

Interactive comment on “Aerosol particle number size distributions and particulate light absorption at the ZOTTO tall tower (Siberia), 2006–2009” by J. Heintzenberg et al.

Anonymous Referee #2

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The authors present a dataset of four years of aerosol size distribution measurements at a Siberian ground measurement site together with an air mass analysis. As the author themselves say, Siberia is poorly represented in the literature of tropospheric aerosol concentrations and the impressive ZOTTO sampling setup diminishes that lack. In the present manuscript the authors present the dataset and its processing with commendable care, and the same can be said for the description of the air mass analysis. The results obtained are interesting for the atmospheric aerosol science community and the material fits very well into the scope of ACP. The manuscript should therefore be published; however, I have a few comments especially on the interpretation of the analysis that I think should be addressed to make the manuscript paint a clearer picture

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of the submicron aerosol characteristics in this continental site. The detailed comments will be given below.

page 1160: It's not clear to me at which height the TSI nephelometer was measuring; closer to the 50 or 300 m?

I found some inconsistency regarding the origin (sources) of the measured particles:

*p 1155, l 6-> "At this site, anthropogenic influences are of minor importance, so that the sampled aerosol can be representative for a very large spatial area."

*p1162; l 8-10: "They are higher, however, than the concentrations at truly pristine sites (Andreae, 2009), indicating a contribution from anthropogenic sources." (Also in conclusion.)

*p1162: l 12-13: "This suggests the measurement at 50m to be influenced in a stronger fashion by particle sources"

*p 1171 l 8-10: "The results underline the location of ZOTTO being representative for a large spatial area and ecosystem, which is sometimes influenced by remote aerosol transport, but which apparently exhibits only a limited intensity of aerosol sources on its own."

For me, these statements give a confusing impression. On the one hand it's stated that local sources are of minor importance, but on the other hand the enhancement of concentrations at a lower lever seem to indicate a surface source somewhat near the measurement site. Also, is the contribution of anthropogenic sources deduced from anything than the higher concentration, by elimination of other explanations? For example, the mean concentrations reported here fall in the range reported eg. Dal Maso et al (Tellus, 2008, 60B, 495–508) or Tunved et al (2005, JGR 110, D07201, doi:10.1029/2004JD005085) for various boreal forest sites, with variable influence of possible natural and anthropogenic sources. It is not clear what is meant by 'truly pristine'. I think a more clear discussion of what the authors mean by sources could be

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given: near/far, natural primary/direct human emission/secondary natural/secondary anthropogenic?

The trajectory clustering analysis is well made and I think the results are valuable. The presentation of the results and the interpretation could be made somewhat clearer, as at least for me following the numbering system and trying to connect it to specific directions was quite some work. Some suggestions:

* label each cluster according to the 'name' given in Table 2 in Figs 9 and 10. The number could also be given for easier comparison to Fig. 8.

* In the caption of Fig 9 it is said that the θ_v profiles allow for a clear distinction between more and less stable stratification. This information could be added to Table 2.

* A suggestion for clarifying the message about "the slower the air mass and the more it originates from the southerly latitudes, the higher the mass and CO": this could be seen clearly in a figure with mass/CO plotted versus parameters like the 144h trajectory length and for the 'southness' maybe the average latitude of the trajectory. This is most certainly not something that I would expect to be included in the MS, but is something that I personally would try to do.

On tables and figures:

Table 2: Regarding the summer/winter parameter, with what criterium is a cluster called summer or winter? Eg. cluster 3 has no season identifier with an absolute s/w value of .3, but cluster 1 is summer with the same value. Also, clusters 4 and 6 have opposite season descriptors but both have quite high positive s/w-indexes (typo?).

I don't understand fig. 5. To my understanding, integrating the whole pdf over the Dg space should give 1 as a result. This might be so, but the way of plotting it leaves it unclear whether the integration should be performed in log space or in linear Dg space. Depicting the pdf in a log scale in the latter case seems quite confusing in my opinion,

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as the area under the curve does not correspond to a the actual probability of finding a given Dg range.

In figure 6, for completeness, I suggest that also a median size distribution for eg. the particle number concentration percentiles from eg. 47.5-52.5 (5% around the median) could be given; I guess that these concentrations are the most frequently observed and thus representative for the 'general' aerosol.

Technical:

Figures: In some figures, the σ_{ap} in the legend has turned into an s.

p. 1166 | 8: Missing 'ln'

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 1153, 2011.

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