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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characterization of the inter-annual, seasonal, and diurnal variations of condensation particle concentrations at Neumayer, Antarctica" *by* R. Weller et al.

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Interactive comment on "Characterization of the inter-annual, seasonal, and diurnal variations of condensation particle concentrations at Neumayer, Antarctica" by R. Weller et al.

Response to reviewer #1

General comments: This work presents a CPC measurement data set of an impressive 26 years from the Neumayer station in Antarctica. Such long data sets of aerosol measurements are rare even from the more accessible locations in the world. The work gives an important insight on the long term trends of atmospheric particle con-

centrations and illustrates the spatial extent of anthropogenic particulate pollution. In this case, it would appear that the Neumayer station is not influenced by anthropogenic aerosols, given that there ex- ist no apparent long-term trends in the particle concentrations. It is equally interesting to notice that large natural phenomena such as El Niño or the eruption of the Pinatubo volcano did not affect the particle concentrations at this location. In my opinion, the scientific content of this paper is important, methodology is valid, and the presentation of results is clear. I have only a few comments and suggestions that the authors should address, and recommend the paper to be published in ACP. Major comments: Even though the measurements seem to be carefully conducted and the authors have clearly filtered their data with caution, I was still missing some pretty basic information about the measurement methods: How often were the instruments calibrated and where? How often were the flow rates of the instruments measured (and how); what was the standard deviation of the flow rates and was this taken into account in the data analysis? Were there any systematic drifts in the flow rates?

It should be kept in mind that Neumayer can solely be visited during austral summer (November through March) and any instrument sent back for calibration or repair will be away for one year. The dedicated, extensively instructed over-winterer is responsible for maintenance of all instruments installed in the Air Chemistry Observatory which are inspected daily for proper operation. Every month a flow calibration of the CPCs is conducted by a Gilibrator air flow calibration system (http://www.sisweb.com/sptd/gilibrat.htm). Based on hourly mean values, the mean annual standard deviation of the recorded flow was 0.078 cm3/s (i.e. $\pm$1.6%, range: 0.009-0.18 cm3/s for individual years), except for the very rare cases of pump failure. We never detected any trend in the nominal flow value of 5.0 cm3/s. Routinely, calibration of all CP-counters was performed by the manufacturer (TSI) in a 2 year cycle, except for the CPC 3025A which was calibrated in 1999 and 2006. Note, that the specification of the CPC 3022 and 3022A is identical and usually there was an overlap period of one year when a freshly calibrated CPC is run in parallel with the other one.

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The performance of the CPC 3025A can just as well be checked by comparing the signal during winter (when nucleation events are absent) with a CPC 3022(A). Only from 2005 through 2007 when the CPC 3022 and 3022A were involved in the thermodenuder experiment, we resign the calibration interruption for this period. As far as our experience goes, a 2-year calibration cycle is reliable anyway and we never noticed any significant off-set after calibration. This may be due to the fact, that the measured particle concentration range was generally well below 5000 cm-3 even in the highest temporal resolution and thus definitely within the single particle counting range.

How long were the inlet tubes to the instrument, and were tube losses taken into account in the data analysis (this can be difficult though due to the lack of size distribution measurements)? What was the inlet tube material?

The length of the inlet tubes to the CPCs was around 70 cm and we exclusively used silicone conductive tubes from TSI, $\frac{1}{4}$' inner diameter. We refrained from a loss correction (which is now mentioned in the revised manuscript), because this is highly dependent on the particle diameter (especially for Dp < 10 nm). We think that such a correction is only reasonable when using a DMA. Hence presented UCP3,7 values may be regarded as a lower limits.

These issues are most important when assessing the results of the nucleation mode particles, or as the authors state, UCP3,7. In fact, this chapter was the only part of the paper which saw some trouble with. It is somehow difficult to believe that throughout the year, 20% of the particles would reside in the size range of 3-7 nm. Typically, particles with this size only come up during nucleation events. I would therefore guess that there is a systematic difference in the results between the two types of particle counters (CPC 3022 and CPC 3025) – at least this prospect should be carefully investigated.

Indeed our conclusion "over the year roughly 20% of the particles could be assigned to the nucleation mode. . ." (see Abstract and chapter 4.3) is obviously misleading! The UCP3,7 portion is definitely negligible during winter and the 20% figure refers to an

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overall annual mean! In our revised manuscript we removed this statement, because we think it is not very instructive at best and misleading at worst. As clearly stated in chapter 3 (Data Presentation, lines 275 through 298), during winter UCP3,7 concentrations were negligible and there is no systematic bias between both instruments.

Minor comments:

Fig. 1. Please state with which instrument the concentrations are illustrated during the time, when there have been more than one instruments in the inlet.

We clarified this ambiguity in the figure caption.

Fig. 3. I understand that the authors would like to have the annual concentration peak in the middle of this figure. However, all the other figures in the manuscript have the annual x-axis from January to December. Having this one from July to June somehow breaks the flow of the figures – it is more difficult to compare against multiple figures. Consider showing the limits of the austral summer and winter here.

We insist on presenting the summer maximum without cease! As a compromise we start with January showing 18 months consecutively.

Figs 4 and 5. Given that the years are so similar in figure 4, would it be sufficient to show only figure 5? Given that the information is again repeated in figure 8.

We agree with the reviewer and removed Fig. 4 in the revised version of the manuscript.

Fig. 9. I do not find the two error bars which should visualize the uncertainty of the calculated UCP concentrations (?).

Obviously the error bars get lost by converting in pdf but should now be apparent.

Fig.10. I find this result very strange and I think the figure is redundant in the article. It gives an idea that the UCP concentrations behave like this throughout the year, which is certainly not likely. If authors really want to illustrate that the nucleation events which they observe are mostly occurring in the afternoon, I would restrict the data to only the

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clear nucleation events, i.e. when the UCP concentrations have been clearly elevated.

In fact, exclusively valid UCP3,7 values were considered (criterion described in chapter 3, lines 280-289, revised version lines 298-312)! The shown diurnal cycle is only evident from September through April and virtually absent from May through August. This is now mentioned in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 20713, 2011.