Atmos. Chem. Phys. Discuss., 12, C4340–C4344, 2012
www.atmos-chem-phys-discuss.net/12/C4340/2012/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Observation of aerosol size distribution and new particle formation at a mountain site in subtropical Hong Kong" *by* H. Guo et al.

Anonymous Referee #1

Received and published: 5 July 2012

The manuscript examines aerosol size distributions and specifically the process of secondary particle formation during a 3 months campaign in Hong Kong. The size distribution measurements are supported by rather extensive gas phase and meteorological measurements. Indeed, as the authors point out, this type of studies are rare in the region, and I find the dataset interesting and potentially important for scientific community. However, in my opinion, the analysis and writing of the manuscript should be largely improved, if not even rewritten, before it could be accepted to publication in ACP. In its current form, the analysis is not full and the presented results are confusing and speculative.

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Interactive Discussion
Discussion Paper

My major concerns are:

1) For a large part of the work the authors present linear correlations and R2 values for nucleation mode concentration and various measured parameters. The R2 values for individual events, which are based on hourly data-points, are not sufficient to make conclusions on the particle formation mechanisms as the authors claim. Statistical significance of the correlations should be at least examined. Even then, the chosen linear correlation model is hardly justified. Many atmospheric compounds have a profound diurnal cycle making the correlation analysis difficult and the variables are not independent. Moreover, a better idea of the compounds involved in particle formation and growth could be obtained by comparing particle formation and non-formation days. Also, as the nucleation mode concentration depends on e.g. particle nucleation rate, growth rate and sink, I would recommend rather examining directly these correlations with gas phase and meteorological parameters instead of the particle number.

2) In addition to analytical deficiencies, the manuscript suffers from several spelling and writing mistakes and incomplete reference list. It is not easily readable and it seems lengthy considering the amount of results presented.

More specific comments:

Introduction:

Introduction has several writing mistakes and incomplete sentences, please check. The whole chapter starting on p. 12121 L25 onward is confusing and full of loose sentences. Maybe it would be enough to state out the most important issues regarding this specific work on the current knowledge of different compounds participating and on the formation mechanisms, such as the role of organics?

2. Methodology:

2.1 Could the authors add here from where the clean air masses originated?

2.2 “polluted air from inland often reaches the sampling site” is contradictory to what

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was said above at 2.1

2.3 How was the SMPS sample RH controlled/dried?

2.4.1:

Maybe it is not necessary to explain in so much detail how modal concentrations were calculated. But how the modal diameter limits where chosen?

“some data from late September to early October were unavailable”.. It seems there is no data missing on number concentrations during this period based on figs. S1-S3.

If the instrument suffered from high RH, was the sample not dried?

What is meant with “power correlations”?

2.4.3:

Authors could calculate 5.5 nm particle formation rates with proper methods, taking into account the coagulation losses (and growth losses). The presented formation rate underestimates the real formation rate, especially if the background concentration is high. At most, it should be stated that this is "minimum estimate".

I assume the GMD here means only nucleation mode GMD? Which sizes where included? Was any RH correction applied in size distributions when CS was calculated?

3.1.2:

Instead of separating the particle properties by months, it might be more informative to separate them e.g. by particle formation/non-formation days; polluted/non-polluted days; different air masses; etc. Even authors seem to suggest that by discussing the reasons behind different monthly diurnal parameters in p. 12133 lines 12-24.

What do authors mean by “photochemical formation of new particles”? (p. 12133 L23)

p. 12134 L5-12: While on O3 episode days nucleation mode concentrations were higher it seems also the total number (in all modes) increased. Maybe authors could

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use the measured NOx/SO2 data to show the air on O3-episode days was less polluted than on other days?

How the modal peak diameters were calculated?

P. 12134 L. 14-16: “On O3 episode days the Aitken mode particles were probably from condensational growth and coagulation and from primary emissions..” This is very speculative and includes practically all the Aitken mode particle sources.

3.2: p.12134 L21: remove the word “mainly”

p. 12135 L1-3: Why there is word “hence”? Why only two events are examined and not all? The presented results don’t after all identify the sources nor the mechanisms of the NPF and growth.

Case 1 – new particle formation under a clean background:

In this section the authors use several times word “perhaps” without further justifying their assumptions. The section should be shorter and less speculative. In trajectory figures (S4-S5) authors present trajectories for 3 consecutive days instead of the examined day. Why? It would be also interesting to compare the trajectories on particle formation and non-formation days.

p. 12136 L10: It should also be noted that particle formation here is observed with a delay as being measured only at 5.5 nm. So this delay must depend also on the GR.

Case I:

What does the sentence “The various temporal variations and correlations. . .” mean?

p. 12138 L20-30: The authors should calculate the H2SO4 concentrations from the proxy (Petaja et al., 2009) instead of speculating with SO2 concentrations. They could also calculate the fraction of GR explained in maximum by sulphuric acid condensation (see e.g. Nieminen et al., 2010, ACP).

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3.3.1:

p. 12140 L21-25: The sentence “In this study, the similar start time for all the NPF. . .” is too general and speculative

p. 12141 L1: “low SO2 mixing ratios on NPF days revealed that clean air masses were favorable to the NPF” is contradictory to p. 12135 L1: “NPF events were often associated with elevated SO2 and O3”

3.3.2:

p. 12141 L26: “The threshold was calculated at representative..” I don’t understand what this means.

Non-NPF days should be added to Fig 7.

p. 12142 L7-9: How is it showed that low temp and RH were favorable conditions for nucleation?

p. 12143 L12-14: Would be nice to see these exponent correlations shown in a figure.

Conclusions:

“dry monsoon .. created favorable conditions for NPF” is quite speculative since seasonal data is not available

Why it is said that NPF occurred on 12 out of 35 days? In three months there should be more than 35 days?

“The SO2 products might dominate the nucleation process while the VOCs could mainly involve in the condensation growth. Further investigation on correlations . . . showed that isoprene, a-pinene and b-pinen might be the major potential BVOCs responsible for the NPF events at the mountain site” seems also very speculative and not fully justified by the results presented

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 12119, 2012.