

Responses to 1st Referee's 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 scientifically 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.

→Reply: We thank the reviewer for the encouraging comments.

We agree with the reviewer that the whole paper is too long, although it is a model development paper. Following the reviewer comment, we moved a large part of the paper to the supplementary. However, we did not split the paper into two shorter ones in the way the reviewer suggests. Much of the comparison between MAM3 and MAM7 is in the evaluation section. Since MAM3 is the default version of MAM in CAM5, we think it is important to present MAM3 in the first paper. Also, presenting MAM7 in a first paper, then MAM3 (with comparison to MAM7) in a second paper, would lead to duplication of materials shown in the second paper,

We moved most of the model description to the supplementary including: Sections 2.1.1 to 2.1.10 (Aerosol processes), Sections 2.2 (Clouds), 2.3 (Radiation), 2.4 (Turbulence) and 2.5 (Resolved transport). We left the paragraphs that introduce MAM3 and MAM7 in Section 2.1 in the main text. Readers can easily look at the supplementary for the details of aerosol and other physical processes. We note the original GMDD version has 16 pages of references, with many cited in the model description, and these were moved to the supplementary as well. All together we

moved about 25-30 pages of text, references and Tables associated with the model description to the supplementary.

In addition we moved Section 4.4 (Evaluation of Cloud property) to the supplementary. We did not move the sensitivity studies (Section 5) to the supplementary as we consider them to be quite revealing experiments, and they are discussed in connection with the MAM3 and MAM7 results in Figures 15-17. However, we removed Figure 27 in Section 5 by just describing it, as its results are not remarkably different from Figure 13. Through the above changes, we estimate the paper has about 70-75 pages in GMDD format, compared to the original paper's 114 pages, which is a manageable page number for readers, considering that this is a model description paper.

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.*

→Reply: Following the reviewer comment, we changed the title to “Toward a minimal representation of aerosols in climate models: Description and evaluation in the Community Atmosphere Model CAM5”. The reviewer is correct that the goal of this paper is to provide a reference for the climate studies that will use this model. A companion paper (Ghan et al., Toward a minimal representation of aerosols in climate models: Comparative decomposition of aerosol direct, semi-direct and indirect radiative forcing, submitted to J. Climate, 2011) presents details of direct and indirect aerosol effects. We also modified the end of introduction section to make it more focused and added a sentence “The goal of this paper is to provide a description and evaluation of the aerosol module and its two representations”.

- *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.

→ Reply: Following the reviewer comment, we have shortened the description of the aerosol module and simulation results in the Abstract.

- *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.*

→ Reply: following the reviewer comment, we added a sentence at the end of the introduction on what we do: “The goal of this paper is to provide a description and evaluation of the aerosol module and its two representations”. Although we discuss the effect of model simplifications (e.g., instantaneous mixing of primary organic matter and BC with other species) on the aerosol lifecycle in the paper, this is not the focus of the paper, but will be that of our future study. There we will further investigate the effects of different treatments and simplifications (e.g., size range of mode and standard deviation of log-normal size distribution) between MAM3 and MAM7 on the aerosol lifecycle.

- *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.*

We agree. We changed the sentence in the revision to “The bulk method neglects the temporal and often the spatial evolution of the aerosol size distribution”

- *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.*

Done.

- *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.”*

Since there are many more models, we added an “e.g.”

- *Page 3490 L9: what do you mean by “few types”?*

By “aerosol type” we mean “aerosol mixing state category”. Most global models treat only one or two mixing state categories for each size range, whereas Bauer et al. (2008) treat many. We changed the “types” to “mixing state categories” and clarified this in the revision: “In most of the models that do treat mixing state, just a few mixing state categories are used in each size range ...”.

- *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)*

Following the reviewer comment, we added some references: Aquila et al. (2011), Seland et al. (2008), and Wang et al. (2009) here.

- *Page 3490 L22: For AeroCom, I would rather cite Textor et al. (2006)*

Following the reviewer comment, we replaced Kinne et al. (2006) with Textor et al. (2006). We kept Koch et al. (2009) since it has the evaluation of modeled BC profiles at higher latitudes.

- *Page 3493 L3: Do you have any reference for the values of the standard deviations that you chose?*

They are based on Easter et al. (2004) and references therein, and this was added to the revision.

- *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?*

We agree with the reviewer that the primary carbon particles from fossil fuel combustion can be pretty small. However, in this work we assume the same emission size (number mode diameter of 0.08 μm and standard deviation of 1.8) of primary carbon particles from the fossil fuel combustion as that from the biomass burning (Table 1). We note that some studies (e.g., Liu et al., 2005) used an even larger size for primary carbon particles from fossil fuel combustion. As we discussed in the conclusion (Section 6), future MAM development will separate the primary carbon

particles by size and by source (i.e., fossil fuel and biomass burning, with different hygroscopicities and optical properties).

- *Page 3497 L8: I cannot follow this. Why a factor 1.5? Where does the factor come from?*

This 1.5 factor was adopted during model tuning involving anthropogenic aerosol indirect forcing. Considering the large uncertainty with SOA formation, the factor is not unreasonable, and it brings our total OA source close to some recent estimates. This is explained in the revision.

- *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?*

Yes, you are correct. We modified the sentence to “Using this criterion, the mass of sulfate required to age all the particles in the primary carbon mode (by covering them all with 3 monolayers of sulfate), $M_{SO_4,age-all}$, is computed”

- *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 you just multiply x by 0.1?*

This is made clearer in the revision: “The mass of SOA required to age all of the particles, $M_{SOA,age-all}$, is that which gives the same increase in volume-weighted hygroscopicity as $M_{SO_4,age-all}$, i.e.,

$$(M_{SOA,age-all} / \rho_{SOA}) \kappa_{SOA} = (M_{SO_4,age-all} / \rho_{SO_4}) \kappa_{SO_4}$$

If $M_{SO_4,cond}$ (kg) of sulfate and $M_{SOA,cond}$ (kg) of SOA condense on the mode during a time step, we assume that a fraction $f_{age} = (M_{SO_4,cond} / M_{SO_4,age-all} + M_{SOA,cond} / M_{SOA,age-all})$ has been aged.“

- *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)*

Yes, we are speaking about both intramodal and intermodal coagulation. We made it clearer in the revision. We neglect all coagulation involving modes larger than the accumulation mode (coarse modes, fine sea salt mode, and fine dust mode). We mention this in the text and now cite the Binkowski and Roselle (2003). We note that the importance of coagulation drops off rapidly with increasing particle size. Also, treating coagulation for these larger modes would require simulating additional species in them (BC, POM, SOA).

- *Page. 3499 L25: reference for CMAQ?*

Added. We cited Binkowski and Roselle (2003).

- *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?*

The hygroscopicities for sea salt, sulfate, ammonium, and SOA are from Petters and Kreidenweis (2007). The hygroscopicity for BC is set to be zero to represent its hydrophobic nature. The hygroscopicity for POM can vary widely with source type, as mentioned in Liu and Wang (2010). We used 0.1 in the control simulation, but investigated the sensitivity in the paper to a smaller value of 0.0 to reflect the hydrophobic nature of POM from fossil fuel combustion. We use the hygroscopicity of 0.068 for dust, but note the large range of 0.03-0.26 (Koehler et al., 2009) in the paper. The impact of a different hygroscopicities of dust will be the subject of future studies. We note this in the conclusion.

- *Page. 3500 L16: what is the UW parameterization?*

UW parameterization means “University of Washington parameterization”. We changed “UW parameterization” to “Park and Bretherton (2009) shallow convection parameterization” in the revision.

- *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?*

We have revised the description of aerosol wet removal to improve clarity. The “solubility factor” comes from an earlier version of CAM3, and we now realize that

using this term when describing CAM5 confusing and unnecessary. The new term is “wet removal adjustment factor” (equivalently, tuning factor). In cases where a certain wet-removal pathway is not applied to a certain aerosol category (as for in-stratiform-cloud wet-removal of interstitial aerosol, in-convective-cloud wet-removal of in-stratiform-cloud-borne aerosol, and below-cloud wet-removal of stratiform-cloud-borne aerosol), the adjustment factor is simply acting as an on/off switch in the code (i.e., values of 0 are used). For other cases, the adjustment factors are used to reduce the wet removal that is predicted when the individual parameterizations (with their various inputs that come from cloud/microphysics parameterizations, etc.) are used without any adjustment. Values between zero and one are used for in-convective-cloud and below-cloud wet removal of the interstitial aerosol. A value of one is currently used for in-stratiform-cloud wet-removal of the stratiform-cloud-borne aerosol.

- *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 is 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?*

Yes, you are correct in the understanding. The wet removal adjustment factor is applied to the unadjusted removal rate, which in this case involves the rate at which cloud water is converted to precipitation (as predicted by the cloud microphysics). For the case of in-cloud wet-removal, an adjustment factor between 0 and 1 can mean that this conversion rate is too fast (to give “good” aerosol fields) or that the model activates too many of the aerosol particles, or both. As noted above, the zero value is acting as a switch for cases where a certain wet-removal pathway is not applied to a certain aerosol category (e.g., for in-stratiform-cloud wet-removal of interstitial aerosol).

- *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?*

The below-cloud scavenging parameterization computes a wet-removal rate using the

predicted precipitation rate and parameterizations of collection efficiency and raindrop size distribution. An adjustment factor below one simply reduces this rate. We do not apply any particular physical meaning to the adjustment factors.

- *Page 3502 L28: how do you treat ice scavenging?*

Currently wet-removal involving incorporation of aerosol particles into ice particles (through ice nucleation and other mechanisms), followed by conversion of ice particles to precipitation, is not treated. Below-cloud scavenging of interstitial aerosol by frozen precipitation is currently treated like below-cloud scavenging by rain.

- *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?*

Gravitational settling of aerosol particles is calculated in all vertical layers in the model. We made it clear in the revision.

- *Page 3507 L12: shown by whom, Pincus et al, 2003?*

Shown by Pincus et al, 2003. We made it clear in the revision.

- *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.*

We agree with the reviewer that it is more important that the model reproduces the observations than agree with other models. The reason why we put the aerosol distributions and budgets before the model evaluation is that we would like to first provide a global picture of the modeled aerosol to readers. This information is also used in some of the explanations/interpretations of the differences between model results and observations.

- *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?*

We assume internal mixing of aerosol species within each individual mode. Thus the

volume-mean hygroscopicity of a mode is used for calculating water uptake and activation for a mode. This is noted in section 2.1.7 (Water update). If the primary carbon mode is 10% BC and 90% POM at a particular time/location, then we assume each particle in that mode has this same composition, and the BC and POM are activated and removed at the same rate. The global-average time-scale of primary BC aging to the accumulation mode to be about 0.50 days (see section 3.2).

- *Page 3511 L25: why do you have higher MAM7 concentration? Why is there a negative difference in South Africa?*

In MAM7, POM and BC are emitted into the primary carbon mode which has a low hygroscopicity. In MAM3, POM and BC are emitted into the accumulation mode and thus are instantaneously mixed with highly soluble sulfate and other components in the accumulation mode. The accumulation mode has a higher volume-mean hygroscopicity than that of the primary carbon mode and is subject to stronger wet removal by precipitation. Thus in general MAM7 has higher POM and BC concentrations than MAM3, especially in some source regions (e.g., Siberia and Indonesia), where sulfate concentrations are low and thus the aging of POM and BC is relatively slow in MAM7. We added some description in revision.

Negative differences in some region are possible because we are compare two climate model simulations with MAM3 and MAM7. There are differences in clouds and general circulations (induced by aerosol effects and amplified by feedbacks) between the two simulations, which can affect the aerosol distributions.

- *Page 3512 L22: “dust emission is often produced by frontal system”. Do you have a reference?*

Following the reviewer comment, we added a reference (Merrill et al., 1989) in the revision.

- *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?*

See reply to earlier comment regarding page 3511 L21.

- *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?*

Yes, in these regions the (preexisting) particle concentrations are low, so the H₂SO₄ loss through condensation is slow. There are modest sources of SO₂ (from DMS oxidation) and thus H₂SO₄, and the very low pre-existing particle concentrations for the very slow condensational loss of H₂SO₄. These make conditions favorable for nucleation. We have made it clearer in the revision.

- *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?*

Condensation onto preexisting aerosols is the primary sink (about 96% globally) of H₂SO₄ in the model, so aerosol concentration differences between MAM3 and MAM7 influence the H₂SO₄ concentrations. Sea salt mass concentrations are lower in MAM7, resulting in slower H₂SO₄ condensation and higher H₂SO₄ concentrations in the marine boundary layer compared to MAM3. We added a note in the revision.

- *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?*

It is the contribution to total accumulation mode number concentration in MAM7. Figure 5 shows the “total accumulation mode” number concentration in MAM7, which consists of the aerosol number in the accumulation and primary carbon modes, portion of the fine dust mode and the fine sea salt mode. This quantity is more directly comparable to the MAM3 accumulation mode number concentration. Here we want to show the breakdown of contributions from different modes to the total accumulation mode number concentration in MAM7. The % of primary carbon is the contribution to total accumulation mode number concentration. In the revision these contributions are given as absolute values rather than percentages.

- *Page 3516 L23: isn't the lifetime that you report actually longer than in the literature?*

We modified the text in the revision to say that it is longer than in the literature.

- *Page 3518 L4: is this % reported in table 9? I cannot find it. Also, higher than what, AeroCom?*

This is the percentage difference of the POM wet removal rate in this study (0.19 d⁻¹ in MAM3 and 0.17 d⁻¹ in MAM7 in Table 9) compared to the AeroCom mean (0.14 d⁻¹ in Table 9). This number is not reported in Table 9. We have made it clear in the revision.

- *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?*

No. What we mean is that geographic distributions of sulfate and POM are different (e.g., higher sulfur emissions in NH midlatitudes and higher POM emissions in the tropical regions). Clouds and precipitation distributions are also different in the NH midlatitudes and tropics, leading to differences in sulfate and POM wet removal rates. However, during the revision, we decided that this explanation is not satisfactory. The reason why wet removal rate of sulfate in this study is close to that of the AeroCom mean, while wet removal rate of POM in this study is higher than the AeroCom mean, is likely due to the fact that a lower scavenging efficiency was often used for POM than that for sulfate in AeroCom models (Textor et al., 2006). This results in a lower wet removal rate of POM than that of sulfate in some of the AeroCom models. However, in MAM the wet removal rates for POM and sulfate are similar due to the rapid (MAM7) or instantaneous (MAM3) aging of POM. (Compare the wet removal rate of sulfate with that of POM for this study and for the AeroCom mean, shown in Tables 7 and 9.) In the revision we changed this sentence to: “This reflects the fact that a lower scavenging efficiency was often used for POM than that for sulfate in AeroCom models (Textor et al., 2006), while in MAM the wet removal rates for POM and sulfate are similar due to the rapid (MAM7) or instantaneous (MAM3) aging of POM (Tables 7 and 9)”.

- *Page 3518 L26: Why is the wet removal rate so much larger? Is it a matter of unrealistic hygroscopic coefficient?*

The higher wet removal rate in this study compared to the AeroCom mean is likely due to the internal mixing of BC with other soluble materials in this study. In MAM3

the BC is immediately mixed with the other soluble species. In MAM7 it is immediately mixed with POM (which is assumed to be somewhat hygroscopic) and it rapidly ages. Thus the BC wet removal rate (0.19-0.20 d⁻¹) is similar to that of sulfate (0.23 d⁻¹) in this study. Also, lower scavenging efficiency was often assumed for BC than that for sulfate in the AeroCom models (thus a lower wet removal rate of 0.12 d⁻¹ for BC than 0.22 d⁻¹ for sulfate). We don't think the hygroscopicity in our model is unrealistic, because observations indicate that BC particles are internally mixed with sulfate and other components except near the source regions (e.g., Moffet and Prather, 2009; Wang et al., 2010, as cited in the paper), which implies a similar wet removal rate of BC as that of sulfate. We added a note in the revision.

- *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?*

Aging is generally more rapid in the industrial regions than in the biomass burning regions because of higher SO₂ emissions and more H₂SO₄ for condensation in the industrial regions. BC has relatively more fossil fuel sources and less biomass burning sources compared to POM. Thus overall BC ages faster than POM. We made the sentence clearer in the revision.

- *Page 3521 L13: from my understanding, the wet removal is then the weak point of the model. Am I correct?*

Yes. This indicates the importance of realistic treatment of wet removal of aerosol in the model.

- *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?*

The scavenging of aerosol particles by ice clouds is not included. It can be one of the reasons for the BC overestimation in the free troposphere. We plan to include this process and to examine its impact on model simulations in a future study. We have added a note and cited Aquila et al. (2011) in the revision: "We note that this high

bias in the EMAC/MADE-in model was significantly reduced when the scavenging of BC by ice clouds was included (Aquila et al., 2011)”.

- *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.*

We prefer to keep Figure 17 in the main text, since HIPPO is an important dataset with the global coverage, although this campaign was only conducted during the 2-week period. We removed that sentence with “ this kind of comparison is for reference purposes ” to avoid confusion.

- *Page 3524 L11: higher than what? Also, do you mean in the model or in the observations?*

We mean “...higher than other latitudinal bands”, and we are referring to the observations. We have made it clearer in the revision by adding “...than other latitudinal bands...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.*

We agree that it would be interesting to evaluate the modeled mixing state of primary carbon with observations, such as Pratt and Prather (2010). This will be done in our future study. We added a note at the end of section 5 in the revision: “Evaluation of the modeled mixing state of primary carbon with observations (e.g., Pratt and Prather, 2010), will be conducted in our future study”.

- *Page. 3529 L15: I would move this section to the supplementary material.*

We feel that this is an interesting section for the readers, showing the revealing experiments of the importance of hygroscopicity of POM and aging criterion for POM/BC. Thus we prefer to keep it in the main text. However, we reduced the content of this section in the revision by removing Figure 27.

- *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?*

Yes, we are speaking about MAM7 aging and MAM7 hygroscopicity. The POM and BC surface concentrations near continental source regions are not as sensitive to the differences in wet scavenging as are those in remote regions and the free troposphere, where wet scavenging plays a more important role in the difference of aerosol concentrations (Liu et al., 2007, Uncertainties in global aerosol simulations: Assessment using three meteorological datasets. *Journal of Geophysical Research*, 112, D11212, doi:10.1029/2006JD008216). We added a note in the revision: “indicating the lower sensitivity of aerosol concentrations to the difference in wet removal near the sources (Liu et al., 2007)”.

- *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?*

Yes, the size distributions for forest fire emissions and domestic/energy/... emissions are assumed to be the same. No, we don't need a “b” in the first line since we added a note about the size distribution for domestic/energy/... emissions. We mean that the emitted size for domestic/energy/... is in-between the lower value (0.0504 μm) in Dentener et al. (2006) and the larger value (0.206 μm) in Liu et al. (2005). We made this change in the revision: “This D_{emit} value is in-between the lower value of 0.0504 μm used in Dentener et al. (2006) and higher value of 0.206 μm used in Liu et al. (2005)”.

- *Page 3554: I don't understand table 2: they are % of what? And how much is the total? Why is Big Alkanes twice?*

They are mass yields of condensable organic vapor (i.e., the SOAG species) from each primary VOC. The total source of condensable organic vapor is 103.3 Tg OM/year. We added notes in the revision: “mass yields of condensable organic vapor from each primary VOC” and “...with an emission of 103.3 Tg OM per year”. Note the “a” in Alkanes versus “e” in Alkenes.

- *Page 3555: I would put table 3 and 4 together*

Done.

- *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?*

In MAM3, BC is emitted into the accumulation mode and is thus instantaneously mixed with other soluble species and aged. We do not have two tracers for hydrophobic and hydrophilic BC, respectively, and an exponential decay. This is one of the major differences between MAM3 and MAM7, as discussed in the paper. However, model simulations and observations show that BC is rapidly aged with a time-scale of a few hours, as discussed in the paper. Future development will include BC in the Aitken mode to account for the smaller emission size of BC from fossil fuel source.

- *Page 3568: I would add a column with the AeroCom or the observed values, too*
AeroCom does not provide the budgets for the break-down of POM (i.e., in primary and accumulation mode). The budget comparison for total POM with the AeroCom has been shown in Table 9.

- *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?)*

We feel that it would make the figure a little messy when we add the size ranges of modes to the figure. Instead we put them in a table (new Table 1). We do not have a hard cut-off between modes, except when calculating sea-salt and dust emissions.

Technical corrections

- *Page. 3488 L14: isn't it cloud CONDENSATION nuclei?*
Yes, change made.
- *Page. 3488 L19: The "however" is not really fitting here*
Removed.
- *Page. 3492 L 10: add a comma after "sea salt"*
Done.

- *Page 3492 L18: add the number of the sections to which you refer.*
Done. We added Sections 2.1.5 and 2.1.6 (now in the supplementary).
- *Page 3493 L3: Add the values of the standard deviations in the text, too.*
We now put the values of the standard deviations in a table.
- *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.*
We changed the sentence in the revision to “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 soluble species like sulfate and SOA. Thus the merging of dust and sea salt in a single mode is unlikely to introduce substantial error into our simulations;”
- *Page 3493 L22: report standard deviations in text, too.*
We now put the values of the standard deviations in a table.
- *Page. 3495 L 15: the units should be after each number.*
Done.
- *Page 3519 L13: you don’t need a reference to table 9, since it’s the table that you are describing.*
Removed the reference to table 9.
- *Page 3559: use “-“ in the AeroCom column, as in table 6*
The numbers in the AeroCom column of Table 7 are not ranges of values for the different models. The first numbers are means and the second numbers are normalized standard deviations (in %), as noted in the Table 7 caption. The numbers in right column of Table 6 are ranges. We added “The range of results from other studies is from Liu et al. (2005) and references therein” in the caption of Table 6 to make it clear.
- *Page 3572 and ff: nearly all figures should have a larger font.*
We increased the font sizes of Figures 3-6 in the revision.

- *Page 3573: I would change the scale and choose a different one for each plot*
Following the reviewer comment, we changed the scales for sulfate, BC, POM, and SOA plots to make the plots more visible.