

Dear Dr. Legrand,

My apologize for such a long time for the open discussions of your manuscript. Your manuscript did undergo an unusual experience, which is certainly unpleasant for you as well as for us. Two referees had accepted our invitation to review your manuscript in April. But both of them failed to submit their reports in early July when the open discussions should be closed normally for your manuscript. It took a few weeks more for me to contact them and to nominate other potential referees. Although I found another two referees agreeing to review your manuscript, unfortunately the report from one referee was missing again by the initial and extended deadlines. While promising to submit the review report soon, one referee said in his/her personal email to me that the paper is good but tough to get through and it's taken longer than expected. Actually, I share the same feeling with that referee when reading your manuscript.

Now we have gotten two review reports. While both referees admire import value of your data and significance of your work, one of them rates a low value of the quality of your manuscript especially in presentation. I agree with the referee (Referee 3) in that the manuscript needs to be focused more on the analysis of chemical processes. Actually, another referee (Referee 1) also suggested that additional chemical process be considered for chlorine depletion relative to sodium with respect to freshly emitted sea salt aerosols.

I noted that the sulfate aerosol issue has been intensively addressed by a companion paper of this manuscript (Legrand et al., 2017), which was also published in ACPD. Therefore, you may refer to that paper for the filtering of biogenic sulfate aerosols and, as suggested by the referee, focus more on the ionic chemistry involved in sulfate depletion relative to sodium with respect to the composition of sea water.

I also agree with the referee in that the discussions on implications for ice core studies (Sect. 3.3) should be skipped over if these discussion help little to explain your measurement and analysis results presented in the preceding sections.

In summary, I think that your manuscript needs substantial revisions based on the comments from the referees. You are welcome to submit the revised manuscript if you think that all the issues they raised can be well addressed. Your manuscript will be sent to the referees for further review, and the final decision can be made then.

If you have any questions, please do not hesitate to contact me. C2

Sincerely,

Jianzhong Ma

Reference: Legrand, M., Preunkert, S., Weller, R., Zipf, L., Elsässer, C., Merchel, S., Rugel, G., and Wagenbach, D.: Year-round record of bulk and size-segregated aerosol composition in central Antarctica (Concordia site) Part 2: Biogenic sulfur (sulfate and methanesulfonate) aerosol, *Atmos. Chem. Phys. Discuss.*, 2017, 1-39, 10.5194/acp-2017-305, 2017.

Answers to the Editor

Thank you very much for your comments. We have now three reviewers and two of them are very positive, both considering the paper as well written and important for atmospheric chemists as well as the ice core community (reviewer 1: "It is well written and with huge scientific implications for our understanding of the sea salt ice records taken from inland Antarctica."), reviewer 4 (for instance "This paper provides constraints to some long-standing questions regarding the source of anomalies in the ratio of Na to Cl in snow, whether and by how much Cl in aerosols is depleted in the continental Antarctic and in ice and the factors driving it."). In our response, we carefully considered all points raised by reviewer 1 and reviewer 4.

Concerning reviewer 3, we have carefully addressed the questions he raised on the poor discussion we had of the ionic balance and have now reported the mean ionic balance of aerosol (Table 1).

We cannot, however, follow two of the proposed changes.

First, it is clearly not realistic to mix the two manuscripts. Indeed in our response the reviewer 3 we argued that: "*The topics of the two papers are totally different: part one is dedicated to the origin of sea-salt and its degree of fractionation over Antarctica, part 2 focuses on the understanding of the behaviour of biogenic sulfate and MSA.*

Part 1: is 17 pages of text (including references), 1 table, and 13 figures, Part 2: is 22 pages, 5 Tables, and 12 figures. Mixing the two papers (even considering the overlaps in section

“sites, sampling and methods”, and possible mixing 2 figures together), the resulting paper would be far too long and we feel this is not realistic.”

Second, we don't agree with the suggestion to remove the section on ice core implications. Indeed, as argued in our answer: *“The ice core data can, in no way, here support the atmospheric observations discussed in this study simply because, as clearly mentioned, post depositional effects modified the original atmospheric signal. On the contrary, as clearly introduced in the paper, the atmospheric studies described in this paper are needed to better understand ice core signals. The reviewer 1 clearly highlighted this point: “In general, this is a novel and deepening study of aerosol depletions for chlorine and sulphate at a central Antarctica site. It is well written and with huge scientific implications for our understanding of the sea salt ice records taken from inland Antarctica.”*

Also the title of this paragraph is very explicit: “Implications for ice core studies” and not the reverse. From our experience some previous papers also ended with a discussion on “implications for ice core”: see for instance the JGR paper “Preunkert, S., Jourdain, B., Legrand, M., Udisti, R., Becagli, S., and Cerri, O.: Seasonality of sulfur species (dimethyl sulfide, sulfate, and methanesulfonate) in Antarctica: Inland versus coastal regions, J. Geophys. Res., 113, D15302, doi:10.1029/2008JD009937, 2008. »