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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.

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We thank Reviewer #2 for a very constructive review and for pointing out several points that improved the manuscript. We respond to the comments in an itemized fashion for clarity:

1. First of all, any backtrajectories longer than let us say 5 days are highly uncertain anywhere and especially in Antarctica. Sensitivity analyses?

   The FLEXPART model used for the backward plume analysis is a Lagrangian transport model. As such, it doesn't calculate single backtrajectories, but traces

   the path of many thousand model air particles. The transport processes considered include not only macroscopic winds, but also turbulent diffusion processes which are represented statistically. In this way, a dispersion model addresses and remedies many (maybe most) classical uncertainties of backtrajectories. A remaining uncertainty is of course associated with the windfield, which is an external input to FLEXPART, especially in a region of few observations such as Antarctica. The above aspects are now discussed in the text by adding to the sentence:

   > The correlation of the two properties is plainly discernable despite some noise, which is probably largely due to transport model errors *and uncertainties of the input wind fields to the dispersion model*, but also variations in aerosol precursor gas composition.

2. Secondly, how did you select the number, 20 days? Why not 5 or 10 days?

   The selection of the time period for following the backward plume was rather uncritical. It needed to be long enough to cover the full atmospheric turnover from upward transport at lower latitudes, transport to Antarctica, descend over Antarctica, and transport to Troll station. This is also the region where insolation depends strongly on latitude. An even longer period would have shown as an offset in the integral solar insolation in Fig. 10, but would have little effect in the slope of the correlation. To reflect this discussion, we added the following sentence to section 7:

   > The exact length of the backward plume has little influence on the result as long as the whole transport path with strong variations in latitude is covered.

3. Would the regression in Fig. 10 actually be any worse if you used simply the global radiation observed at the site? Any of these choices would

give quite a different formula for the regression line so its quantitativity is problematic. Discuss this point in the next version.

It is certainly true that the analysis done in Fig. 10 is subject to many uncertainties. The uncertainty stated with the calculated production rate covers only the uncertainty of the fit, not other systematic uncertainties. We therefore added the following sentence to section 7 discussing these in more detail:

> Also, the uncertainty on the stated aerosol volume production rate reflects only the uncertainty of the fit. Other sources of uncertainty include at least the ECMWF windfields driving the FLEXPART transport model, cloud cover, and the chemical nature of aerosol precursor gases.

4. Section 2.1 Describe the inlets, especially the cutoff sizes.

Thanks for pointing out the missing inlet description. We added to following sentence to section 2.1:

> All instruments receive their sample through a common PM10 inlet with the main pipe entering the observatory container vertically, and sample take-offs to each instrument located centrally in the main inlet pipe.

5. p.23061, L24-25. Is the criterion for contamination that number concentration in the range 30 - 40 nm is more than 3 times larger than in the range 100 - 150 nm a bit too restrictive? Why wouldn't that happen in natural particle formation?

It probably would happen also in natural formation of new particles. On the other hand, formation of new particles is the only indicator we can use to detect contamination by diffuse emissions of the main station. Due to the current location of Troll observatory, it is therefore impossible to distinguish between natural and

contamination induced new particle formation, and natural new particle formation can't currently be investigated at Troll. For completeness, the observatory is being moved as we speak.

6. Section 4, p. 23064. Did you try iteration for the refractive index?

No, because the effect of refractive index changes on the scattering coeffcient while keeping the particle size distribution constant has been quantified before (Fiebig et al., 2002). Varying the real part refractive index within the limits occuring in real aerosol causes a change in the scattering coefficient on the order of 13%. This dependence can contribute to the fact that the slope between measured and calculated scattering coefficient (Fig. 4) is less than 1. We added the following sentences to section 4:

> In addition, the assumption on the refractive index made in the calculation may contribute to the regression slope differing from 1. A variation of the real part refractive index causes a change in scattering coefficient on the order of 13% (Fiebig et al., 2002).

7. p. 23068, "L26-28 Particles in the accumulation mode size range are the result of coagulation between Aitken-mode particles and between Aitken- and accumulation mode particles" You miss cloud processing. It results in clearly higher masses than could in practice be reached by coagulation only. This has definitely consequences also in the interpretation of the relationship between aerosol volume and solar insolation integrated over the last 20 days. Discuss this.

This is a valid point. In discussing this aspect, we added the following sentence to section 6:

> Particle growth through cloud processing may occur also, but will be of little importance due to the large scale subsidence of air over Central

Antarctica.

8. Fig 1. Please use a clearly different color for the lines that are now black and blue. It is difficult to see which is which. Black and red would be a more easily distinguishable pair.

We changed the colours of the concerned graph lines to black and light red.

9. Fig 5. I cannot see the Antarctic coastline in the figure. Please use some different color coding.

The figure has been completely revised, and now also shows the Antarctic coastline clearly.

10. Fig 10. The unit of the slope of the regression should be μm^3/ (MJ m^{-2}). However, the exponent (-2) is missing in the figure legend. Also in the abstract, and elsewhere. Check it.

We tend to disagree with this comment, and think that the unit on the slope of the regression line in Fig. 10 is correct. The slope needs to convert a property of unit MJ/m^2 to a property of unit μm^3/cm^3. A simple consideration of units reveals that the resulting unit of the regression line slope is of type "aerosol volume produced per insolation energy and per light path passed" (μm^3 / (MJ m)), which could be simplified further to m^2 / MJ by using a scaling factor. A unit of "aerosol volume produced per insolation energy and per insolated surface" ((μm^3 m^2) / MJ) as proposed by the reviewer wouldn't match here. However, the misunderstanding is probably caused by a misleading title of the y-axis in Fig. 10. While the unit stated in the y-axis title is correct, the property plotted on the y-axis is the *total particle volume concentration*, not the *total particle volume* as stated in the ACPD article. This will be corrected for the ACP version.

References

Fiebig, M., Petzold, A., Wandinger, U., Wendisch, M., Kiemle, C., Stifter, A., Ebert, M., Rother, T., Leiterer, U., October 2002. Optical closure for an aerosol column: method, accuracy, and inferable properties, applied to a biomass burning aerosol and its radiative forcing. J. Geophys. Res. 107, LAC 12–1–LAC 12–15.
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