

Interactive comment on “Aerosol number fluxes over the Amazon rain forest during the wet season” by L. Ahlm et al.

Anonymous Referee #2

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Eddy fluxes of total aerosol number concentration were measured and interpreted as being controlled by deposition and entrainment processes. This is an excellent study and should be published if a few key revisions are made to the wording and the equations are redone.

The authors have done a very nice job of obtaining, screening and analyzing their covariance data. I am troubled, however, by two ways in which the discussion is presented when the terms "secondary aerosol particles"(or "natural" or "primary"; lines 29, 30, 32, 86, 494, 624, 649, 669, 693) and "advection" (lines 534, 639, 648) are utilized.

It seems to this reviewer that there is no evidence that the particles are primary or secondary here (or natural vs. anthropogenic) in that their formation mechanism is not able to be inferred other than by simply ASSUMING that the usual processes occur

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upwind. What the study does suggest is that local production of new aerosol (within the upwind flux footprint) is limited due to a lack of frequent upward fluxes but it does not show how the particles that often mixed downward with entrainment were originally formed. (There is no way to distinguish particle production reactions that occur upwind versus direct emission of particles upwind). All references to secondary or primary or natural ought to be removed from the text. This will not substantially limit the authors' conclusions that fluxes usually are downward to forests for this situation.

The last paragraph of the abstract is not useful and should be deleted.

The other misused term here is "advection", which the authors use very generally to imply transport into the region by the mean wind but in an eddy flux sense this is a specific term in the budget equation which, if non-zero, of necessity would imply that time change (i.e. non-stationary; storage; as analyzed in Fig.10 here) and/or change in flux with height MUST occur (see J.A.Businger in J. Climate Applied Meteorol. 25, 1101, 1986 and other work). IF there is a change in flux with height the measurements presented here have much less value because they are not representative of any larger area. Thus, "advection" is a term to use very carefully with eddy fluxes (see also G.Slinn, "Precip. Scavenging, Dry Depos....", 1983). Thus the statement in lines 144-146 is NOT correct when advection and/or time change (in the sense of the eddy flux budget equation) are important. The authors present no citation for this statement which is o not correct despite the many times that it is ASSUMED in order to simplify a study.

The authors make good points about how entrainment affects surface fluxes (e.g., Fig. 11 is quite interesting) and use effective means for evaluating local sources. Overall the data analysis and the interpretation of the influence of entrainment are excellent.

Equations 1, 4, 5, and 6 are garbled on the screen and when printed and must be redone in that they appear to be incorrect in their scrambled format available through the review process. One can guess that the authors used the correct equations through

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the way that the equations are garbled (somehow).

Minor revisions are needed in the following locations:

line# comment

83-84 be careful with wording here, entrainment was shown to be important not emission 94 "contribute" 126 add units for LAI 294-95 how were equations 1-2 used; were the corrections ever applied to the final data ? 296-302 what did co-spectra of heat and vapor fluxes show ? were they similar ? 377 the friction velocity is actually > 3 times as high in the day, not "slight" 399 give r^2 for this "correlation" 439 processes may be more numerous but not necessarily "More complex"; see line 664 443 "(other errors) are more difficult to quantify...but counting error dominates" ? You give no citation nor analysis to support this. What about hygroscopic growth 570-574 Also higher background reduces new particle nucleation compared to coating the existing particles with any new secondary organic material (due to surface area). 581 Held et al. 2006 citation is not in the reference list 622-24 Excluding periods with likely HIGHER anthropogenic influence does not mean that all anthropogenic influence is gone. Furthermore, many secondary particles trace their origin back to anthropogenic emissions like SO₂. This distinction is poorly made in this paper, if at all. 629-30 There is no way to separate deposition of natural from anthropogenic particles in this study as it is presented. Too many nearby anthropogenic emissions present a problem but what difference would there be between deposition of long range transport of natural or anthropogenic emissions (unless receptor modeling and chemical tracers could be somehow utilized) ?

688 A different equation is presented for V_d vs U^* here than in the abstract or text. Is an intercept needed or not ? what is the R^2 for this equation in either form ?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 17335, 2009.

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