Atmospheric
Chemistry
and Physics
Discussions

Interactive comment on "The Elbrus (Caucasus, Russia) ice core glaciochemistry to reconstruct anthropogenic emissions in central Europe: The case of sulfate" *by* Susanne Preunkert et al.

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This manuscript presents an ice core record of sulfate from a glacier on Mount Elbrus covering the time period 1774-2009. Generally, it is well written and structured and mostly scientifically sound (see comments below). The high-quality data set fills a gap, since it is the first sulfate record from South-Eastern Europe. Regional data on pre-industrial to industrial concentration changes of major aerosol components are essential to constrain emission estimates used in modelling the aerosol effect on climate. I therefore expect that this record will have an impact. The manuscript definitely deserves publication, after taking into account the comments and suggestions listed in

the following.

Specific comments:

There is very little information about the ice core itself. The coordinates are just given in the abstract and the length in the introduction. I suggest adding a short paragraph about the Elbrus ice core, including some additional information, e.g. name of the glacier, ice thickness, ice temperature, and net accumulation rate. In the abstract it is called a deep ice core, but that is relative. More important is if it reached bedrock or not. In addition, it would also be good to summarize briefly previous work published on this core.

Use the term South-Eastern Europe instead of Central Europe for the source area of emissions detected in the Elbrus record.

You observe a stronger thinning with depth of the winter layers compared to the summer layer. This is interesting. Is it due to a change of precipitation seasonality or is it an artefact caused by diffusion of chemical tracers or even different flow behaviour of summer and winter layers?

Dating of the core: This is so central for the interpretation and it was extended compared to the previous publication (Mikhalenko et al., 2015). I therefore suggest including a depth-age figure with the 14C dating points to give an idea about the thinning (can this be fitted with a glaciological flow model?). Also the volcanic horizons used to anchor the counted layers should be shown. You evoke basal melting to explain why the deepest ice is so young. Does this mean, the glacier is not frozen to bedrock at the drilling site? This has implications on the thinning. Please clarify.

I am not convinced by the equidistant binning of the summer and winter layers to obtain monthly values. This requires the absence of seasonality in precipitation and snow preservation. Precipitation data from nearest meteorological stations show strong seasonality (Kozachek et al., 2017). Since the monthly data are not really discussed,

accept for showing the seasonality of chemical tracers in Fig. 6, I suggest deleting this part and the figure.

Identification of annual layers and attribution of summer and winter layers: You use two criteria for that (ammonium and succinate). To which of the two do you give priority when the two signals do not agree? How does the attribution of summer and winter layers presented in this manuscript agree with the one based on the stable isotope record of the same core (Kozachek et al., 2017)?

14C-dating: Was the AMS equipped with a gas ion source? You used a rather old version of Oxcal. I suggest using an updated version.

Ion balance: Use the same unit (either ppb or uEq/L) in the text and in Figure 4.

Attribution of dust sources: This part of the manuscript is not convincing to me. What is the argument to relate high Ca concentrations to Saharan dust and low Ca concentrations to sources in the Middle East? The plots in Figure 5 show a large scatter and low correlation coefficients, so I wonder if the ion ratios you discuss are significantly different. For the ions with strong anthropogenic influence this correlation analysis is anyway not meaningful without splitting the data set in the pre-industrial and industrial periods. To me this part of the manuscript is weak, distracts from the main message, and could be omitted. Important is to estimate the amount of sulfate originating from dust and correct for that when discussing anthropogenic sulfate.

Attribution of sulfate related to mineral dust: Instead of arbitrarily introducing a Ca level to identify dust events, I propose to look at the pre-industrial Ca to sulfate correlation. If both are highly correlated, you can use this ratio to correct for mineral dust sulfate in the industrial period. I recommend adding a map with the Elbrus site, outlining the dust and SO2 emission source areas.

Table 5 is mentioned in the text, but does not exist.

Discussion of outliers: This is hard to follow without seeing the raw data (which should

be shown anyway). Can some of the outliers be explained by volcanic events? It is strange that you don't see a signal of the largest eruption in the last centuries (Tambora, 1815) and the largest eruption in the Northern Hemisphere in the last centuries (Laki, 1783).

Comparison with emission estimates: You stress in the manuscript the importance to distinguish between summer and winter sulfate values and trends (to me the trends look similar). And then you compare this with emission estimates, which are annual values (I guess). This is inconsistent. You need to include the total anthropogenic sulfate record, which would also be very valuable for comparison with data sets from other ice cores, which are not resolved in summer and winter values. In addition, you give the impression that SO2 emissions in winter are much lower than in summer. The opposite is the case. The major factor producing the difference in summer and winter values at high-alpine sites is the reduced vertical atmospheric transport in winter (and not the variation in source area). You need to explain this in the manuscript.

Considering the SO2 emission source areas you identified it is strange that you just compare the Elbrus record with the CDD record from the Alps. I strongly recommend to include the sulphate record from Eastern Europe (from Belukha ice core, Eichler et al., ES&T 2012).

Figure 1. I don't see the point of showing the mean summer and winter sample length. This should not be so different from the sample resolution.

Technical corrections

Title: seems too long and a bit cumbersome. Suggestion: Reconstruction of anthropogenic sulfate trends from Elbrus ice core, Caucasus.

Abstract L. 18: After having examined... Rephrase and give the results: dust contribution to sulfate concentrations was identified and subtracted to focus on anthropogenic sulphate (not sulfur)

P2L4: Replace Andreae et al., 2015 with a newer estimate e.g. from IPCC.

P2L8: "impact" instead of "disturb"

P2L13-15: The Altai and Kamchatka are not part of Europe.

P2L16-19: ice cores have been investigated . . .to examine

P3L12: Give more details how ice cores were decontaminated (by removing xx cm from the outside of the core. . .)

P3L17: loss

P3L29: Give details, which fluid was used.

P4L7: For the ammonium seasonality earlier work should be cited (Maupetit et al., Atmos. Environ., 1995; Eichler et al., JGlac., 2000)

P6L14-15: Replace "disturb the chemistry" by changes the chemical composition

Table 1: Include 14C lab sample reference number.