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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol measurements at the Gual Pahari EUCAARI station: preliminary results from first year in-situ measurements" *by* A.-P. Hyvärinen et al.

Anonymous Referee #1

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The paper "Aerosol measurements at the Gual Pahari EUCAARI station: preliminary results from first year in-situ measurements" by Hyvärinen et al reports measurements of aerosol microphysical and optical properties from a sub-urban site near Indian capital New Delhi. Authors present in consistent form description of the site, methods used, data treatment, availability of data and summary of preliminary results. With increasing pollution levels in developing world, especially in SE Asia, in-situ measurements are of high importance to assess properly aerosol effects on climate and air pollution levels. However, there are issues I would like to ask the authors to consider. First I will address main points, which will be followed by more detail comments.

1) It is obvious that measurements suffered from large data gaps. Authors deserve credit for clear reporting of the problems and lack of data. It is clear that besides PM10 and PM2.5 measurements, data gaps are large and on the edge of being able to address temporal trends on seasonal to annual cycle. On page 9019 is stated that measurements are also available for year 2009 and these measurements should be included in the paper. However it is stated in title, that these are preliminary results, they are too preliminary and too incomplete. Aerosol size distribution being a good example. Moreover, there are data from following year available and there is no need for additional measurements. Analysis will benefit also from use of the sunphotometer and lidar data. For example in part focused on diurnal cycle and links between aerosol evolution and boundary layer dynamics. Combining data presented here with 2009 measurements will also significantly improve part focused on optical properties and all corrections needed to assess them properly (authors state it very clear in last paragraph on page 9024)

2) Significant part of the analysis is devoted to discussion about distribution on nucleation, Aitken and accumulation mode aerosol. Authors used arbitrary division for these modes, which does not reflect aerosol properties observed. It does not make sense set the division between Aitken and accumulation mode to 100 nm, which correspond to of accumulation mode of the whole size distributions. Why? Figure 5 show that accumulation mode covers sizes approx. from 70 – 80 nm. This small change in size, more realistic with respect to measured aerosol will make a huge difference in relative proportions of various modes. This part of the analysis should be done again with more robust division based on observations and aerosol properties, not just arbitrary division.

3) Why authors used back trajectories calculated for several days backwards and then cluster them to very narrow sectors based on trajectory arrival. This way the majority of the information from trajectory analysis is lost. With respect to rather stable and seasonal changes of air mass movement in monsoon controlled region, clustering the

trajectories based on typical transport pathway will be more suitable. There must be large fraction of trajectories passing through more than one sector. And in opposite, there must be trajectories showing the same source region and general transport pattern, but arriving in two or more sectors. Local to near-regional differences can be assessed with simple wind sectors based on local wind.

More detail comments

P 9017, l 3: it is Asian Brown Cloud

P 9018, l 19: How did you control RH for PM samplers? Did you control temperature inside the container? It must have been for significant periods of time lower than outside? How did you correct for changing RH and possible condensation in the instrument? If the inlet was heated, to what temperature did you heated the sample line? How this can affect volatilization of semi-volatile aerosol fraction? There is plenty of evidence that semi-volatile compounds play an important role (Feng and Penner, 2007;Jimenez et al., 2009;Pankow, 2010)

P 9019: can you clearly state how big fraction of data was removed or was not available for each instrument?

P 9021: based on the map provided in the article, Sector influence by Delhi is 300 deg – 60 deg, but discussion around Figure three does not correspond to it.

P 9022: can you show also standard deviation everywhere where you provide mean values for various parameters?

P 9022, l 20: I suggest to authors to use BCe terminology. It is nowadays well established parameter and measurements presented are BCe, not BC. For the sake of clarity.

P9023, l20-22: How can authors be so confident about occurrence of nucleation if data coverage is so bad?

Fig. 4 and discussion in section 3.3: Measurements are very sparse and with large gaps. With exception of period around April 2008 there is no trend. In addition to questionable division of particle size groups, this part should be combined with measurements from 2009, re-analyzed or removed from manuscript.

Fig. 7b and Page 9025: I do not see any decreasing trend. On what arguments authors base this claim?

Section 3.4: this part should be strengthen with data from ceilometers and lidar, which are available

In summary, there is a strong potential in data available from Gual Pahari site and authors presented first encouraging attempt to analyze aerosol properties, it's temporal evolution and source region analysis. Nevertheless, data included in the analysis are with exception of particulate PM10 and PM2.5 mass too incomplete to support the conclusions and discussion. Thus I recommend acceptance of the paper only if later data from 2009 are included in analysis. These data are available as stated in the manuscript.

References

Feng, Y., and Penner, J. E.: Global modeling of nitrate and ammonium: Interaction of aerosols and tropospheric chemistry, J. Geophys. Res.-Atmos., 112, -, Artn D01304 Doi 10.1029/2005jd006404, 2007. Jimenez, J. L., Canagaratna, M. R., Donahue, N. M., Prevot, A. S. H., Zhang, Q., Kroll, J. H., DeCarlo, P. F., Allan, J. D., Coe, H., Ng, N. L., Aiken, A. C., Docherty, K. S., Ulbrich, I. M., Grieshop, A. P., Robinson, A. L., Duplissy, J., Smith, J. D., Wilson, K. R., Lanz, V. A., Hueglin, C., Sun, Y. L., Tian, J., Laaksonen, A., Raatikainen, T., Rautiainen, J., Vaattovaara, P., Ehn, M., Kulmala, M., Tomlinson, J. M., Collins, D. R., Cubison, M. J., Dunlea, E. J., Huffman, J. A., Onasch, T. B., Alfarra, M. R., Williams, P. I., Bower, K., Kondo, Y., Schneider, J., Drewnick, F., Borrmann, S., Weimer, S., Demerjian, K., Salcedo, D., Cottrell, L., Griffin, R., Takami, A., Miyoshi, T., Hatakeyama, S., Shimono, A., Sun, J. Y., Zhang, Y. M., Dzepina, K.,

Kimmel, J. R., Sueper, D., Jayne, J. T., Herndon, S. C., Trimborn, A. M., Williams, L. R., Wood, E. C., Middlebrook, A. M., Kolb, C. E., Baltensperger, U., and Worsnop, D. R.: Evolution of Organic Aerosols in the Atmosphere, Science, 326, 1525-1529, DOI 10.1126/science.1180353, 2009. Pankow, J. F.: Organic particulate material levels in the atmosphere: Conditions favoring sensitivity to varying relative humidity and temperature, P Natl Acad Sci USA, 107, 6682-6686, DOI 10.1073/pnas.1001043107, 2010.

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