

## ***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 #2**

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In their manuscript, the authors present data from a measurement campaign that covers quite well a lot of parameters that are interesting for investigations on particle formation in the planetary boundary layer, such as aerosol number size distributions, BVOC and AVOC concentrations, SO<sub>2</sub>, O<sub>3</sub>, and radiation. As the authors state, such observations are far from ubiquitous in Asia, and therefore such reports are generally welcome.

However, despite the fact that the measurements and the data obtained are interesting, the manuscript has some shortcomings that in my opinion would require some major work before it could be published in ACPD. I will list them in the following:

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1) Throughout the results and discussion part, the authors present several conclusion-type sentences, (as an example. p.12135, l24-l26: "Obviously...", or p12136, l23–26: "In this event..."), which are generally formulated quite conservatively so that the evidence suggests some processes or pathways, quite often photochemistry being involved in a non-specific way. The large number of suggested possibilities makes it quite difficult for the reader to get a clear picture of what is/are thought to be the main process/es causing particle formation. It also makes the whole manuscript very speculative, and the conclusions seem very vague.

2) Linear correlation coefficients of particle numbers and different trace gases are the main tool used by the authors to back up their suggestions. As in the atmosphere many parameters are correlated, in my opinion the correct way to use such correlation coefficients would be to compare correlations of different factors, and take into account the processes possibly leading to such correlations, and based on such analysis concentrate on the process of interest. Correlation between two parameters alone does not imply or mostly even suggest a direct link between two parameters. Additionally, the use of the R<sup>2</sup> was not done very convincingly, with no discussion of eg. the confidence level given. Generally, I think that if correlations are used to elucidate the particle formation process, it is important to include days with no particle formation also in the analysis; for example, even though isoprene is positively correlated with particle number on event days, if isoprene is also elevated on some non-event days, it cannot be considered a critical parameter for NPF without some explanation for this. To summarize, I think a more focused approach with some mechanistic and process-oriented analysis is needed, where possible particle formation mechanisms are ranked by their likelihood based on the observations made.

In addition to these, I have some detailed comments on the manuscript:

p12120, l 25: "Especially... " This sentence is strange, I'd consider reformulating

p12121, l1: "radioactive"-> radiative

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p12122, l1 "While...": this sentence does not go well with the surrounding text

- l 11: what is meant with "low VOCs"?

l12124 l19: "The sampling site is..." this sentence duplicates the info on l6.

p12127-12128: Data processing: to my understanding, the formula to get the number concentration in a size interval, given in eq 1, holds if  $N(dp)$  is the concentration density in size space, which is the way number size distributions are usually given (unit  $dN/d\log Dp$ ). In the case that  $N$  stands for number concentration (as stated in the text), then no normalization factor is needed in the summation. This is important also for the calculation of the condensation sink (eq 4), as the number concentration  $N$  in that formula should already have the normalization (ie. the transformation from concentrations density to concentration) factored in. This should be checked.

p 12128, l 13-> "The mean PN concentrations..." I did not understand what was done here.

p12134, l25: To my experience, the growth rates cannot be determined from field data to an accuracy of 0.01 nm/h, so using such high accuracy to report them is not appropriate.

p 12136, l28: as a note, I would like to note that the correlations for SO<sub>2</sub>, CO and NO reported here are mostly representing no correlation at all; this could be discussed.

p. 12137, l20: In my opinion, the anti-correlation of  $N_{nuc}$  with surface area is not an explanation of the  $N_{nuc}$  increase, merely an observation.

p. 12139, l11&12: the "weak linear correlations" seem to me to be more no correlation at all

p 12143: There has been discussion of the role of the power exponent in explaining the nucleation mechanisms. Whether it can be reliably used or not is still open, but in my understanding it should be computed for the relation between the concentration of the

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vapour in question and the formation rate  $J$ , not the concentration of small particles.

p12144: the section "Particulate Matter" seems quite disconnected from the rest of the text.

In addition, I would suggest careful proofreading for the use of English, as I found a number of examples where the language was not used correctly.

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