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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Black carbon variability since preindustrial times in Eastern part of Europe reconstructed from Mt Elbrus, Caucasus ice cores" *by* Saehee Lim et al.

Anonymous Referee #1

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This paper presents the first high-resolution record of refractory BC (rBC) from Eastern Europe based on an ice core retrieved from Mt. Elbrus and covering the time period 1825-2013. The trend in rBC concentration is discussed with respect to atmospheric BC loads simulated with the FLEXPART model using BC emission inventories. The main conclusion is that the record mainly reflects BC emissions in the Eastern part of Europe. Discrepancies between the ice core record and the simulated BC load suggest an underestimation of anthropogenic and biomass burning BC emissions in Eastern Europe. The Mt. Elbrus rBC record has the highest temporal resolution of the BC or EC records, which have been published from European ice cores (Col du Dome, Colle Gnifetti, Fiescherhorn, all in the Western part of Europe). The authors made a

real effort to attribute potential source areas, instead of just presenting the record. This is very valuable and the study nicely demonstrates the importance of such records to constrain highly uncertain BC emission estimates. The paper is generally clear, well written, thorough, and suitable for ACP, and should be published.

However, I have some comments that the authors might want to consider for the sake of clarity in the paper.

I know it is kind of tradition of the "Grenoble" group to classify the ice core data into summer and winter values and there might be circumstances were this is justified. However, this is always difficult, since the record does not contain clear time markers for the seasons and therefore assumptions have to be made. In this case, the 25th and 75th percentiles of thickness of an annual layer were arbitrarily chosen, assuming equal distribution of precipitation (and preservation on the glacier) throughout the year. This is conducted without explaining the hypothesis behind. The obtained summer and winter rBC records do show similar trends and the slight differences around 1900 are not discussed at all. As expected the JJA and DJF scenarios for the atmospheric BC load are different, with much higher contributions from North America (NAM) to DJF. However, the authors do not question their summer and winter classification in the ice core because of this finding, but do instead explain the difference with an overestimation of the NAM footprint density by the simulation. To my opinion, the classification into JJA and DJF needs better justification, for example by showing that the annual ice core rBC concentrations and annual atmospheric BC loads agree less. What is puzzling is that in the other manuscript about this ice core (Kozachek et al., CPD 2016), classification into seasons is conducted by introducing the mean delta18O value as threshold. If at all, the procedure should be the same. Please also reconcile the details about the core (20.4 m here, 20.5 m in Kozachek et al.; dating uncertainty few years here, +- 1 year in Kozachek et al.) and include a reference to that manuscript.

The rBC size distribution data are very valuable since they support other findings that the rBC particle sizes in snow and ice are larger than in the atmosphere. However, the difference in MMD between summer and winter (Fig. 5) is not so obvious to me. The main discrepancy is for the few data points before 1960 where the data coverage is anyway poor. Have you tested if the MMDs for the period after 1960 are significantly different, considering the strong variability of the data?

Minor comments:

The English is generally good, but still some editing is required.

Line 63: rephrase: that is reconstructed in the downstream of Europe.

Line 95: Please specify “upper section” (move this up from line 116).

Line 117: Give max and min numbers of data points per year.

Lines 211-214: The fact that biomass burning emissions frequently occur in summer should be reflected in the emission estimates. I do not understand the argument for not considering the biomass emissions in DJF.

Lines 232-236: Include Jungfraujoch (e.g. Bukowiecki et al., Aerosol and Air Quality Research, 16: 764–788, 2016).

Line 293: Mikhalenko et al. (2015) do not mention aerosol removal processes. Please clarify that you assume that wet deposition dominates, since there is often and regular precipitation throughout the year.

Line 355: Matthias, 2004: Is this Matthias and Bösenberg, 2002?

Figure 2 would benefit from a better quality map. Please indicate location of ELB and explain abbreviations in the figure (NAM etc).

Fig. S1: The overlap between the 2009 and 2013 cores is not convincing. Could you support this with other ice core parameters (e.g. stable isotopes)?

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