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Geoscientific Model Development Discussions

Interactive comment on "Description and evaluation of a new 4-mode version of Modal Aerosol Module (MAM4) within version 5.3 of the Community Atmosphere Model" *by* X. Liu et al.

Anonymous Referee #2

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The manuscript presents MAM4, an extended version of the three-mode MAM3 aerosol scheme incorporating an additional externally-mixed mode for primary carbonaceous aerosol. This allows it to represent the delayed transition to the hydrophilic state via ageing processes, but without the full complexity of the seven-mode MAM7 scheme. Improvements to the aerosol distribution are well demonstrated, and also compared with those from increasing model resolution.

The manuscript is well presented, based on sound methodology and shows clear results. Subject to the comments below, I would certainly recommend it for publication in GMD.

1 General comments

Since MAM4 is positioned between MAM3 and MAM7 in terms of its complexity, it is a pity that it is only compared against the former. An additional run with MAM7 in set 1 would demonstrate how much of the improvement in MAM7 is achieved by MAM4 with its lower computational cost.

For the set 2 experiments, the model is constrained by the YOTC analysis. Does this cover the time period of all the observational campaigns used, in order that these evaluations can be performed on the correct year? If not, how do the authors deal with interannual meteorological variability when comparing simulations for one year with observations for another?

Similarly, could the authors please clarify the temporal nature of the biomass-burning emissions in particular? These are highly variable, and there is likely to be a large difference between using climatological or hindcast-style "correct year" emissions when comparing to specific campaigns.

In the budget analysis presented in Section 4.2, is it possible to quantify the interannual variability in these budgets and thus to determine whether or not the differences are statistically significant?

When comparing the model to the aircraft campaigns in Figures 9–12, how is the model sampled in space and time to match the observations? (Is it interpolated to the flight time and location, or are e.g. monthly means over some region used? This will affect the sampling error to be expected in the comparison.)

2 Detailed comments

Page 8344, lines 3–5. If the models compare better with ground-based sun-photometer measurements than with satellite retrievals, doesn't this suggest that the poorer comparison with satellite might be due in part to either retrieval errors or collocation/sampling issues between models and observations, rather than deficiencies in the models?

Page 8344, lines 6–9. Hydrophobic and water-insoluble are not quite the same thing. Insoluble materials may nevertheless be hydrophilic ("wettable") and thus act as CCN via adsorption, although this is rarely treated in models. Also, "and are not able to nucleate cloud droplets" is unnecessary – this is what "cannot serve as CCN" means.

Page 8345, line 20. Remove hyphen in "high latitudes" and insert "the" before "northern hemisphere".

Page 8346, line 21. Please explain how homogeneous nucleation might be affected by aerosol particles. Unlike for heterogeneous nucleation, this is not clear.

Page 8348, line 23. Change "standard-alone" to "stand-alone" and explain what is meant by this. I presume an uncoupled atmosphere-only simulation with prescribed SST and sea ice?

Page 8349, line 23. By "specified dynamics" do you mean what is often referred to as "nudging" (i.e. Newtonian relaxation of model fields to the (re)analysis ones)? It's probably worth either using the term "nudging", or explaining how the technique differs.

Page 8350, line 9. Delete "the" before "comparison".

Page 8350, line 12, and captions to Figures 2–5. "Latitude and longitudinal" is grammatically inconsistent. I'd suggest "zonal and meridional" or "latitudinal and longitudinal" instead.

Page 8351, line 6. Although primary carbon particles may not act as CCN, they are still subject to impaction scavenging by cloud droplets, ice crystals and falling precipitation, leading to wet deposition. Please clarify if this process is included in the model or not.

Page 8351, lines 28–29. Why is this of importance for aerosol–cloud interactions in particular?

Page 8352, lines 6–11. Please justify, quantify and show evidence for, the difference between SD and free-running simulations being due to a convectively less stable atmosphere.

Page 8352, lines 17–18. This gives the impression that dry deposition overtakes wet deposition as the dominant process; please clarify that wet deposition remains very much the dominant sink even at 8 mono-layers, although dry deposition becomes a larger secondary sink.

Page 8353, line 23. Inserting "absolute" before "wet removal sinks" would make it clear how these statements are consistent.

Page 8354, lines 7–8. A citation of Schwarz et al. (2013; 10.1002/2013GL057775) is probably in order for the HIPPO1–5 SP2 observations.

Page 8355, lines 3–5. A similar attribution of excess BC in the upper troposphere to the relationship between convective transport and scavenging has been done for other models, notably HadGEM3–UKCA (Kipling et al., 2013; 10.5194/acp-13-5969-2013).

Pages 8356–8357, and/or Figures 9 and 11. Please add references for the aircraft campaigns used, where available.

Page 8358, line 7. Should be either "which however weakens" or "which, however, weakens".

Page 8359, lines 23–25. Citation or evidence for these deficiencies in the emission inventory?

Page 8360, lines 14–16. Probably worth using the AR5 terminology explicitly, i.e. either "ERFaci" or "ERFari+aci", depending which this is.

Table 1 caption. Try "number of ageing mono-layers set to 8", and "...with MAM3 is run for the comparison".

Figures 2, 4, 6, 7–12. Font sizes are very small, although this may only be due to the reduction required to fit in discussion page layout. Please check that labels etc. will be easily legible in the final paper.

Figure 3. Using the same colour scale for a difference plot as for the absolute burdens is confusing. Also, relative difference ($(x_1 - x_0)/x_0$) on a logarithmic scale seems strange. Either relative difference on a linear scale, or ratio (x_1/x_0) on a logarithmic scale would be easier to interpret.

Figures 7–12. The standard-deviation shading looks very odd, often being highly asymmetric and reaching down to zero. It's also not described in the caption of Figure 7. This is probably the result of using arithmetic standard deviations on a logarithmic scale – on such a scale, and for a quantity which roughly follows a log-normal distribution, the geometric mean and standard deviation would be more appropriate (or alternatively median and interquantile range). Including both means and medians on the plot doesn't seem to add much, and makes them quite cluttered.

I'd suggest sticking with one, and removing the other, unless some important conclusion relies on the distinction.

Interactive comment on Geosci. Model Dev. Discuss., 8, 8341, 2015.