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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Emission and deposition of accumulation and coarse mode particles in the Amazon basin" *by* L. Ahlm et al.

**Anonymous Referee #3**

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General Impression

The paper by Ahlm et al. presents size-segregated supermicron fluxes measured above a tropical vegetation canopy and derives some interesting conclusions on emission and deposition processes. It represents the third in a series of papers on a study, which arguably looks like the most successful attempt to quantify tropical aerosol fluxes to date. Thus the material is very worth publishing. The paper is mostly well written, with some suggestions to the English already made by the other referees. This paper mainly deals with the emission fluxes of aerosol (in the size range of the optical particle counter used) measured under clean conditions, while total particle number fluxes have been presented in two previous papers. A further paper draft with material on deposition fluxes of 0.25-0.45 um particles under more polluted conditions is con-



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tained in the PhD Thesis of the lead author (Ahlm, 2010), and presumably the plan is to publish this as a fourth paper (this was implied by the original version of this paper, but the reference appears to have been removed, so it is unclear whether the plan has changed). One of my main concerns about the paper is that the information of what is basically an integrated study is spread rather too thinly across too many papers, which makes it more difficult to provide a coherent, comprehensive picture of the emission / deposition processes at the site. I would therefore strongly encourage the authors to include the further material on the deposition into this paper, rather than splitting it off into a separate publication, for reasons that will become apparent below.

Major comments

Referees 1 and 2 have already made a number of good suggestions for improvements, which I will not reiterate here. However, I would like to expand on a few of them: (a) In disagreement with comment 1 of Referee #1 I believe that the drying of the sample stream has been the key to these measurements succeeding where others have failed. If the dryer is sufficiently effective for the OPC to measure the dry radius, the corrections implied by Referee #1 clearly do not apply. However, it would be worth showing (possibly through a simple modelling exercise or through a test with two dryers in series), how much of the water is likely to have been removed in the setup. (b) Related to this point is the fact that equilibration with water vapour is not the only potential source of artefacts on the measurements. Loss or uptake of semi-volatile aerosol pre-cursor gases (organic and inorganic) can equally result in particle growth or shrinkage that leads to spurious number fluxes for fixed-sized bins as e.g. pointed out by Nemitz and Sutton (2004) and Nemitz et al. (2009) for the NH4NO3 equilibrium. This should at least be noted in the revised manuscript. (c) Expanding on comment 4 of Referee #1, it is true that the setup measures the (bi-directional) net flux, which is potentially composed of emission and deposition. In order to derive robust source functions for the aerosol (Eq. 6) the fluxes should be corrected for deposition, unless this can be shown to be negligibly small. Similarly, for the quantification of deposition



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velocities, the 'contamination' by emission processes may need to be considered. In my mind, if emission and deposition were fully considered in the same paper a more thorough job could be done in correcting Eq. (6) for the deposition term. Incidentally, what is the effect of gravitational settling on Eq. (6)? Gravitational settling fluxes do not show up in the local turbulent flux measurement, but they will, to some extent, reduce the number of particles that reach the measurement height. Could the authors estimate the importance of this effect? It really depends on whether the authors want to derive the source function or the net flux from the canopy, although the former would be more informative. With reference to the literature on resuspension I support the use of U (rather than u*) to parameterise the emission flux. However, I am surprised that the relationship between U and u* (Fig. 11) appears to be quite variable at this site. What is the reason? (d) Like Referee #1 (their comment 11) I was left wondering how the results of the coarser particle fluxes tie in with the observed morning emission peak in total particle number fluxes (Ahlm et al., 2010), which are used to motivate this analysis (page 14015, lines 27ff). It appears that the temporal pattern in the total number flux is not consistent with the biogenic primary aerosol emission implied by the measurements here and thus, a different explanation needs to be found for this phenomenon, which has now also been reported for another tropical forest (Whitehead et al., 2010). The implications of the findings for the earlier publication need to be discussed.

Minor comments

Title: In the air pollution community the term 'coarse mode' usually refers to the size range 2.5 to 10 um, whereas this paper deals with particles in the size range 0.25 to 2.5 um. I would therefore suggest amending the title to bring into line with common terminology.

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