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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Formation and growth of nucleated particles into cloud condensation nuclei: model-measurement comparison" *by* D. M. Westervelt et al.

Anonymous Referee #1

Received and published: 14 May 2013

General Comments:

The authors present a novel approach for model-measurement comparisons which introduces the use of several new metrics relevant to particle formation and growth. Using a global aerosol microphysics model and measurement data from 5 locations, the authors evaluate their new metrics, concluding that global models are appropriate tools for assessing the contribution of processes like nucleation to particle size and number. The paper is well written, interesting and certainly within the scope of ACP; I would recommend publication, following clarification on the below, minor issues.

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Specific / Minor Comments:

p8336, line 5: Rephrase “With increasing aerosols. . .” to be more specific?

p8338, line 19: since you go on to use this tuning factor for your ternary nucleation simulations some discussion of the value used might be helpful to the reader

p8339, line 17: “. . .. . ..self-coagulation is much smaller than. . .” rephrase to clarify what you mean by smaller than, i.e. less important?

The model has relatively coarse horizontal resolution, do you linearly interpolate across adjacent grid-cells to the observation locations? Perhaps add this at p8346, line 15-18 if so.

p8353, lines 20-28: both nucleation mechanisms seem to under-predict the number of event days during April at Hyytiala, any ideas why?

p8354, line 3: it looks like the near exact prediction in April and May at Pittsburgh is only with the ternary mechanism?

p8356, line 23-25: true, but the activation median is even closer (than ternary) to the observed median?

p8357, line 9: “. . .. . .small positive bias. . .. . .”, based on what? The median and mean CoagS values are lower than the observed value

p8358, line 11-12: Would be useful to give the relative growth rate numbers somewhere in this paragraph; from the plot it’s difficult to compare the relative contributions of organics and sulfuric acid in the model v. measurements, since the total growth rate is different

p8358, line 23-24: You say here that this approach “. . ..should help isolate individual processes biasing model predictions.” Have you been able to do this in this study? It would be useful here to give an example of how your approach has allowed this, if it has, I’m not sure...

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p8375, Figure 1: Nice figure, does a good job of explaining the competing processes here.

p8384, Figure 10: Panel A is also a CDF?

p8386, Figure 12: You could perhaps add additional lines at (or shading between) +0.3 and -0.3 to emphasise the region that indicates a factor of 2 difference to aid the reader. You could move the explanation that is given in the text (p8359, lines 9-11) to the figure caption, on its own this figure is difficult to interpret.

Technical Comments:

p8344, line 25: Is the Riipinen 2010 reference correct? should it be 2011?

p8345, line 10: Is the Spracklen 2009 reference correct? should it be 2010?

p8353, line 14-16: this is just the ternary model?

p8354, line 12: insert "," between Hyytiala and Pittsburgh

p8355, line 24: I think these are the mean values? rather than the medians

p8361, line 2: replace "they" with "it"

A few acronyms are used, e.g. CS for condensation sink, SP for survival probability: would be better to define the terms and then use consistently throughout the text, rather than swapping between the full term and the acronym.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 8333, 2013.