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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 #2

Received and published: 14 April 2011

General comments:

In the present paper, Irwins et al., have reported the simultaneous measurements of aerosol size distribution, chemical composition, hygroscopicity, and CCN properties of aerosols using SMPS, AMS, HTDMA, and size resolved CCN measurements. The measurements were carried out, marking the first study, in the tropical rainforest in Borneo, Malaysia. The data appear to be of high quality and of high relevance for atmospheric science studies and manuscript is within the scope of Atmospheric Chemistry and Physics (ACP). And I congratulate authors for this achievement. However, manuscript appears to be, in parts, haphazardly written as there are some open ques-

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tions, which I believe, authors should try to address before manuscript is finally accepted for publication in ACP. I echo the concerns raised by Referee#1 in his opening comments.

Specific comments:

1 Introduction Page 3119, Line 18: I believe the cataloguing of Amazonian aerosols as almost “marine” is derived based on mostly the numbers similar to marine regime and not on activation properties as they are markedly different from each other. Authors may consider rewriting the statement.

Page 3121, Line 14: I am not sure if Shinozuka et al., 2009 performed the size resolved CCN measurements, kindly double check and Pringle et al., 2010 was a model study. Recently Rose et al., 2010, 2011 have also reported the size resolved CCN measurements, although, from megacity.

2 Methodologies

2.2 CCN measurements

This particular section is very important but appears little difficult to follow especially with the switching between how Sc and D0 were derived and used (as to derive a parameter one need to be kept constant and scan other one, is this the way it was carried out?). I did not find the values of the flows for the instruments used during campaign in the manuscript and same is with the values of set supersaturations in DMT CCNC. Authors have given standard errors of supersaturations derived from the fits but did not talk about errors associated with the model choice, as according to Rose et al., 2008 that may introduce errors up to >20% for lower supersaturations. Kindly comment.

2.4 Other measurements

This section is explicitly discussing about AMS measurements then why to call it other measurements and why not as AMS, or mass spectrometry measurements something

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like that? I do repeat Referee#1 comment about CE, unless authors compared submicron volume time series from the DMPS measurements “measured” during campaign. If that is the cases please say it explicitly that it was compared with DMPS measurements carried out in field.

2.5 Air mass classification

Page 3125, Line 17: Did authors mean 850 hPa instead 950 hPa????

3 Results

3.1 Aerosol composition and size distribution

Is it possible for authors to mention the local season when the measurements were carried out? It would be meaningful to show the average number size distribution of aerosols (diameter vs. concentration) along with what is show in Fig. 1c, at least in supplement. For the complete result section I recall the concern that some figures and associated Tab. 1 appears to be redundant and need to be modified. In addition it is not clear what is the reason behind the diurnal variation is? Does it mean that the aerosol properties were dominated by some local sources and not by wind change as evident form back trajectory analysis? Please comment.

3.4 Aerosol sub-saturated water uptake

Page 3130, Line18: With refereeing to Figs. 6a, 2, and 1c authors claim that number distribution does not influence the water uptake behaviour substantially. Did authors mean number – size distribution or only numbers? What is water uptake behaviour; did authors mean GF or kappa? Please be specific as water uptake does not have any definition. I have major concern about the claim here: First of all Fig. 6a humidogram is from 26th June to 27th June where as on page 3122, line 6 authors explicitly mentioned that results during 3rd to 20th July are discussed, I am confused here and secondly Fig. 2 and 1c do not cover the data period shown in Fig. 6a humidogram (i.e., 26th to 27th June). Hence by comparing two different “regime” time data sets I am not convinced

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with this claim. Kindly address or take out the sentence, mainly because authors not only do not have CCN data but also any other data except HTDMA (humidogram).

3.5 and 3.6 Hygroscopicity

I am partly not convinced and sure with the reason behind the incongruities in the kappa values of kappa_sc and kappa_gf. I request authors to please mention this incongruity explicitly in the abstract and may be elaborate what do authors mean by "instrumental difference" so as to support that other data is not affected.

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 11, 3117, 2011.

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11, C1842–C1845, 2011

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