

[Interactive
Comment](#)

Interactive comment on “Intercomparison and evaluation of aerosol microphysical properties among AeroCom global models of a range of complexity” by G. W. Mann et al.

Anonymous Referee #1

Received and published: 10 December 2013

Review of “Intercomparison and evaluation of aerosol microphysical properties among AeroCom global models of a range of complexity” by Mann and many others.

This paper is an initial comparison of the global aerosol microphysics models in the AeroCom comparison. The authors approach the comparison by evaluating the group of models as a whole, rather than focusing on individual models, which would make for a cumbersome paper. The variability between the middle-2/3s models is particularly informative in this paper.

A considerable amount of work went into the comparisons in the paper, so the lead

[Full Screen / Esc](#)

[Printer-friendly Version](#)

[Interactive Discussion](#)

[Discussion Paper](#)



authors should be commended. The paper is very useful for understanding regions and species where the models need improvement. I feel that it should be published once several issues have been addressed.

Throughout: The discussion and figures jump between diameter and radius (seemingly randomly). For example, the cutoffs for N30 etc are in diameter while the size distributions are plotted as a function of radius. This may likely cause confusion for those readers integrating size distributions by eye (I frequently do this). I find it tedious to switch between them within a paper, I highly recommend using only diameter.

P30852 and Table 1: GEOS-Chem-APM is a hybrid microphysics scheme. Sulfate and SOA is sectional, POA and BC is modal, and sea-salt and dust is sectional (but different sections from sulfate and SOA). http://wiki.seas.harvard.edu/geos-chem/index.php/APM_aerosol_microphysics describes the scheme fully. It is possible that the Albany folks did something different for AeroCom, but I've never seen an APM paper with size sections for all species. The Yu and Luo (2009) paper that is cited as the APM reference uses this hybrid scheme.

P30854: I don't think Dthresh is defined anywhere.

Figures 1 and 2: Please make it clear that panels a and b are the means and c and d are the diversity. Please say in the caption that the diversity is a ratio (you say this in the text, but many readers will skim figures).

Figure 3 and 4: Are these STP or ambient concentrations? This need to be specified (will make more than a factor of 5 difference in the concentrations in the UT).

P30868 L20 (and elsewhere): What is "b"? Is it the bias. If it's bias, is it in log space? Is it normalized? Please define it clearly, later you use "b" but there is less context to figure out that it is probably a form of bias.

P30871 L5: It took me a long time to figure out that Ispra is "JRC" (still not positive). I'm not sure why the full name and 3-letter abbreviation have nothing in common. Would

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

it be possible to always put the 3-letter abbreviations in parentheses when discussing the sites throughout the text?

P30871 L28-29: “...only one of the models includes boundary layer nucleation...”. Only one of the models includes “activation-type nucleation” or “kinetic-type nucleation” that have been confusingly referred to as “boundary-layer nucleation”. The ternary and IMN simulations will also have nucleation in the boundary layer (and the ternary schemes have been shown to be better at predicting the occurrence and non-occurrence of nucleation and the magnitude of nucleation rates [when scaled] than activation nucleation [see Westervelt et al., ACP 2013 and Jung et al., AS&T, 2008]). Thus several models have nucleation in the boundary layer. Activation and kinetic nucleation schemes need to stop being referred to as “boundary layer nucleation” as if they are the only schemes that predict nucleation in the boundary layer; it leads to confusion such as in this sentence in the manuscript.

Figure 14 and 15 and section 3.2.3: I’m not sure that these add benefit to the paper since the distribution statistics (e.g. N10, 30 100) at the same sites are given in the previous figures. If you wanted to make the paper a little more sleek, I recommend removing these figures and section 3.2.3.

P30874 L27: I think “month” should be “season” here.

P30877 L14: “concentrations” is misspelled.

Figures 17 and 21 and P30878 L5: Figure 17 is ambient concentrations, and Figure 21 is STP concentrations (thanks for labeling these here). Also, the y-axes are different (pressure vs. altitude, respectively). Please make these consistent (e.g. both STP with pressure axes). I believe these inconsistencies between the concentrations in the figures leads to authors thinking that there are bigger differences between figures than there are. For example, at P30878 L5, the authors claim that UT concentrations above the north pacific reach 5000 cm⁻³ (in Figure 21), compared to only 800 cm⁻³ above Europe (Figure 17). However, at 200 hPa, the difference between STP concentrations and

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

Interactive
Comment

ambient concentrations are about a factor of 4, so the concentrations measured over Europe are more like 3000-3500 cm⁻³ when corrected to STP, which is much closer to the concentrations over the north Pacific. Thus, while some of the difference may be attributable to differences between condensation sinks between continental and oceanic regions, the differences are not as large as currently claimed in the manuscript.

P30882 L6-8: The authors claim that models have a low variability in CCN because of the low variability in N30, but there is a higher variability in N100. Whether N30 or N100 is more representative of CCN very much depends on the situation.

P30883 L22: Ammonium nitrate has a high-enough volatility that it tends to stay close to equilibrium as it condenses (i.e. quasi-equilibrium growth <http://www.tandfonline.com/doi/abs/10.1080/02786826.2012.679344>). This means that its size-resolved condensation is proportional to mass (rather than condensation to the Fuchs-corrected surface area as low/non-volatile species, such as with sulfate and low-vol organics). Thus, unless nitrate is reacting in a way to make it less volatile than ammonium nitrate (e.g. organic nitrates perhaps), it will have little effect on nucleation and growth (e.g. see the “thermodynamic” cases in <http://www.atmos-chem-phys.net/11/3865/2011/acp-11-3865-2011.html>).

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 30841, 2013.

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)