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Interactive Comment

Interactive comment on "Aerosol dynamics within and above forest in relation to turbulent transport and dry deposition" *by* Ü. Rannik et al.

Anonymous Referee #2

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This is a useful and interesting manuscript. However, I have a few comments that the authors must address prior to the manuscript being considered for publication.

Major points:

1. Over-statement of the results: The statements about aerosol dynamics impact on particle concentrations relative to deposition (e.g. in the abstract 'can frequently exceed') need to be clarified to note if this is FOR A given size, for NUMBER concentrations and to note in these 10 simulation days most exhibited nucleation and FOR THIS SITE . . . all these facts are rather important to the global importance attached to this statement. Making these statement more specific and tempered will not devalue the manuscript but will avoid unwarranted claims. Equally this work does not prove that 'eddy covariance techniques do not generally represent dry deposition' I think it is

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hard to make that statement based on simulations of 10 days at a forest site in Finland. These statements in the abstract and elsewhere should be corrected. Equally the first sentence in the conclusion – the authors have not observed – simulations conducted using their model (which is based on some assumptions) that indicate. . ..

2. Insufficient testing of parameter space: Given this is a numerical experiment I am unclear why the authors do not explore more parameter space. Its useful to present a brief evaluation using 'real data' but why only do 10 days of simulations** – the time is not really relevant surely the point is to explore the realm of plausible situations. More exploration of the parameter space should be undertaken to examine generalizability. The following aspects of the initialization are unclear: - I understand the only data the team have access to are the 2m PSD but this is below the canopy and under weak turbulence it could be quite decoupled from the actual level where it is 'applied' i.e. at the upper boundary. This point is actually quite unclear in the manuscript but I believe a uniform PSD is applied throughout the vertical domain? (there is some (unclear) discussion on 19375. - The manuscript states the model is initialized with 'vertical profiles describing the atmospheric state' but no details are provided about what parameters are set based on which measurements. This should be clarified. - The initialization of the gas phase chemistry is not described. Neither is the mechanism. The authors should document this fully (perhaps in Supplemental Materials)

3. The authors do not provide any quantitative evaluation of the model. While this evaluation should naturally focus on the PSD, it could/should include other aspects – e.g. physical parameters that show the turbulence conditions are being represented (the plot of TKE extends to 1500m but surely most of the 'relevance' is for much lower heights? They plot a time series of <I GUESS> surface (or maybe top of the canopy) Latent heat and sensible heat flux but these plots (Fig 2) only really serve to emphasize the diurnal cycle (which is trivial to reproduce). A scatter plot would be more meaningful as would application of statistical model skill metrics (to all measured parameters – e.g. TKE must surely be quantified from sonics deployed at the site).

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4. The authors claim their results are ‘generality’ (generalizable) – but I think they are also very specific to the canopy structure. Clear documentation should be provided of that (e.g profile of LAD), and an analysis conducted to examine the sensitivity to it.

5. The model is based on a number of assumption many justifiable (but maybe not full justified) assumptions. BUT it should quantify the uncertainties on their estimates of the importance of the terms they include and those they neglect.

Minor points:

1. It’s strange to have a definitional statement (aerosol dynamics) in the abstract and even stranger when it is not linked to the previous and subsequent sentences – I suggest rewriting the abstract to improve it.

2. P19371 ‘leads us to the assumptions that’ I think this is really the PREMISE. I think its correct but a little bit imprecisely formulated. I would say that the vertical transport by turbulent eddies is rapid compared to other aerosol dynamics processes that impact the number particle size distribution, but as the authors suggest because other resistances to actual surface removal are slower the composite timescale for surface removal by dry deposition may under some circumstances be comparable to the time scales on which or process that act to modify the particle size distribution act. . . (i.e. we are not disagreeing but I think the formulation of the postulate could be tighter).

3. The standard of English grammar is not very high – this leads to some imprecise statements e.g.; ‘The time scale of turbulent transfer is the estimate of the transfer time within turbulent air layer.’ I think perhaps a careful reading and correction would benefit the manuscript.

4. P19377: The authors state ‘the correspondence was not exact’ (i.e. flux defined at canopy top is not = deposition) the authors say it indicates a complex relationship . . . – This is a ‘throw away’ statement – and it is associated with a reference to Figure 5a but looking at that figure I think it is still unclear how important the discrepancy is

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(or average and in the worst cases – both of which should be presented) or what the cause is.

5. A lot of the figures are time series or plots for individual time periods – I realize they are easy to plot but they are poor from a diagnostic perspective – The author should generate better, more synthetic diagrams.

6. Figure 3 the terms (e.g. 'Aer. Dyn.') should be defined in the caption.

7. Figure 7 why is the transport 'clipped' the authors should note values not shown. This figures is quite surprising on many levels – one would not expect such large variations from Dp to Dp. . ..What does it look like when an average and sd are plotted for the entire simulation period?

8. Figure 8 and 9 why is there such a large discrepancy in the PSD in frame (a)? – I guess this is the model being reinitialized but it does appear to indicate the model is drifting a long way from 'reality' –doesn't that give pause to some of the more sweeping generalization statements they make in the manuscript?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 19367, 2015.