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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Long term particle size distribution measurements at Mount Waliguan, a high-altitude site in inland China" *by* N. Kivekäs et al.

Anonymous Referee #2

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The paper relates the continuous measurements of aerosol numbr concentration and size distribution properties performed at the station of Mt Waligan, Central China. The novelty of this set of measurements derives mostly from the geographical origin of the measurements covering an area whic has never been well documented. Impact of aerosol emissions in China clearly raises the issue atmospheric composition changes over the entire region. This is really the relevant information from this paper to document with robust statistics (2 years) the variability of aerosol number and size at that height representative of the regional/synoptic atmospheric environment.

General comments

To my opinion, there are few corrections required at this stage on the manuscript although the paper remains somewhat descriptive. In the following, I suggest a few ideas to improve the paper.

My main concern relates to the statistical evaluation of the results and their potential use for deriving typical aerosol properties in this area. My feeling is that, at this stage, the paper fails identifying the main factors involved in the observed variability of both aerosol size and concentration which is identified as one of the main objectives. It is important to insure that a statistical analysis does not lead to mean values that do not reflect any real observation. I am not sure that all statistical evaluation presented in this paper are not biased by mixing different categories of observation. Results are sorted into categories but the statistical relevance of the sorting is no well discussed. Practically, we do not know if the observed variability is driven by day/night changes, air mass origin, seasonality (etc..) while this should be the main information from the paper. This is resulting from the fact that statistical approaches to categorize the variability are performed independently from each other and not organized into a hierarchy. For example, Figure 5 shows quite high diurnal variability for the nucleation mode aerosol. The variability is more marked during summer than during winter (the Figure is difficult to read on a log-scale). This is certainly related to local phenomena as discussed in the text. Now, how is this variability impacting on the Table 1, i.e. to which extent seasonal averages are biased by seasonal changes of diurnal variability (results from Table 2)? A similar problem can be raised for the representativity of the observed distributions (Fig 1), i.e. to which extent distribution types contain changes in diurnal variability. This is why a more careful hierarchy in applying statistical data reduction may lead to a more robust description of aerosol properties. My suggestion is therefore to remove the local effect seen in Figure 5 before sorting according to another variable. This can be done selecting periods during which the local influence is limited (i.e. night/morning).

SPecific comments

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(p.2055, line 20-25) Size distribution categories are not well defined and it is not clear on which basis they can be considered different. How sure are we that 5 (and not 4 or 6) distributions are needed to describe all cases? Please provide criteria used for differentiating one to the other. It would be certainly interesting to better link the different distributions to environmental conditions (distribution 1: nucleation events, distribution 2: etc…). In particular, is there any distribution connected to high-altitude versus low-altitude sorting of air masses mentioned p.2056?

P. 2056, line 11. Please check if sorting according to seasons is not a European-centred view of Central China climate. Are we sure that this seasonality applies there? In addition, I am concerned that summer may not be well represented given instrument failures. This issue should be addressed somewhere in the text.

In table 2 and in most reported averages throughout the text, mean values are reported without corresponding standard deviation. This should be added also for statistically comparing different conditions.

P.2056, line 18-25: Using the CPC instead of the DMPS certainly fills the gap but also bring additional uncertainty (see Fig 3). It is surprising to find CPC values lower than those of DMPS. Excluding new particle formation events from the statistical analysis may then significantly reduce the Ncpc/Ndmps variability. The use of the CPC data is not clearly mentioned in the text or in the tables.

As mentioned in the text, new particle formation is most likely affecting size distribution. This is a very local and intermittent phenomenon. To which extent is this impacting on the mean values of Table 2? Wouldn’t the correct way be removal of all new particle formation events before data processing or to treat them separately? It is surprising to see P.2064, line 10-15 that occurrence of particle nucleation events is derived from correlation between air mass origin and average number of particles in the nucleation mode: analysis of raw DMPS information can be used directly identify nucleation events. Check Venzac et al. (2008, High Frequency New Particle Formation

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in the Himalayas; PNAS, vol. 105, no. 41, 15666-15671) for additional comparison at high-altitude Asian sites.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 2049, 2009.

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