Reply to reviewers' comments on "History of desert dust deposition recorded in the Elbrus ice core" We would like to thank both reviewers for their comments that help us to improve and clarify the manuscript.

Please note that this is a companion paper of another manuscript submitted to ACP https://www.atmos-chem-phys-discuss.net/acp-2019-402/ Preunkert et al., "The Elbrus (Caucasus, Russia) ice core glaciochemistry to reconstruct anthropogenic emissions in central Europe: The case of sulfate."

Some additional text was added to this manuscript as suggested by the reviewer of the Preunkert et al. paper (see section 4.3). Figure 5 was changed and an additional Figure 7 was added following the recommendation of one of the reviewers as well.

Reply to

Anonymous Referee #1

Received and published: 5 June 2019

The manuscript by Kutuzov et al. presents a calcium-dust proxy record from an ice core drilled on Mt. Elbrus, Caucasus, spanning the time period 1774-2013 CE. The discussion on the dust proxy include separating the background signal and the main dust events, evaluating the frequency and amplitude of dust events, establishing a relation with the potential dust sources by means of analysis of atmospheric circulation patterns and climate indices. I found the dataset very interesting per se, which warrants publication in ACP. The discussion and interpretation of the record is quite detailed and includes very interesting findings, following an approach established by the same group of authors. However, I also found that one relevant issue, related to the almost 10-fold increase in snow accumulation with potential implications on the interpretation of the proxy record, is not discussed. Therefore I recommend a major review of the manuscript.

General comments

After reading the manuscript, I am not sure whether the trends mostly reflect changes in deposition/accumulation processes rather than changes in dust emissions. This aspect is not discussed in the manuscript, while I believe it is central for the interpretation of the record. I elaborate on this consideration in the lines below.

In Figure 1a one can be appreciate how the summer half-year thickness, expressed in meters of water equivalent, increases significantly since the beginning of the record. This implies increased snow accumulation, in addition to the expected ice compaction with depth. In fact the "companion" discussion paper by Preunkert et al., reports a "decrease of the net annual snow accumulation from 1.5 mwe (0.8 mwe in summer and 0.7 mwe in winter) near the surface to 0.18 mwe (0.15 mwe in summer and 0.03 mwe in winter) at 157 m depth". Therefore we see an almost 10-fold increase in snow accumulation rates along the core. The authors discussed the related potential issues in determining the the ability to detect the frequency of dust events; in order to overcome this issue, they adopted a strategy with finer sampling in the bottom sections of the core. While this precaution is an effective measure to that aim, it does not respond to the issue of whether the increased accumulation rates reflect increased precipitation and wet scavenging, in other words a larger or more frequent sampling of the atmospheric dust loading during precipitation events. As a result, it cannot be safely concluded which effect primarily (or maybe both) determines the observed trends in the dust proxy. This kind of reasoning is partly grounded in the long-standing debate on whether for instance dust concentrations or deposition fluxes are a better proxy for atmospheric dust / dust variations (e.g. Fischer et al., 2007; Mahowald et al., 2011).

I recommend that these issues are throughly discussed in the manuscript, and the interpretations and conclusions weighted accordingly.

Taken into account. We thank reviewer for these general comments. One important issue was raised by the reviewer. The significant change in the accumulation rate indeed may influence the results of ice cores interpretation. However, in this manuscript we do not show or discuss any accumulation changes. The presented figures and data show only the thickness of annual layers. In order obtain an accumulation rate the layer thickness must be corrected for the compression which occurred since it was deposited (e.g. Paterson and Waddington, 1984). This effect at Elbrus can be clearly seen at Figure 9 in (Mikhalenko et al., 2015) which shows the annual layer thickness and the Nye model fit. The monotonic decrease of annual layer thickness is an effect of layer thinning. A separate paper dedicated to the accumulation rate change in Elbrus should be submitted soon. The accumulation is calculated using the dating and available reference horizons together with depth age modelling. When accounted for the ice layer thinning the accumulation variations are within 20-30% and there is no linear trend in accumulation change over the whole period. Therefore we do not expect any significant influence of the accumulation change on Ca2+ concentrations giving the reasonable sampling resolution. The observed trends in Ca2+ concentration cannot be explained by the changes in accumulation.

We can see that this misunderstanding was due to inconsistent wording in figure captions and in the text of two companion manuscripts. We added paragraph to explain this issue.

"It show be noted that Fig. 3 show the thickness of layers and does not represent the linear change in accumulation rate. In order obtain an accumulation rate the layer thickness must be corrected for the compression which occurred since it was deposited (e.g. Paterson and Waddington, 1984) which is out of the scope of this paper. "

Specific comments

p. 2 / lines 3-4: Please explain how

Taken into account. Text revised. "The discrepancies between models are partly explained by very limited observations of dust variability over the past and therefore limited possibilities to evaluate the model's reproducibility of the dust cycle."

2/4-5: please provide some references

Done. References added (e.g. Gautam et al., 2009; Chudnovsky et al., 2017; Li and Sokolik, 2018).

2/8: it would seem more precise to say that many of the archives reported in the cited manuscript show a doubling of the respective dust signals

Taken into account. Text revised.

2/9-13: Given the level of detail reported here with reference to the cited paper, it may be worth reporting other studies as well (e.g. Ginoux et al., 2012; older papers assessing the issue at the global level)

Taken into account. Text revised, reference added.

2/25: Two references are listed as Kutuzov et al. 2015 a,b. Please delete the nonrelevant one. In addition, remove from the reference list the discussion paper (Kutuzov et al., 2013).

Done

3/9: Make sure that the special character is properly displayed. In addition, in the legends of Figures 1 and 2, a resolution of 0.5 x 0.5 degree is reported. Which one is correct?

Both the resolutions are correct. The first one is related to NCEP/NCAR Reanalysis, the second one simply show a resolution with which the Figures 1 and 2 were produced.

3/12: Rather than aerosols, the HYSPLIT analysis reported here shows that "Elbrus glaciers receive AIR MASSES from sources . . ."

Text revised

3/14-21: Please clarify whether the density plots in Figure 2 are based solely on the the back-trajectories passing close to the ground (and what about Figure 1?). In addition, please explain how did you define the well-mixed boundary layer.

Text revised. "Density plots were calculated only for 10 day backward trajectories which descended below mixed layer depth. The depth is calculated by HYSPLIT_4 (using NCEP/NCAR Reanalysis data) for each point of backward trajectory as the height of the first exceeding of potential air temperature over surface air temperature by 2 K in the point (Draxler & Hess, 1998)."

3/25: Please report the geographical coordinates

Done

3/26: ranged from . . . AT 10 m depth . . .

Done

3/30: Could you report in a few words the main aspects of that methodology?

Done. "Cores were subsampled and decontaminated at -15°C using the pre-cleaned electric plane tool methodology described in (Preunkert & Legrand, 2013). In brief, in a first step, ice samples were cut with a band saw. After that, all surfaces of the cut samples were cleaned under a clean air bench *by using a pre-cleaned electric plane tool over which the ice was slid. To control the decontamination efficiency process blank ice samples, consisting of ultrapure frozen MilliQ water were preceded regularly."*

3/30-31: Please specify whether the sampling is continuous along the core

Done. Text revised.

4/8-9: Define what is meant by decontamination blank

Done. Text revised. "To control the decontamination efficiency process blank ice samples, consisting of ultrapure frozen MilliQ water were preceded regularly."

4/18-19: Please rephrase

Done. Text slightly revised

5/7-8: Please report briefly the methodology of the cited paper, i.e. how one can identify dust events on the basis of $Ca2+$ and acidity records.

The section with this part of the text was removed. This criteria is explained further in the manuscript in section 4.1.

5/20-22: It would be interesting to estimate/report the uncertainty arising from this assumption, and propagate it to the dust proxy.

Rejected. The sea salt $Ca+2$ fraction of the summer $Ca2+$ concentration is 1±0.7%. We show that we can neglect its influence.

5/22-25: Can you provide an estimate of how much this uncertainty could amount to?

Rejected. We really cannot say more than what is stated. "That percentage is clearly an upper limit since, in precipitation deposited at continental free tropospheric sites (e.g. Legrand, 2002), $Na+$ is not only related to sea-salt due to the presence of leachable sodium in alumino-silicate particles but also $Na+$ from halide evaporates present in the deserts".

This upper limit (1 and 1.5%) is low enough to completely neglect the sea-salt contribution to the calcium level.

5/31 - 6/3: It is not clear what you mean by "disturb" in both sentences. Please rephrase.

Taken into account. Text revised.

6/8: What do yo mean by "warm periods"? Warm years/decades? Warm seasons?

Warm seasons. Text revised.

References

Fischer, H., Siggaard-Andersen, M. L., Ruth, U., Rothlisberger, R., and Wolff, E. W.: Glacial/Interglacial changes in mineral dust and sea-salt records in polar ice cores: sources, transport, and deposition, Rev. Geophys., 45, RG1002, doi:10.1029/2005RG000192, 2007.

Ginoux, P., Prospero, J., Gill, T., Hsu, N., and Zhao, M.: Global-scale attribution of anthropogenic and natural dust sources and their emission rates based on MODIS deep blue aerosol products, Rev. Geophys., 50, RG3005, doi:10.1029/2012RG000388, 2012.

Mahowald, N., Albani, S., Engelstaedter, S., Winckler, G., and Goman, M.: Model insight into glacial-interglacial paleodust records, Quat. Sci. Rev., 30, 832–854, 2011

References

Paterson, W. S. B. and Waddington, E. D.: Past precipitation rates derived from ice core measurements: methods and data analysis, Rev. Geophys., 22, 123–130, 1984

Mikhalenko, V., Sokratov, S., Kutuzov, S., Ginot, P., Legrand, M., Preunkert, S., Lavrentiev, I., Kozachek, A., Ekaykin, A., Faïn, X., Lim, S., Schotterer, U., Lipenkov, V., and Toropov, P.: Investigation of a deep ice core from the Elbrus western plateau, the Caucasus, Russia, The Cryosphere, 9, 2253-2270, https://doi.org/10.5194/tc-9-2253-2015, 2015.