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Geoscientific Model Development Discussions

Interactive comment on "The multi-scale aerosol-climate model PNNL-MMF: model description and evaluation" *by* M. Wang et al.

Anonymous Referee #2

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General Comments

This paper describes improvements made to the "Multi-scale Modelling Framework" (MMF) which essentially modifies a global climate model to have its cloud properties and processes to be as simulated by an embedded cloud-resolving model rather than by conventional parameterizations.

The paper describes three main improvements on the "original MMF framework" (as in Khairoutdinov et al, 2008) in CAM v3.5: i) the use of a two-moment modal aerosol

scheme in CAM, ii) the use of the explicit-cloud parameterized-pollutant (ECPP) approach to use the CRM-scale cloud information to affect the gas/aerosol simulated on the climate model scale, iii) the use of a two-moment cloud microphysics scheme in the CRM rather than the original single-moment scheme.

This improved version (referred to as the PNNL-MMF) represents a novel and promising way to use the MMF "super-parameterization" approach to improve the representation of aerosol-cloud interactions in global climate models.

As well as clearly describing the developments to the CAM model, the paper provides a comprehensive documentation of how the global aerosol simulated by the improved model compares against other global models and evaluates a number of key quantities against an impressive collection of key observational datasets suitable for assessing global aerosol models. The description of the improved MMF implementation within CAM is interesting and the evaluation of the improved CAM is comprehensive and, as such, the paper is certainly suitable for publication in Geoscientific Model Development. The paper is in good shape and reads well throughout. The introduction is appropriate and the model description is comprehensive and clearly explained.

My main criticism however, is that, although the paper presents evaluation of the improved model against an impressive number of observational datasets, there is no explanation of how much better the improved CAM is compared to the current standard CAM5 version without the MMF approach. The final sentence of the abstract states that "the MMF version of CAM5 simulates aerosol fields as well as conventional aerosol models". I guess one would hope that it might simulate the aerosol better or more realistically. Here, and elsewhere in the paper, the benefits of the multi-scale approach needs to be stated more clearly.

For instance, are the PNNL-MMF-CAM5 simulated size distributions and size-resolved number concentrations in Figures 17-20 better or worse than those with the standard CAM5?

Without this information, the reader can only see this new framework as a whole without being able to assess whether the use of the CRM information improves the model or not. Or which aspects of the model are improved or made more realistic by this new approach.

From the description in the paper, the Liu et al (in prep, 2010) paper will describe runs of the standard CAM5 model that could be included in this analysis and used to specifically examine where the use of cloud-resolving scale statistics changes the model predictions. If it is straightforward to do, I would ask the authors to consider adding this standard CAM5 simulation as a reference model line in as many of the figures as possible — this ought to be possible for many of the Figures and would greatly improve the paper, and help to understand the impact of the new approach on the simulated aerosol properties.

Another aspect of the paper that needs improving is that, although in some of the comparisons to observations (Figures 7-14), there are values for the correlation coefficient R given, in many of the Figures there are no statistical measures of the model comparison to the observations at all (e.g. Figure 21).

Also, even in the Figures which do have R values, I recommend that there should also be added a measure of the model normalised-mean-bias or standard-error since just the R value does not constrain the skill of the model very well.

I recommend that an extra Table (or perhaps 2) be added to the paper which give the R and normalised-mean-bias/error values for each of the observational datasets used to assess the skill of the model.

Related to this, I also recommend that, if possible, R and bias/error values are given for the standard CAM5 simulation from Liu et al (in prep, 2010) and then the reader can see exactly how the model has improved or otherwise with the incorporation of the new MMF development.

However, overall this is a nice paper and I recommend it is published once the issues I have raised have been sufficient addressed.

Specific Comments

1) Abstract – the 2nd part of the abstract which summarizes the skill of the model uses several times the phrase “are in reasonable agreement” – the authors should give some kind of quantitative measure here in each case in terms of correlation and/or mean bias values.

2) Introduction – pg 1629, line 9 – the sentence "The MMF models have been shown to improve climate simulations in several important ways (...several refs...) " should be re-written. State very briefly exactly they key ways that the climate models are improved by using the approach.

3) Section 2.1 – pg 1631, lines 23-24 – please explain what mechanism(s) are used to represent Aerosol nucleation in the model. Since the paper includes several comparisons to observations of the size distribution in section 4.2 this should be explained here. On page 1653 there is reference to a boundary layer nucleation mechanism being included in the model – but this is not described anywhere in the paper – please include a sentence in section 2.1 on which binary nucleation mechanism is used and the approach (and coefficients) used for boundary layer nucleation.

4) Section 2.4, pg 1643, lines 3-9: Here, and at several other points in the paper, the phrase ““Gas phase SOA” is used — and indeed an acronym SOAG is used to describe this. I would suggest the authors not use the phrase “Gas phase SOA”. Although one could argue that technically, since the term “aerosol” represents both the particle and gas-phase, the phrase “gas-phase SOA” does make sense, I feel this terminology is confusing for the reader because it suggests that SOAG is the gaseous part of a semi-volatile aerosol, whereas in fact all the “SOAG” is condensing into the particle phase. I would suggest that the authors remove reference to “SOAG” and instead either refer to the gas with a different name reflecting the fact that it is an condensing gas phase

organic species (e.g. CONDORG) or else re-phrase so that there is no reference to the SOA in the gas phase (and remove the top-part of Table 4 which refers to SOAG) except in section 2.4 where it should be explained that this is the technique for producing SOA in the model.

5) Section 3.1 – pg 1644, lines 24-25: the text refers to the larger fraction of the sulphate burden above 5km than other AEROCOM models. To what extent is this a product of the MMF approach for the scavenging and to what extent is it a general feature of the CAM model (i.e. without the MMF approach)? There should be some reference here to the values in the standard CAM model – again the reader needs to have a better handle as to how the MMF-CAM framework compares with standard CAM – can values from Liu et al (in prep., 2010) be added to Tables 1-6? Also this is referred to in the Summary (page 1658, lines 24-26) and it needs to be much clearer in the paper whether this is a problem with the way the MMF approach has been implemented or a general problem with CAM5.

6) Section 4.2 – pg 1654 lines 3-5 – the authors attribute the "difficulties in simulating the monomodal size distributions in the free troposphere" to "the modal representation of the aerosol size distribution in the MMF model". The authors should clarify what is meant here. Firstly I presume they are referring to the treatment of the aerosol in CAM5. If so then they should state that it is in that model rather than "the MMF model" as it is what is used in CAM5. Secondly, there are different implementations of a "modal" aerosol in different models – for instance Stier et al (2005) and Mann et al (2010) use 7 modes which include a separate nucleation mode (representing particles smaller than 10nm diameter) in addition to an "Aitken mode" to represent particles in the 10nm-100nm size range. In this study, only 3 modes are used with only 1 mode representing particles across both these size ranges. Indeed the authors explain in section 2.1 page 1631 lines 13-21 how they use the 3-mode approach rather than a 7-mode approach for computational efficiency reasons. To what extent are the deficiencies here a product of the use of the simplified (3-mode) modal approach rather

than the detailed (7-mode) approach and to what extent is it a problem with all modal approaches using constant mode-widths? I realize the authors may not be able to answer that question in the revised paper but there should be reference to the different possible cause here and that it may not be a general problem with the modal approach, which may be inferred by the reader here. Again, reference to how well the standard CAM5 simulation performs against these observations would help here.

7) Section 5 – pg 1658 lines 15-22 – the authors refer to the accumulation mode being underestimated and the Aitken mode being overestimated in the free troposphere. The only possible cause given in this section is that the SO2 is over-estimated. Isn't is also possible that the scavenging approach being implemented here may be the cause? Or couldn't it also be possible that the simplified 3-mode treatment of the aerosol is causing problems representing nucleation with only 1 mode covering sub-100nm particles? Also, as referred to in the last part of the section 5 on pg 1659 (lines 4-10), could the fact that the low cloud are biased be affecting the processing of Aitken mode particles into the accumulation mode? These (and any other) possible causes for this bias should be mentioned in this section of the conclusions rather than suggesting it is likely only a problem with the SO2.

Minor Comments & Typos

1) Abstract – pg 1626 lines 5,6 and afterwards: "Global Climate Models (GCMs)" – the acronym GCM is generally accepted to refer to "General Circulation Model" rather than "Global Climate Model". I suggest that, if the authors are specifically referring to the climate model, they avoid the GCM acronym as it can be confusing to the reader.

2) Section 3.1 – pg 1643, lines 23-24 – Sentence beginning "For gas species, a range of results from other models. . ..." mentions Liu et al (2005) twice in succession – and so does the caption to Table 1 – suggest just to state "values listed in Liu et al (2005)" – for a while I thought it must be a typo but I now see what you mean here – but suggest to reword this.

3) Section 4.2 – pg 1652, lines 25-28 – The text explains that the model data is sampled over the same regions as the 15x15 degree gridded observational data – but there is no explanation of the temporal sampling here – is this an annual mean

4) Section 4.2 – pg 1653, line 3 and pg 1654 line 22 and pg 1658 line 19– Aikten à Aitken.

5) Section 4.2 – pg 1653, lines 22-23 – the authors refer to the "depletion of accumulation mode particles in the boundary layer" – I presume the authors are referring here to scavenging by wet removal – please clarify what is meant by "depletion".

Interactive comment on Geosci. Model Dev. Discuss., 3, 1625, 2010.