Answer to Anonymous Referee #2

We would like at first thank the reviewer for his comments. In the following our answers are made in italic.

This publication reads like a solid piece of work, well written, and logically structured. The caveat is that I am not an ice core specialist- and if there are methodological issues in this part, I have probably not spotted that. From a general atmospheric chemistry perspective, however, the manuscript and story make a lot of sense. I can therefore recommend this manuscript for publication in ACP, with some minor suggestions for improvements below.
Minor suggestions: General: As this manuscript is submitted to a more general Atmospheric Chemistry journal, I would recommend to spell out/explain specialized abbreviations used in this manuscript. E.g. I didn't know the meaning of Yr cal BP; also BP, CE may not be known to all readers. Possibly a table ?
We agree and we have now specified in the "Basal ice Dating" section that: "As seen in Table 1, the mean age of the ELB-178-03 sample (1530 yr cal BP, i.e. 1530 years before 1950)……;"
Also in the first sentence of the abstract: "This study reports on the glaciochemistry of a deep ice core (182 m long) drilled in 2009 at Mount Elbrus in the Caucasus, Russia. Radiocarbon dating of the particulate organic carbon fraction in the ice suggests that the basal ice dates to 280 ± 400 yr CE (Common Era)."

General: it would be useful if in addition to concentrations also the deposition fluxes would be presented, which is the more obvious quantity for comparison with models.
We agree that the knowledge of deposition fluxes would be useful to compare with model simulations. Unfortunately, that is not an easy task. Indeed, what we estimate on the basis of the seasonal dissection (or annual counting) is the annual ice thickness. This annual ice thickness systematically decreases with depth due to ice flow. Ideally, using the annual ice thickness versus depth and a good ice flow model (that does not exist) it would be possible to derive the original accumulation rate. But even with that, you have to consider a possible strong erosion of snow by wind after deposition.

P1 l. 19 focus on dust-free sulfur pollution. (to clarify).
OK done here and throughout the text.

P1 l. 26 I would say also the much later onset is an important piece of information, which confirms knowledge on industrialization.
As now discussed when comparing the three sites (see also our answer to your comment on comparison with other ice cores) in section 5, we also emphasized the difference in the onset: "Long-term dust-free sulfate trends observed in the ELB ice cores are compared with those previously obtained in Alpine and Altai (Siberia) ice, the most important differences consist in a much earlier onset and a more pronounced decrease of the sulfur pollution over the three last decades in western Europe than south-eastern Europe and Siberia."

P2 l 2 In general short lived climate forcers, with one of the most important components being aerosol.
OK done

P 2 l 6 this is somewhat naïve statement, as models will usually calculate the concentrations and verify them with observations. Only from the satellite era onward, aerosol is assimilated but not in 'climate' models.
You are right and we now corrected the sentence as follows: "However, uncertainties still exist in quantifying the climatic impact of aerosols. The spatial distribution of aerosols is very heterogeneous and requires therefore numerous observations to make these parameters useful as inputs and constraints for transport and chemistry models."

P 2 l 6 A number of other continental ice cores are mentioned, but only CDD is explored later in the text. It is not entirely clear, why a comparison with the other icecores is not included in the manuscript.
Yes, in the draft the ELB record was only compared to the CDD (alpine) record. In the revised version we also add a comparison with the one extracted from the Belukha glacier in the Siberian Altai. The abstract was changed consistently.

2 l 26 Another argument is that there is a quite strong seasonal dependency of the oxidation chemistry of SO2, which has probably been oxidant limited in the emission era.
Yes you are right: three factors influence the seasonality: the upward transport intensity (maximum in summer) is the main factor, the second and third are the seasonality in the emission with a slight maximum in winter counteracted by the lowering of the conversion SO_2/SO_4.

P3 l. 15 explain meter water equivalent, and if this information is available how do these precipitation rates compare to a larger footprint around Mt. Elbrus?
Annual firn and ice (density varying from 0.3 to 0.9 g cm^{-3}) thickness are commonly converted into a water column. As answered above, the link between ice annual thickness and local precipitation rate is anyway quite complex. For your information, Kozachek et al. (Climate of the Past, 13, 1473-489, 2017) reported a mean annual ice thickness close to 1.3 mwe in the recent layers (see also the new Figure 2 in the revised version) against a precipitation rate of 1.7 m of water at the site of Klukhorskiy Pereval located at 2037 m elevation.

P 3 l 28-32. Later in the text outliers are removed, are these outliers related to these known problems? If not what could be the cause of such outliers ?
Generally speaking single values considered as outliers are very likely due to contamination during the subsampling or due to the poor quality of the ice. To better illustrate these outliers, we report them in Figure 7 together with all raw data.

P 4 l. 12 Again for non experts explain whether the decrease of NH4 with depth is a 'real' signal, or rather related to gradual degradation/oxidation with time.
No, as far as we know there is no evidence from numerous other ice core studies that the decrease of ammonium with depth is due to a degradation. More likely the increase of ammonium in the recent layer (as for nitrate) is related to atmospheric changes but a detail discussion of these trends is out of the scope of this paper that focuses on sulfate.

P 4 l. 34 I understand the chemical stratification is a preferred method compared to radio carbon dating, can you confirm because that is because of higher accuracy?
Yes, definitely when the annual counting is possible the accuracy is typically a few years for a period of 100 years. When not possible because of poor record in ice of the seasonal atmospheric contrast, and in the absence of absolute horizons such as volcanic events, the

unique possibility is the radiocarbon dating (which is far less accurate). In the revised version we report a dating figure (Figure 5) that illustrates fairly well this point.

P 6 l. 27 the 616 and 67 numbers are the samples influenced by high dust? Sentence is ambiguous. *OK we reworded this sentence: "In this way, 616 (on a total of 2524) summer and 67 (on a total of 1150) winter samples were considered as containing large amount of dust."*

P 7 l. 4 at the best=>at the most :
OK Done

P 7 l. 14 Dust may contain a quite large fraction of CaSO4, which is quite insoluble under alkaline conditions, may be dissolve when more acidic. If I understood well this would not be picked up in the analysis, and can not influence the trend estimates? Please confirm.
Fraction of gypsum (primarily emitted) may indeed be quite insoluble. However the other fraction coming from neutralization of calcium carbonate during transport (sulphuric acid or SO_2) would be more soluble. Also the change of acidity over time in this region is not so large than in the Alps since together with the increase of anthropogenic species (sulfate and nitrate) we have an increase of alkaline calcium (as discussed in the companion paper).

P 8 l15- you can mention here that the corrected values were rather constant as also shown in Figure 9. ???
We are not sure if we understand your question. This figure appears far later in the text.

p. 9 l 1 Please provide some plausible reasons for the outliers, or connect to the statements in the analysis section.
See our previous answer for outliers.

P 9 l. 27 It would be good to mention here which emission database was used.
Yes done: "Using data from Smith et al. (2011), available at http://sedac.ciesin.columbia.edu/data/set/haso2-anthro-sulfur-dioxide-emissions-1850-2005-v2-86, we report in Figure 12 emissions of SO2 from countries located nearby the ELB site: Georgia, Azerbaijan, Syria, Irak, Turkey, Russian, Iran or located further north (Ukraine) and west (Bulgaria)." And we also add to the reference list :" "Smith, SJ, J van Aardenne, Z Klimont, RJ Andres, A Volke, and S Delgado Arias. (2011). Anthropogenic Sulfur Dioxide Emissions: 1850–2005, Atmospheric Chemistry and Physics, 11:1101–1116."