

Interactive comment on “A comparison of dry and wet season aerosol number fluxes over the Amazon rain forest” by L. Ahlm et al.

Anonymous Referee #2

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General comments:

This manuscript extends a recent article “Aerosol number fluxes over the Amazon rain forest during the wet season” by the same authors. The new manuscript focuses on dry season observational data over the Amazon rain forest. In addition to a discussion of dry season aerosol number and CO₂ concentration and flux data following the previous article, the authors examine the dependence of the particle deposition velocity on stability. Since aerosol number flux data are scarce (especially from remote sites such as the Amazon rain forest), this is a valuable contribution to the field. However, parts of the manuscript repeat the previous article by Ahlm et al. (2009) and should be shortened. For example, the “Method” sections are almost identical. The results are presented in a clear and structured way, put in context with the present state-of-the

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art, and discussed accordingly. The general conclusions are somewhat tentative and should be strengthened by additional supporting data (if available) and data analysis.

Specific comments:

1. In my opinion, the abstract is too long and should be shortened. For example, the second part of the first paragraph (The primary goal is to . . .) could be replaced by a summary of the main results and conclusions with regard to these goals.
2. The authors give emission of natural biogenic particles from the rain forest as a possible explanation for morning upward particle fluxes. Does this imply a primary or secondary source of biogenic particles? What mechanisms could generate these particles? Are there any supporting data available? It is stated from the literature on p. 26884 (lines 21–23) that “natural biogenic particles are present in both the dry and wet season (. . .), and are a significant fraction of the aerosol mass, with a strong dominance of coarse mode particles (. . .)”. Thus, while coarse natural biogenic particles contribute to aerosol mass, their contribution to aerosol number remains unclear.
3. The CPC was logged at 1 Hz, and therefore a response time of 1 s has been used to correct the aerosol number fluxes for limited instrumental response according to Eq. 1 in the manuscript. While the time response of the CPC alone may be around 1 s, the measuring system consisting of a 4 m long 1/4-inch sampling line and the CPC with a flow rate of 1.08 l min⁻¹ is certainly much slower. Without a backup flow, the flow regime within the sampling line is laminar and will generate a parabolic flow profile. This will lead to additional attenuation of the number concentration fluctuations. Thus, a slower time response (without knowing the technical details, 1.5 s to 2 s would be my guess) seems to be more appropriate. In addition, the original formulation of Eq. 1 has been put forward by Horst (1997). This reference should be added.
4. I cannot follow the explanation why the Webb correction has not been applied to particle fluxes. While it is certainly true that temperature fluctuations are dampened in the inlet tubing, I am not sure if the temperature and humidity conditions inside

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the CPC are relevant considering the time scales when sampling at a frequency of 1 Hz. The vertical advection term that is added to the measured flux when using the Webb correction is evaluated from the water vapor and sensible heat fluxes. One could argue, however, that the Webb correction may be negligible for particle number fluxes because the number fluctuations are typically large compared to the mean number concentrations.

5. On p. 26890, the authors state that “there are of course other sources of uncertainty in aerosol flux quantification. . .”. The authors should state some of the other uncertainties and give at least a qualitative estimate of their relevance for aerosol number fluxes even if it is difficult to quantify their magnitude.

6. In the discussion of Fig. 2, the authors state that the “curves for sensible (Fig. 2b) and latent (Fig. 2c) heat fluxes are rather well correlated with the PAR”. However, both sensible and latent heat fluxes set in 2 hours later. The friction velocity (Fig. 2i) starts to increase 1 hour after PAR starts to increase. These differences should be discussed in more detail because they characterize the morning hours when CO₂ and particle emission fluxes were observed. Maybe one could gain more insight if the data were classified on a day-by-day basis in classes when morning particle emission was observed or not. Are there differences in the onset of turbulence (sensible/latent heat fluxes, friction velocity) and the emission fluxes of CO₂ between days with and without early morning particle emission? Is the early morning particle emission associated with a certain wind direction?

7. One very interesting aspect of the manuscript is the discussion of the early morning emission fluxes of CO₂ and particle number, just when rapid changes in many variables occur. Have the data been tested for stationarity (e.g. Foken and Wichura, 1996)? Did CO₂ and particle emission fluxes occur simultaneously, i.e. has particle emission been observed on all days when CO₂ emission was observed? When looking at the 75 percentiles, there seems to be a similar feature present in the wet season as well yet less pronounced. Additional information about the atmosphere-canopy coupling

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regimes would be very helpful. While it is stated that the canopy is decoupled from the atmosphere during stable nighttime conditions, it would be a valuable addition if turbulence data could be evaluated to obtain coupling regimes at the site.

8. For calculating the diurnal cycles of the vertical particle flux, data from wind directions between 310 and 20 degrees have been discarded to exclude any impact from the diesel generator and house at K34. Is this based on some kind of footprint analysis?

9. Figure 5a and b present the same data and could be combined in one graph. In addition, I suggest to add error bars to Figure 6 and also to present the uncertainties of the particle deposition velocities in Fig. 8. In addition, some metric to represent the uncertainty in the presented relations of deposition velocity and friction velocity should be included in Eqs. 6 and 7.

10. Particle deposition velocities: Since both particle emission and deposition fluxes can occur and were observed, the term “particle transfer velocity” may be more appropriate instead of “particle deposition velocity”. The authors have used this terminology in their previous manuscript mentioned above. In general, it is difficult to compare studies of particle number fluxes because there is no convention on how to report average transfer velocities. This is discussed by the authors on p. 26900. When averaging upward and downward fluxes, the opposite signs of the flux values may lead to unfavorable averaging results. One suggestion is to separate deposition and emission fluxes, i.e. positive and negative particle transfer velocities, and report means or medians of the transfer velocity separately for deposition and emission periods.

11. The manuscript lacks observational data of the particle size distributions during the dry and wet seasons which is crucial for some of the presented discussions and conclusions. The authors discuss possible differences of the particle size distribution during both periods, but they don't present any direct measurements of the particle size distributions at the site. If such data is not available, it would be necessary to discuss the comparability of the studies cited for particle size distribution measurements in the

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Amazon and the present study. The influence of dry season biomass burning appears to be less important in the present study. On p. 26895, the authors state that “the Cueiras Reserve is located in an area of pristine rain forest where the direct influence of biomass burning is much lower than in Rondonia or other locations in the southern part of the Amazon rain forest. Even in the dry season, impact of biomass burning emissions is not very high at the Cuieiras Reserve, but can be observed most of the time”. This contradicts statements such as “during the dry season, when the impact from biomass burning is high” on p. 26904, line 21. Can you give an estimate of the contributions of the Aitken and the accumulation modes to the total particle number concentration during the dry and wet seasons at K34? Some more information about the particle size distribution would be a critical addition to the manuscript.

12. The dependence of particle deposition on stability is compared with a parameterization for sulfate particles by Wesely et al. (1985). Stable cases are discarded because emission fluxes prevail during the nighttime stable conditions, thus not representing the deposition process. This criterion for exclusion seems somewhat random. Emission fluxes may continue throughout the day but be masked by larger daytime deposition fluxes as stated on p. 26900. In this case, the observed apparent particle flux is always a superposition of emission and deposition fluxes, and the parameterization of the deposition flux will be influenced by a potential emission component. Taking this into account and the large variability of the presented fluxes, a straightforward dependence on stability such as the one given by Wesely et al. (1985) may not be expected.

Additional minor comments:

p. 26884, line 1-8: An estimate of the relative contributions of wet and dry deposition to aerosol removal from the atmosphere in the dry and wet seasons would be a valuable addition in this section.

p. 26892, line 8: Replace “what is happening more frequently” by “what is typically happening”. Since the median is the 50 percentile, “typical” appears to be more adequate

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than “more frequently”.

p. 26895, line 4: “Sect. 3.4.2.” should be changed to “Sect. 3.4.3.”

p.26895, lines 12-13: “. . .dry season particle concentration most of the time was three times higher. . .”: Suggest to change to “. . .dry season particle concentration was typically three times higher. . .”:

p. 26901, lines 1-3: For comparison, some typical values of deposition velocities over boreal forests (and maybe also other environments, e.g. marine environments) may be a valuable addition.

p. 26901, line 18: Eq. 7 slightly deviates from the relation of deposition velocity and friction velocity during the wet season previously published as Eq. 9 in Ahlm et al. (2009). What is the difference in data reduction and which of the two relations is recommended for further use?

p. 26903, line 19: “. . . it is obvious from Eq. (9)” should read “. . . it is obvious from Eq. (10)”.

p. 26904, line 1: remove second “on L-1”

p. 26914, Tab. 2: The median of the diurnal min of B_{Ce} concentration is missing and should be added.

References:

Ahlm, L., Nilsson, E.D., Krejci, R., Martensson, E.M., Vogt, M. and Artaxo, P. (2009) Aerosol number fluxes over the Amazon rain forest during the wet season. *Atmos. Chem. Phys.* 9, 9381-9400.

Foken, T. and Wichura, B. (1996) Tools for quality assessment of surface-based flux measurements. *Agric. Forest Meteorol.* 78, 83-105.

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Interactive comment on *Atmos. Chem. Phys. Discuss.*, 9, 26881, 2009.