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Comment

## ***Interactive comment on* “Characterization of ultrafine particle number concentration and new particle formation in urban environment of Taipei, Taiwan” by H. C. Cheung et al.**

### **Anonymous Referee #2**

Received and published: 10 May 2013

This manuscript investigates the origin and behavior of ultrafine aerosol particles in an urban environment. The paper is worth to be published after the authors have addressed the issues brought up below.

Page 8993: Based on nighttime measurements, the authors have derived a primary emission ratio of 60 cm<sup>-3</sup> per ppbv of NO<sub>x</sub> emissions. I suppose a corresponding relation have been reported in some other investigations as well. How does this number compared with values found by others?

Page 8994 and figure 5: How was Aitken mode particle number concentration changing

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diurnally? The discussion and fig 5 might benefit from including Aitken mode particles as they connect nucleation and growth. Lines 9 and 10: technically, total particles cannot be divided into nucleation and accumulation mode particles, as there is always Aitken mode present as well.

Page 8995, line 1: What do the authors mean by stating that NPF influenced significantly on local air quality? The particle number is usually not used as a measure of air quality, and NPF did definitely not have any influence of particulate matter concentrations.

Page 8996: The interpretation of figure 9 is weak. Without the one point at the upper part of the plot, a linear fit to the data would be as good as exponential one. The authors have excluded one point from the bottom of the same plot without a good reason. In my opinion, the only thing that can be interpreted from the figure is that GR increases with increasing  $J(O1D)$ . Claiming that the increase is exponential is not convincing.

Section 3.5: There are a few minor issues related to formation and growth in this section that could be improved. First, I recommend that the authors use  $J_{10}$  instead of  $J_{10-25}$ , i.e. the formation rate of 10 nm particles, to make the notation consistent with most of the literature. The upper limit is not that relevant here. Second, I do not agree that the observed formation rates of 10 nm particle are at the lower limit of values reported in the literature. It is true that  $J_{10}$  is comparable or larger than observed here in many urban locations, but in rural or more remote location values of  $J_{10}$  are almost always lower than reported here. Besides the Kulmala et al. 2004 paper, the authors could include more recent material on  $J$  and GR (see e.g. Manninen et al. 2010, Atmos. Chem. Phys. 10, p 7907-7927, and references therein). Third, the authors should state explicitly that  $J_{10}$  is always smaller than the actual particle formation rate (nucleation rate), or the formation rate of 3 nm particle reported in some studies, because some fraction of formed nuclei is always scavenged by coagulation into larger pre-existing particles before they grow larger by themselves by condensation (see Lehtinen et al. 2007, J. Aerosol Sci. 38, p 988-994;

Minor/technical issues

Different units should be separated with a space (e.g.  $\mu\text{g m}^{-3}$  and  $\text{m s}^{-1}$  on page 8992).

Page 8989: please spell out a.g.l. and Lpm

Page 8992, line 15: should read associated with...

Page 8994, line 27: one fold sound strange in this context. Do the authors mean several fold or 10 fold?

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 8985, 2013.

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