

## ***Interactive comment on “A global view on atmospheric concentrations of sub-3 nm particles measured with the Particle Size Magnifier” by J. Kontkanen et al.***

**Anonymous Referee #1**

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The manuscript entitled "A global view on atmospheric concentrations of sub-3 nm particles measured with the Particle Size Magnifier" by Kontkanen et al. summarizes PSM measurements from 9 sites in North America, Europe and Asia. The study compares substantially different ambient settings ranging from highly polluted Asian mega cities to boreal forests and a mountain site. The data suggest a diurnal trend in sub-3 nm particle concentrations in all places and indicate higher sub-3 nm concentrations in polluted areas than in rural background conditions. Therefore the authors conclude that for the existence of sub-3 nm particles the availability of precursor vapors is more important than the level of sink from pre-existing particles. Implications for new particle formation are also discussed. While I think that the topic itself is clearly of sufficient

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significance for the scientific community to justify publication in Atmospheric Chemistry and Physics (ACP) I have major concerns about the scientific quality of the manuscript. Below is a list of points which in my opinion should be carefully taken into consideration when revising the manuscript.

As indicated by the title the manuscript aims at a global interpretation of sub-3 nm particle concentrations. One can of course argue that data from only nine measurement sites are no acceptable justification to claim global views. However, what I found even more disappointing is the distribution of sites. All sites are located within a narrow band between 30°N and 60°N (mid-latitudes of the northern hemisphere). The authors themselves have recognized this shortcoming and suggest extension of measurements at the end of the conclusions section. Furthermore, the heterogeneity among these sites (mountain station, forest, cities) suggests to me that an extrapolation to global interpretations is hard to justify. Even if trends are found similar the reasons for this might be quite different. This also reflects in a statement on page 9, lines 29 and 30 where the PSM response in different environments is questioned. Needless to say that data from some stations have been obtained from three weeks measurement campaigns only and in different seasons, respectively. Some of the conclusions are clearly biased by data taken from the long-term measurements in Hyytiälä and Helsinki. From my point of view the data presented here are not sufficient to infer global conclusions.

I would strongly suggest giving the manuscript a different direction and focusing more on data assimilation between different PSMs. Given the effort that was obviously needed to arrive at the numbers presented here that would much better reflect the current work. Especially, since the authors clearly state in the description of the PSMs on page 4, line 8 onwards, that each instrument uses individual settings and needs separate calibration. Interestingly, when problems with unphysical data are identified such as on page 12, line 15 and following, the reason is attributed to the performance of the PSM. I consider this argument fair enough and consistent with my expectations. The intercomparison and reliability of PSM data needs critical treatment. Therefore I

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think for this study it would be more appropriate to compare rural background conditions with sites heavily influenced by anthropogenic activities.

Some technical comments: In subsection 2.2.7, site description of Centreville, the authors mention in detail precursors and their emission rates (biogenic and anthropogenic). I assume these numbers have been published somewhere and should therefore be referenced.

Also in subsection 3.4.1, page 13, line 29, a reference regarding the sulfuric acid concentration estimate from a proxy would be desirable. The same applies to line 1 on page 14 where the condensation sink is mentioned. A brief description would be good, how condensation sink was determined, or it should be referenced properly.

Page 15, line 14: I suggest rephrasing this line to "...less important for sub-3 nm particles in urban environment."

Page 16, line 14: "... aerosol particles which act as a sink for small particles."

Figures 4, 5, 6, 8 and 9: If these are median diurnal variations of certain concentrations I would expect that the concentration values should agree at midnight (left and right end of the plots). What does it mean if the concentrations are different? E.g., in Fig. 5, the red dashed line shows a factor  $\sim 2$  higher concentration than 24 hours before. Shouldn't that be the same?

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