

## ***Interactive comment on “Saharan dust events in the European Alps: role on snowmelt and geochemical characterization” by Biagio Di Mauro et al.***

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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

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comprehensive description of the research together with substantial literature review, results and discussion. I recommend this manuscript for publication after a minor revision.

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.

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. P.2. 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. 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. 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 fun-

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damental is repeated several times later. 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.” P.3 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. P.4 Fig. 1 The map should be enlarged and zoomed, font size adjusted. Preferably the geochemistry sample site should be included as well. P.4 L18-P.5 L2 This paragraph should be moved to the introduction. P.6 L5 Which instrument was used to measure diffuse shortwave radiation? P.6 L5 These instruments should be listed in site description. 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? 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. P.7 L9. “only” is subjective, for some sites this would be considered quite substantial sample. P.7 L18. So far there was no mention about this modelling. Probably it should be mentioned somehow in introduction. P.7 L27 3.1 Modelled dust depositions? P.7 L36-38 repetition. P.8 L2 this strong dust event? (singular?) P.9 L4 than P.9. 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? P.10. 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

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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. 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. 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. P.12 L17 new paragraph? or maybe it's better to introduce a separate section on SDI 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. P.14 L26 Is it possible to compare bulk concentrations (e.g. CC results) with the deposition modelling results? 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. 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. P 16 L.20-27 repetition of the introduction 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.

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