct measurements of black carbon in snow at our experimental site, we cannot exclude a possible influence on snowmelt. In the new version of the manuscript, we added:

"The role of black carbon in Alpine snow still represents a great uncertainty in snow modelling and climate prediction in the Alps. While the role of industrial black carbon on post-industrial glacier retreat has been debated (Painter et al. 2013; Sigl et al. 2018), its role on seasonal snow melting has not been studied in the European Alps."

References:

Painter, T. H., Flanner, M. G., Kaser, G., Marzeion, B., VanCuren, R. A., & Abdalati, W. (2013). End of the Little Ice Age in the Alps forced by industrial black carbon. Proceedings of the National Academy of Sciences of the United States of America, 110(38), 15216–21. https://doi.org/10.1073/pnas.1302570110

Sigl, M., Abram, N. J., Gabrieli, J., Jenk, T. M., Osmont, D., and Schwikowski, M.: 19th century glacier retreat in the Alps preceded the emergence of industrial black carbon deposition on high-alpine glaciers, The Cryosphere, 12, 3311-3331, https://doi.org/10.5194/tc-12-3311-2018, 2018.

P.12 L17 new paragraph? or maybe it's better to introduce a separate section on SDI

We added a new paragraph on SDI data and simulation.

P.14 L23 I doubt that it's a good argument to compare advancement in snow melt to distances from the deserts. You can either compare average (long-term) deposition rates or differences in snow duration reduction with similar dust concentrations.

We modified the sentence according to your comment. Now it reads:

"Despite the different deposition rates in the Alps, the advancement of the snowmelt owing to dust is comparable with published results regarding the Western US. This is true at least for one season (2015/2016), characterized by a major Saharan dust deposition."

P.14 L26 Is it possible to compare bulk concentrations (e.g. CC results) with the deposition modelling results?

A numerical comparison with Crocus prediction is provided in pg 13 ln 7. As showed in Tuzet et al. 2017, the concentration of impurities within the snowpack can be directly compared with Crocus predictions. In that case, dust concentrations were underestimated, while BC concentrations were overestimated. Our results show that observed dust concentrations were reasonably comparable with simulated ones. Also considering the large spatial mismatch between the point measurements and the ALADIN fluxes predictions.

P.14 L32-35 This is again partly repetition from the introduction. As well as in the next paragraph. Trends are not discussed in this paper at all so it can go to introduction.

We agree with this comment. But we prefer to lose this (and the following) sentence, since the introduction is already lengthy, we prefer not to add further text to it. We also removed the sentence in line 1-2 (pg 15), since it is repeated in the following paragraph.

P.15 L.8-10 This is a bit exaggerated. If the average over 82 years is late May than I believe snow disappeared in early May a number of times. Or maybe not? How many times exactly? So how rare such snow duration actually is? is it really extremely short? The next part of the paragraph is again an introduction-like and can be possibly moved up there too. The importance of the snow duration shifts is explained there. In results section it's better to discuss the exact results.

We used the expression "extremely short" because the first important snowfalls occurred in January for the 2015/2016 season. Considering this coupled with an earlier snowmelt due to dust depositions, this season is characterized by a snow cover duration of 4 months, over an average of 7 months. We acknowledge that the term is a little bit strong, so we replaced "extremely" with "very".

P 16 L.20-27 repetition of the introduction

We prefer to keep these introductory sentences in this chapter. They are important for putting into context the geochemical characterization of dust.

P.20 L. 9-11 I'd suggest to rephrase or remove this. This large topic needs much more regular analysis suitable methodology etc. It just sounds a little bit speculative.

We removed this sentence according to your comment.

Best regards,

Biagio Di Mauro and co-authors