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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Annual cycle of Antarctic baseline aerosol: controlled by photooxidation-limited aerosol formation" *by* M. Fiebig et al.

Anonymous Referee #1

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General comments.

The present study by Fiebig et al. is an interesting and original approach to combine continuous long-term observations of particle number concentration, size distribution and aerosol optical properties (scattering coefficients) with the aim to gain insight into natural aerosol formation mechanisms determining the seasonality of baseline Antarctic aerosol. Their analysis is based on corresponding continuous five yearlong aerosol records from the Norwegian Antarctic Station Troll, but also includes available data from all over Antarctica. The surprising crucial point of the final outcome is that annual cycles of Antarctic baseline aerosol data are not emission- but photochemistry-limited.

This conclusion also has potential implications on the interpretation of sulphate and MSA records retrieved from Antarctic ice-cores and for this reason is of relevance for the ice-core community.

The paper is written and organized in a clear and concise way. My impression is that the drawn conclusions are in general supported by a thorough analysis. Necessary assumptions are clearly identified. On the whole, the manuscript at hand is certainly appropriate to ACP and I recommend a publication after some (minor) revisions.

Focussing on the most crucial discovery i.e. the predominance of photochemistry over source strength, there are some issues which I feel deserve some more discussion. First, the assumption that source strength (e.g. sea-air exchange of precursor gases) is of negligible importance for the baseline aerosol budget in Antarctica relies largely on FLEXPART results. These model calculations demonstrate that air masses (20-days backward plumes) virtually never resided within the lowest 100 m above sea surface. The case in point is that apart from inherent uncertainties of dispersion model calculation itself, the marine boundary layer is typically well mixed up to several hundred meters. Hence I am not really convinced of this reasoning. In this regard an additional FLEXPART analysis comparable to that shown in Fig. 5 but now showing 20-day backward plumes were particles reside above the boundary layer in the free troposphere / lower stratosphere (i.e. about 1500 m or so above ground) would be advisable to support this chain of argumentation.

A second point concerns the fact that the results are restricted to baseline aerosol data. In order to judge the relevance of the main conclusion (photooxidation control) for the aerosol budget of Antarctica on the whole, the percentage of baseline aerosol (particle number and volume concentration) related to the total budget should be assessed.

Specific comments:

Page 23065, line 21: Why using here a 20th percentile and not the 2.5 times of the running 5th percentile introduced in chapter 2.2?

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Page 23066, line 15-23, impact of NOx emissions: To my knowledge NOx emission from the snow surface leads to ozone production (and not destruction), at least on the Antarctic plateau (see e.g. Crawford et al.: Evidence for photochemical production of ozone at South Pole surface, Geophys. Res. Lett., 28, 3641-3644, 2001).

Page 23077,the correct reference is: Yang, M. Y. M., Vay, S.A., Stohl, A., Choi, Y., Diskin, G. S., Sachse, G. W., and Blake D. R: Chemical composition of tropospheric air masses encountered during high altitude flights (11.5 km) during the 2009 fall Operation Ice Bridge field campaign, J. Geophys. Res., 117, D17306, doi:10.1029/2012JD017858, 2012.

Some typos:

Page 23066, line 4: “cyanobacteria”

Page 23070, line 24: “boundary”

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 23057, 2013.