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Geoscientific Model Development Discussions

Interactive comment on "Toward a minimal representation of aerosol direct and indirect effects: model description and evaluation" *by* X. Liu et al.

Anonymous Referee #1

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General comments

The authors present the new aerosol module MAM implemented in the Community Atmosphere Model CAM5. MAM simulates aerosol size distribution, mixing state of carbon, aerosol microphysical and chemical processes, and aerosol optical properties, using a modal approach to describe the aerosol size distributions. Two version of MAM are presented in this manuscript, with 3 (MAM3) and 7 (MAM7) modes, respectively. The paper presents the model description and the model evaluation. Additionally, it presents the comparison between MAM3 and MAM7 and some sensitivity tests.

This is an excellent work and the whole study deserves to be published. It is scien-

tifically relevant, mostly clearly written (some parts sound a bit awkward, and would require to be read by a native English speaker), and the details are enough to make the work reproducible. However, I strongly suggest dividing this paper in at least two smaller ones, or to move a large part of it to the supplementary material. Every section is interesting and relevant, but the whole paper is definitely too long (114 pages!). I suggest dividing the paper into a first one that includes the description and evaluation of MAM7 (with the sensitivity studies in the supplementary material), and a second one that introduces MAM3 and its comparison with MAM7.

Specific comments

- Title: I think that the title is not very fitting. The authors are not really going into the details of direct and indirect effects; they are presenting a new aerosol model. Of course, this model can be used for climate applications, but they are not presented here in details. I would rather choose a title that includes the name of the model, because the goal of the paper is to provide a reference for the climate studies that will use that model. If I have misunderstood the goal of the paper (it might as well be, it is difficult to find the message in such a long paper!), the introduction should be more focused and let the message come through more clearly.
- Abstract: the abstract is too long. I suggest shortening the description of the results. As it is now, it looks more like a "conclusion" section.
- Introduction: I think you should add a sentence, at the end most probably, where you state exactly what you do. I know that you are actually doing a lot in this paper, but the message get lost. If the goal is the description and evaluation of a new model, than you should clearly state that. If the goal is to investigate the effects of model simplifications on aerosol lifetime, than you should write it clearly. I think you do both these things in your paper, so I stress again the need

of splitting this manuscript in two shorter manuscripts. I think that the author's results would gain much more visibility.

- Page 3489 L10: I think that the limitation of the bulk model is more not to be able to simulate the time evolution of the aerosol size, not really the difference between ocean or land aerosols and surface or upper troposphere aerosols. You could imagine a bulk model that applies different size distribution in ocean and land grid boxes, or above and below the troposphere.
- Page 3489 L23: I would include also Whitby and McMurry, (1997). I would also add "e.g." before the reference list, since there are many more models using the modal method.
- Page. 3490 L2: Again, the modal method has been implemented in many more models, as in the NASA GISS (Bauer et al. 2008) and in ECHAM4 (Lauer et al., 2005). Either you write more models, or you add an "e.g."
- Page 3490 L9: what do you mean by "few types"?
- Page 3490 L10: I would add some references, here. It's not really typical, yet, to simulate explicitly the mixing state. I would mention Aquila et al. (2011), Seland et al. (2008), and Wang et al. (2009)
- Page 3490 L22: For AeroCom, I would rather cite Textor et al. (2006)
- Page 3493 L3: Do you have any reference for the values of the standard deviations that you chose?
- Page 3493 L9: primary carbon particles from fossil fuel combustion can be pretty small (Dentener et al., 2006). Don't you think that merging them with the accumulation mode could move the accumulation mode to too small radius?

- Page 3497 L8: I cannot follow this. Why a factor 1.5? Where does the factor come from?
- Page. 3499 L1: you should explain the ageing criterion better. You take the diameter of the mode, and calculate how much mass of sulfate is required to cover a whole particle that large with 3 monolayers. Have I understood it correctly?
- Page. 3499 L8: "the SOA that condenses in a time step is scaled by its lower hygroscopicity to give a condensed sulfate equivalent". I do not understand what you mean. How do you scale it? If you have x grams of SOA with hygroscopicity 0.1 you just multiply x by 0.1?
- Page 3499 L15: Are you speaking about intramodal or intermodal coagulation? Do you neglect also the intramodal coagulation in modes larger than the accumulation mode? About intermodal coagulation, do you consider the coagulation with fine sea salt and dust modes? They look in the same size range of the accumulation mode. If you do not consider them, did you estimate how much is the error created by neglecting them? I think you should cite Binkowski and Roselle (2003)
- Page. 3499 L25: reference for CMAQ?
- Page. 3500 L5: how did you choose the k? I cannot find them all in Petters and Kreidenweis (2007). Did you try any sensitivity tests changing the k of dust?
- Page. 3500 L16: what is the UW parameterization?
- Page 3502 L1: I do not fully understand this. In CAM5 you must multiply somewhere for the aerosol concentration in cloud droplets, otherwise you could remove more particles than what you have, am I correct? So it should be like in previous versions of CAM. Where is the difference?

- Page 3502 L5: a solubility factor of 1 means that if the aerosol is in a cloud drop, and this drop precipitates (through the multiplication by the cloud water loss rate), then the aerosol is removed. If the solubility factor if 0, then the aerosol is not taken in the cloud drop. Would a solubility factor different from 1 or 0 make physical sense? Is it a factor or a switch?
- Page 3502 L23: in the case of below-cloud scavenging, is the solubility factor the probability to stick to a falling drop? Is it the same factor as for in-cloud scavenging?
- Page 3502 L28: how do you treat ice scavenging?
- Page 3503 L4: why do you write, "Layers above the surface"? Do you calculate the settling only in the lowermost layer or also in some layers above?
- Page 3507 L12: shown by whom, Pincus et al, 2003?
- Page. 3510 L 7: I think that the evaluation with the observations should be moved here, before the results of the aerosol distributions and budgets and comparisons with other models. In the end, it is more important that your model reproduces the observations than other models.
- Page. 3511 L21: I don't understand why, if BC has a very low hygroscopicity (tab. 3), it should be scavenged away as POM. Did you calculate the time-scale of ageing from primary BC to accumulation mode? Riemer et al. (2004) found it of a couple of hours in polluted areas: do you have similar results?
- Page 3511 L25: why do you have higher MAM7 concentration? Why is there a negative difference in South Africa?
- Page 3512 L22: "dust emission is often produced by frontal system". Do you have a reference?

- Page 3513 L1: As above, I don't understand this. BC should have a wet removal lower than POM because of lower hygroscopicity. Or is it that in MAM3 you use the same k for all species in one mode?
- Page. 3514 L1: Do you mean that in these regions there are few other particles available for condensation, and therefore there is nucleation has no competition? How are the concentrations of nucleating gases in these remote regions? Shouldn't they be relatively low, too?
- Page 3514 L4: why are H2SO4 concentrations higher in MAM7? I thought the difference between MAM3 and MAM7 was only in aerosols. Or is it because of the different nucleation scheme?
- Page 3514 L15: contribute to what? The % of primary carbon that you report is the contribution to total carbon or total aerosol? Why don't you write it as an absolute value? Also, is it a fraction in mass or in number concentration?
- Page 3516 L23: isn't the lifetime that you report actually longer than in the literature?
- Page 3518 L4: is this % reported in table 9? I cannot find it. Also, higher than what, AeroCom?
- Page 3518 L8: do you mean that CAM5.1 has higher precipitation formation only in some areas? If so, could you elaborate a bit more on this?
- Page 3518 L26: Why is the wet removal rate so much larger? Is it a matter of unrealistic hygroscopic coefficient?
- Page 3519 L5: how do you justify the faster ageing of industrial vs. biomass burning BC? Is it because industrial BC is smaller (Dentener et al., 2006) or because of the different amount of ageing factors emitted in industrial and bb areas? You use the same radius for bb and industrial BC, correct?

- Page 3521 L13: from my understanding, the wet removal is then the weak point of the model. Am I correct?
- Page 3522 L1: Aquila et al., 2009 shows the same profiles for EMAC/MADE-in. The model improved very much in the free troposphere after the implementation of a better ice scavenging. How is the ice scavenging parameterized in CAM5.1? Could that be the reason of the BC overestimation?
- Page. 3522 L24: if Fig. 17 is here only for reference, I think this is a good candidate for the supplementary material. In general, you show several figures for each species. This is great, because it you made a really through evaluation, but makes the paper quite heavy. I would keep one comparison per species, and move the rest to the supplementary material.
- Page 3524 L11: higher than what? Also, do you mean in the model or in the observations?
- Page. 3529 L5: you've really done a great work with the evaluation! Do you think there is a way of evaluating the mixing state of primary carbon, too? I know that there are not many observations, but I am thinking about Pratt and Prather (2010), for instance.
- Page. 3529 L15: I would move this section to the supplementary material.
- Page. 3530 L 25: are you speaking about MAM7-ageing or MAM7-k? Why does a lower k for POM produce only small changes? Because there is so much condensing material that also a stricter criterion for BC ageing can be easily satisfied?
- Page 3553: the size distribution for forest fire and domestic/energy etc. are the same? If so, then you need a b in the first line, right? Also, what does intermediate value mean?

- Page 3554: I don't understand table 2: they are % of what? And how much is the total? Why is Big Alkanes twice?
- Page 3555: I would put table 3 and 4 together
- Page 3557: is in MAM3 all BC is hydrophilic? You do not even have two tracers, for hydrophobic and hydrophilic BC, respectively, and an exponential decay?
- Page 3568: I would add a column with the AeroCom or the observed values, too
- Page 3570: I would add the typical size ranges of the modes to the figure (you do not have a hard cut-off between modes, right?)

Technical corrections

- Page. 3488 L14: isn't it cloud CONDENSATION nuclei?
- Page. 3488 L19: The "however" is not really fitting here
- Page. 3492 L 10: add a comma after "sea salt"
- Page 3492 L18: add the number of the sections to which you refer.
- Page 3493 L3: Add the values of the standard deviations in the text, too.
- Page. 3493 L13: "Although dust is much less soluble than sea salt, it readily absorbs water (Koretsky et al., 1997) and activates similarly as CCN (Kumar et al., 2009), particularly when coated by solutes like sulfate and organic, and so it is likely to be removed by wet deposition almost as easily as sea salts, so this is unlikely to introduce substantial error into our simulations;" This sentence should be rewritten in a nicer and clearer way.
- Page 3493 L22: report standard deviations in text, too.

- Page. 3495 L 15: the units should be after each number.
- Page 3519 L13: you don't need a reference to table 9, since it's the table that you are describing.
- Page 3559: use "-" in the AeroCom column, as in table 6
- Page 3572 and ff: nearly all figures should have a larger font.
- Page 3573: I would change the scale and choose a different one for each plot

References

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