

***Interactive comment on* “Tethered balloon-borne aerosol measurements: seasonal and vertical variations of aerosol constituents over Syowa Station, Antarctica” by K. Hara et al.**

Anonymous Referee #2

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General comments

Measurements of the vertical distribution of aerosols in Antarctica are notoriously rare, especially regarding chemical composition. The manuscript at hand, submitted by Hara and colleagues, presents results from tethered balloon soundings conducted at the Japanese Antarctic research station Syowa. To my knowledge this group succeeded for the first time in accomplishing year-round tethered balloon-borne aerosol measurements in Antarctica. Even though such measurements are inherently restricted (and thus biased) to “good” weather conditions, I highly appreciate the effort of this study. Accordingly, the manuscript presents authentic and original scientific material that has

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relevant implications for understanding geochemical and atmospheric processes in Antarctica as well as for the ice core community.

Nevertheless, there are several points which require some careful revision/supplements. On the whole, however, the subject is appropriate to ACP and should be accepted after responding to the questions and comments listed below.

1. Airborne measurements generally require fast sampling methods. This is an exceptional challenge for balloon-borne studies demanding extremely low weight instrumentation. As a matter of fact, low sampling volume is typically a critical factor (in this case just some 12-20 L compared to several cubic meters usual in ground based aerosol sampling). Thus solely very sensitive analytical methods come into consideration. This study totally relied on the SEM-EDX technique, which is basically limited to determine specific element ratios. Hence, it would be helpful if an intercomparison with available ground-based aerosol samples, analysed e.g. by ion chromatography, is available.

2. The possibility and consequences of aerosol modification/fractionation after sampling should be discussed. Aerosol samples are extremely dried up under high vacuum conditions during SEM-EDX analysis. Given that single particles are probed, this may cause considerable artefacts (e.g. post-sampling fractionation may be responsible for the formation of MgCl₂ particles or totally Mg-free sea salt particles).

3. Analytical accuracy, reproducibility and limits of detection (LOD) should be provided throughout (e.g. per table).

4. Large parts of the Results and Discussion section could be written more concisely (see also comment by referee #1!). It seems that the authors described the results of each balloon flight in depth. This is somewhat exhausting and a challenge for the readers' endurance. The same is true for vast selection of figures which gets really out of hand. I recommend focusing on the main results and conclusions, select exemplarily corresponding results from which these conclusions have been drawn and finally

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present a thorough scientific analysis of these cases. In addition short sub-summaries for each sub section would be helpful and welcome!

Specific comments:

Page 8157, lines 20-29: The term “upper atmosphere” should be avoided in this context, because it is usually reserved for the atmosphere above the mesopause!

Page 8185-8187, Summary and Conclusions: This chapter is, if anything, just a summary but concrete conclusions are unfortunately missing and should be provided.

Page 8186, lines 22-25: It should be noted that the impact of nitrate was merely deduced and not really measured.

Page 8187, lines 6-7: Does this mean that strong vertical gradients in relative humidity occurred above the well-mixed boundary layer (in a well-mixed layer, any gradients should be negligible)? Please clarify.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 8153, 2013.

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