

Interactive comment on “Size-resolved aerosol water uptake and cloud condensation nuclei measurements as measured above a Southeast Asian rainforest during OP3” by M. Irwin et al.

Anonymous Referee #1

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The paper by M. Irwin et al. is reporting on simultaneous measurements of size distribution (SMPS), chemical composition (AMS), hygroscopicity (HTDMA), and size-selected CCN properties of aerosol in the tropical Malaysian rainforest. It is a very dense data set of good quality, which enables to evaluate the consistency of the measurements and give some information on the degree of our understanding of aerosol-cloud interactions. The amount of information available in this work is very large, and it is sometimes frustrating that little space is left for a more in-depth analysis. Many results are shown without comments and the overall impression is of a rather descriptive paper. There might be some space for some synthetic figures (average size distribution diurnal variations?). On the contrary, some figures might be redundant with others (see

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detailed comments). It would have been convenient if some measurements such as the aerosol size distribution and chemical composition had been published separately, as the goal of this paper is clearly orientated towards aerosol water uptake.

Detailed comments:

Page 3123, line 1: SMPS steeped discretely: a DMPS? Line 6: each mobility diameter is spanning around 12s (not scan)? Line 15: generally particles produced by a nebulizer are overcharged Line 20: if the lower limit is 45 nm, then this value should be used throughout the paper (abstract included)

Page 3125, line 2: The CE is dependant on different parameters including the aerosol chemical composition so I am not sure that relying on measurements performed in the laboratory with another instrument (?) is useful for assessing the present CE.

Page 3126, line 10: Are the authors still referring to supermicron particles? Line 12: collapsing of the BL below the measurement site would lead to a decrease of the number concentrations during the night. From line 12: I am not convinced that the description of the time series is usefull as it is if no interpretation is given, nor any attempt to synthesize the observations. Is there a typical diurnal variation? Does it differ in marine and terrestrial air masses? Which processes might be the cause? Any noticeable event disturbing the eventual regular diurnal variations? It is not easy to get these information from figure 1c.

Page 3127, line 4: the part may better belong to the previous paragraph. This means that the submicron aerosol concentration varies opposite to the supermicron aerosol concentration (page 3126, line 1à)? Any comment? Lines 24-26: the methodology for screening for external mixing has not appeared very clear to me. If the mean growth factor is close to the peak growth factor (within 5%), this indicates an internal mixing state, it might be clearer to mention that you want to exclude external mixing from the data set.

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Page 3128, lines 6-12: even when selecting periods when the aerosol is found as an internal mixture in a given air mass type, its hygroscopicity can be quite variable through these selected measurement period. Could this variability explain the non monotonic behavior of the average activation fraction distribution? I suggest further selecting for constant average HGF periods: 80 -90 nm particles activating more readily than 100-120 nm particles might just be an averaging artefact (these particles might never coincide in time).

Page 3130, line 7: a quantification of the number fraction of aerosols belonging to each hygroscopic class would give a more precise idea of the degree of external mixing in each size class and help with the discussions later in the paper. An increasing contribution of the hydrophobic mode particles with increasing particle size is rather unusual, no? lines 8-11: this sentence is hard to follow; conclusions might change according to results obtained from the previous remarks

Page 3131, line 16-18: again, the small diameters seem less externally mixed than the large diameter, so external mixing can not explain the variability in the activation fraction; however, a higher variability in the mean HGF itself might introduce a higher variability in the activated fraction?

Page 3133, lines 15-20: from Figure 10, the same conclusions are given than from figure 9. Is there any use of the figure if the differences between marine and terrestrial conditions are not commented? Discussion on discrepancies: upon volatilization, the dry diameter might decrease, but the resulting HGF might either increase or decrease according to which type of compound has volatilized (NO₃, Organics..). In the present data set, organics would be preferentially volatilized, and preferentially from the largest particles?

Page 3135, The authors have now a very good knowledge and experience of reconciliation exercises (Good et al. 2010a, 2010b, Irwin et al. 2010 for the most recent). Here, the authors point different features than in previous studies. Any explanation?

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Page 3136, line 18-19: "possible external mixing": the degree of external mixing is exactly known from HTDMA measurements. Line 21-22 : this should have been quantified before ; again, quite unusual.

Page 3138, line2-6 : this has not been quantified neither before, by how much the Nccn differ one from the other using different instruments/methods ?

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