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Interactive Comment

Interactive comment on "Aerosol fluxes and dynamics within and above a tropical rainforest in South-East Asia" *by* J. D. Whitehead et al.

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We thank both of the reviewers for their encouraging comments and address each of their specific questions below.

Referee #1

Page 12026, line 22: What is meant by "Maritime Continent region"?

"Maritime Continent region", is a widely used term to describe the mixture of land masses and sea spread over a very large area in South East Asia (e.g. Neale and

Slingo, 2003; Ramage, 1968). We will make this clearer in the revised manuscript.

Page 12026, line 28: This paper focuses on aerosol transport (small-scale one), rather than aerosol dynamics. Usually, aerosol dynamics refers to processes that shape the aerosol size distribution.

We will substitute “dynamics” with “vertical transport” where this is specifically described, and with “behaviour” where we are speaking more generally. Elsewhere, “dynamics” will remain where we discuss changes to size distribution.

Page 12036, line 15: To me, the particle number size distribution in Figure 4 is at least bi-modal, with mode mean diameters at about 50 and 150 nm. Furthermore, the modes are not very narrow. Thus, the impression given by authors that there is a mode at 50 nm is not correct to me.

We will re-write our discussion of figure 4 to read “A dominant mode was consistently seen at 50 nm, with evidence of another mode around 150 nm”.

The authors use the following expressions: “within canopy”, “in-canopy” and “below canopy”, presumably to indicate the same thing. Please be more consistent in terminology.

“within canopy” and “in-canopy” do mean the same thing and we will be more consistent and use “in-canopy” in the revised manuscript. However there are various levels in a forest, and these expressions refer to measurements made in the canopy level. “below canopy” on the other hand refers to measurements made nearer the ground in the trunk space, where the leaves and branches are much less dense. We will try to clarify this by using the term “trunk space” instead of “below canopy”.

Figure 6 is a bit confusion. Would it be better to separate this two picture clearly into

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separate ones, i.e. 6a and 6b?

We will separate figure 6 into figures 6a and 6b in the revised manuscript, as suggested.

Referee #2

1. Perhaps the observation of a lack of new particle nucleation should be more carefully considered; no information is provided concerning calibration or cut-off point verification of the CPCs used, in light of which the observation cannot be considered reliable.

The TSI CPCs are commercial analysers that are periodically sent back to the manufacturers for full service and calibration or this is performed in house by one of our instrument scientists who has been trained by TSI. This was done shortly before the OP3 campaign. Evaluations of the cut-off for these models of instruments have been published by Quant et al. (1992) and Mertes et al. (1995). While such verification has not been performed on these two particular instruments, previous measurements running them side-by-side have been successful in detecting nucleation mode aerosols (e.g. Whitehead et al., 2010), with agreement with integrated SMPS ultrafine particle size distributions over the CPC size range. This has also been verified by running alongside a DMPS in an urban environment (unpublished data). Whilst the exact cut off of these two instruments has not been determined beyond theory, we are confident that the lack of difference between the two instruments indicates that there aren't significant numbers of nucleation mode particles above the rain forest. This is confirmed by comparison with the DMPS measurements. We will briefly discuss this in the revised manuscript.

2. No consideration has been given to the influence of the extremely long inlet systems to (a) the APS and DMPS, and especially (b) the AMS bag samples. The length of the main inlet pipe to system (a) is not stated in the Methods section, but I assume it was

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about 33m. The sampling height needs to be stated in Section 2.3. Potential losses of aerosols in the various tubes of the complex inlet system need to be considered, not least since plastic pipes can eliminate a fraction of very small as well as large aerosols. A solution could be measurements at the same height with/without the inlet (experiments conducted in the UK would probably be sufficiently convincing – providing differences were minimal). In case (b), the 40m/0.25 inch OD inlet line is incredibly long. What were the losses / size shifts / compositional changes incurred during transit through the inlet (note: at quite a slow flow rate of 10 L/min)? It is not sufficient merely to consider the losses once the aerosol had arrived in the bags.

(a) The main inlet pipe was 30 m. This, and the sampling height of 33 m, will be stated in section 2.3 of the revised manuscript. A test was conducted at the start of the campaign with two of the OPCs: one at the top and one at the bottom of the inlet. This showed that the transmission efficiency of the inlet was around 70% for the submicron range of the OPC (0.3 – 1.0 mm), but gradually reduced to around 30% for 20 mm particles. In addition a comparison of the total concentrations from the DMPS (at the bottom of the inlet) and the CPC 3010 (at 47 m on the GAW tower) showed a strong correlation (r^2 = 0.86) with a slope of 0.96, suggesting losses in the small size range were 4% or less. This will be explained in the revised manuscript.

(b) The long inlet at the in-canopy site was $\frac{1}{4}$ inch stainless steel. Theoretical calculations (Brockman, 2001) suggest that, at this sample flow rate the losses were small ($<$ 1%) for the measurement size range of the AMS (we will explain this in the revised manuscript). In any case, the residence time in the inlet (4 seconds) was small compared to the residence time in the bags (around 40 minutes), so it is in the bags where most of the losses are expected to occur, and we attempt to account for this, as already described in the manuscript.

3. "Above canopy" measurements using the APS, DMPS and PVM were conducted at heights lower than the average canopy height (35m). What therefore is the influence *of the different canopy heights here and at the within-canopy site? How are different canopy top heights (for example 25m and 45m, the stated min and max range) likely to affect aerosol production and exchange?*

The GAW tower was situated in a wide clearing at the top of the Bukit Atur ridge. The sides of this ridge are steep and the average canopy height at the top is smaller than further down in the valley, where the in-canopy site is situated (we will clarify this). Therefore the air at the GAW tower is representative of above canopy air within the measurement footprint that covers a large area of forest. As already mentioned in section 2.1, the average tree height within this footprint is 35 m with a range of 25 – 45 m. Thus we do not have information on how different canopy heights might affect aerosol production and exchange.

4. I wasn't entirely happy with the proximity of the sonic anemometers to the trunk of a large tree. How far were they from the tree, and what was the influence of this large object on the measurements?

As the data from these sonic anemometers are not used for this paper, we will remove reference to them. The measurements from these instruments will be dealt with in a forthcoming paper.

5. The calibration data for the Grimm monitors is incomplete. Calibrations (slopes) should be reported for each of the 15 size channels. At least ranges of slopes for individual channels should be stated.

We will add the following sentences to section 2.5, along with a table of the slopes: "Inter-calibration with each other at the start and end of the measurements showed one of the units (at 8 m) underperformed compared to a pre-calibrated reference analyser (forest floor). The slopes of this and the other OPCs when plotted against the reference OPC are shown in table 1. Therefore, the data from the 8 m OPC has been discarded,

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and only the data from the other levels, calibrated accordingly, will be presented here"

6. The discussion of particle growth (Section 3.4) is not clearly presented. Please clarify whether the aerosols exhibiting growth originated above or below canopy top.

Aerosol growth was observed inside the canopy with the OPCs and was attributed to fog formation. Fog was also detected above the forest concurrently by lidar, so the growth occurred both above and below the canopy top. These are likely to have grown from submicron sizes (as already discussed). We do not speculate on the origin of these particles in section 3.4, but as discussed in section 3.5, they are likely representative of regional scale aerosols. We will state this in section 3.4 of the revised manuscript.

7. Evidence for the time delay between phenomena observed at the tower and in canopy sites is poor. Sometimes there was a delay, at other times not. Figure 9 presents data for only one day to support this assertion, which is not enough. Overall, for the full campaign, what was the frequency of the time delay?

Figure 10 presents the diurnal median and inter-quartile range variation of the quantified time delay for the whole of the June-July measurement period. It therefore shows roughly when there was a time delay and when there wasn't, as a function of time of day. It clearly shows that there was little or no time delay during the middle of the day, compared to night-time when long time delays could be seen, which is the important observation here. Figure 10 should therefore already be sufficient to answer the reviewer's query and we will explain this figure more clearly in the text of the revised manuscript.

In addition to the revisions addressing the reviewers' comments, we also wish to add a figure (Fig. 13 in the revised manuscript) along with some explanatory text, to clarify the discussion on the spectral analysis in Sect. 3.5. It shows a regular diurnal pattern

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in the power spectra of *w*, with spectra typical of particle transfer events being seen during the day, approximately coincident with the reduction in Δt. Spectra similar to ones seen when particle transfer does not seem to occur are generally seen at night. This supports the idea that particle transfer through the canopy is dependent on large scale processes and so would be a valuable addition to this paper.

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