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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Strong atmospheric new particle formation in winter, urban Shanghai, China" *by* S. Xiao et al.

Anonymous Referee #2

Received and published: 19 December 2014

The paper by Xiao et al. reports field observations of new particle formation events in a polluted environment and is trying to discern potential formation mechanisms. The paper is presented fairly well but lacks clear focus, good articulation of goals and more in-depth analysis of data, especially when it comes to unexpected findings. The paper may become suitable for publication pending consideration of comments and appropriate revisions.

Major comments

The goals of the study are presented poorly and it looks like a repetition of other similar studies in just a different area. The whole paper is presented like a report, seeking similarity with other studies. Why nucleation would be different at a Chinese location as

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is implied in line 14, P26658? At the moment I don't see one unique finding enhancing the understanding of new particle formation.

The relationship between J1.34 nucleation rate and sulphuric acid proxy is poorly investigated and the conclusions poorly supported. The correlation between the two parameters is poor and above all not statistically significant. Note that R2 is not a correlation, but the variance. R2=0.23 suggests that only 23% of the total covariance can be explained by the relationship and owing the number of points (8) is not statistically significant even at P<0.05 level (r=0.71 at P<0.05 or r=0.83 at P<0.01 is required instead of obtained r=0.48). That is suggesting that either sulphuric acid plays insignificant role or the scarcity of data prevent statistical robustness. By contrast correlation with ammonia is much stronger and statistically significant, but it is played down to secondary role. What about considering organic species in forming new particles, like amines? The power of sulphuric acid proxy is also presented without error bars or standard deviation and considering the above is highly questionable. At the end there a big question mark whether sulphuric acid proxy was adequately derived.

The relationship with PM2.5 concentration is not appropriate because new particle formation is unrelated to particle mass, but rather its surface. PM2.5 mass is a qualifier of air quality and only remotely linked to condensation sink. Why not to correlate to particle surface and also to examine any relationship between surface and mass?

In a highly dynamic and polluted environment such as urban one should be very careful when it comes to averages. Strictly speaking average only applies to normally distributed values while generally aerosol parameters are log-normally distributed and, consequently, geometric average (median) and ranges should be used instead. Even more so that the authors presented parameters varying by more than one order of magnitude.

Minor comments

Abstract

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Please tidy up numbers throughout the paper: report particle sizes to three digits consistently; remove technical information about size bins in the abstract; growth rates present with one number in the subscript; present error bar for sulphuric acid proxy power. Revise abstract according to major comments.

P26657, Line 17. “Very recently progress has been made by the use of PSM, etc. . .” P26658, line 2. Populated region, not area. Line 13. “the use” not “employment”. Line 13. Measurement down to the certain size is not the measurement of the nucleation which has to be worked out from data. Line 24. Competition between available surface and condensable vapour is a fundamental process and is very well known.

Experimental

The sampling manifold was poorly designed. First, the flow in the main duct was highly turbulent. Second, various instruments were non-isokinetically subsampling as the air velocity in the duct was 9.1m/s versus subsampling velocityofs 0.75m/s. That created a two-fold problem. Particles of different sizes were not sampled at the same efficiency leading to under-sampling of small particles and over-sampling of larger ones (it was like in a virtual impactor). While under-sampling was probably a minor problem, over-sampling was rather detrimental leading to overestimated condensation sink. Was the diffusion loss taken into account when processing data? Please report time resolution of SO2 measurements as it is important in drawing some of the key conclusions. P26661, line 8. PM2.5 aerosol inlet, not cutter.

Results

P26665, line 15. Reduction of particles, not loss. P26668. More thorough study is required about sulphuric acid proxy. Present and study not only average concentrations. but also 25/75 percentiles and perhaps min/max too. Remove Figure 8 as it is conceptually wrong and the graph explains nothing.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 26655, 2014.