

Dear Editor and reviewers,

First, we would like to thank once again to the reviewers for their comments and efforts to improve this article. We carefully revised our manuscript, by taking into account the comments and suggestions, and we hope that we adequately addressed remarks. The responses to the reviewers follow in this document.

Yours sincerely,
Authors

Reviewer 1:

1) Section 3.3.2, paragraph starting with 'In SIM1, the raindrop size': replace 'size' with either 'diameter' or 'radius'.

r: We changed 'size' to 'diameter' as suggested.

2) Section 3.3.3, paragraph starting with 'The Slinn (1977, 1982a) parameterization': change 'aerosols type' to 'aerosol type'.

r: We changed it as suggested.

3) Section 6, paragraph starting with 'Figure 3 presents': Replace 'Figure 3' in the first sentence with 'Figure 6'.

r: We replaced it as suggested.

4) Section 7: correct the mislabel on the section from 6 to 7, and likewise correct number labels on following sections.

r: We solved this issue.

5) First paragraph of Section 7. Please quantify what is meant by 'influence the AOD only moderately' and change 'influence' to 'influenced'.

r: We added to the sentence (line 648 in the updated manuscript): ...the results of the SIM2 and SIM2_WDEP simulations are very similar with the high correlation between them (0.92, Fig. 10a).

6) Summary and conclusions section, second paragraph: Please quantify what is meant by 'significantly improve agreement'.

r: Before the end of that paragraph we added (line 802): Overall, the updates had a positive effect on the correlation with observations. Quantitatively, as an example, in the comparison with MODIS observations on the global scale, the update of parameterizations improved the correlation from 0.06 to 0.32.

7) Table 4: OC is mentioned in the caption but not included in the table. Please add OC.

r: We did not include OC in the table from the reason that we believe that AeroCom POM value cannot be easily converted to ACCMIP OC, since the different AeroCom models have POM implemented in numerous different ways (Textor et., 2006), and that BC in the table reflect the differences in the performance of the carbonaceous aerosols in our model simulations. We removed OC from the caption of the table.

8) Table 4: Lifetime of BC for SIM1: The lifetime calculated by dividing burden/emissions is 1.5 days lower than the value in the table - please check this.

r: Yes, a typographic error was in emissions of BC, and this we now corrected.

9) Table 6: The results presented for SIM2 are a repeat of the results for SIM2 in Table 5. Could Tables 5 and 6 be combined to avoid this repetition?

r: We have combined Tables 5 and 6 into one table in order to avoid the data repetition.

Reviewer 2:

–The authors added some more references to relevant studies on the impact of different parameterization schemes on model simulations but did not discuss or described (e.g. with numbers) the findings in the paper. That would stress the impact different parameterization schemes can have in the model.

r: In order to stress how different processes affect the model uncertainties, we added in the introduction some findings from different relevant studies. In the paragraph dealing with aerosol sources we added (from line 55 in the updated manuscript):

“The AEROCOM model inter-comparison run with and without harmonized emissions (Textor et al., 2007) showed that, although the uncertainties in emissions can be large, after the emission harmonization the inter-model diversity decreased slightly but remained large. The standard deviation of the total aerosol burden decreased from 18 Tg, for non-harmonized emissions to 16 Tg, for harmonized emissions. Therefore, the parameterizations of physical processes contribute significantly to the model uncertainties.”

In the removal processes paragraph we added (from line 67):

“The comparison of the models and their performance compared to dust measurements after long-range transport by Prospero et al. (2010) showed that the ratios of different deposition mechanisms varied greatly among the models and against the observed ratios. For example, the ratios of wet deposition to dry deposition ranged from about 1:1 to 30:1 in the models, in contrast to about 3:1 to 4:1 at the measurement stations. This and findings from the other studies demonstrate that aerosol deposition is complex and challenging to implement in an accurate way (Rasch et al., 2000; Sportisse, 2007; Prospero et al., 2010).”

And regarding wet deposition we added (from line 75):

“Its uncertainty is augmented by the uncertainties in precipitation and aerosol properties, and wet deposition is identified as a key source of uncertainty in aerosol models (Vignati et al., 2010; IPCC, 2013). Rasch et al. (2000) showed in an inter-comparison that model simulations differ most strongly in the upper troposphere for species undergoing wet scavenging processes.”

– “Regarding the anthropogenic aerosol emissions, it is true that so-called unspecified primary anthropogenic aerosols are not included in the presented configuration of the model. The reason is availability of such emission inventory for the global emissions. It is not present in ACCMIP inventory. In explaining the difference between SIM2 and observations we mentioned only secondary aerosols, because their quantities are probably more important for the influence in AOD. Of course, we can be sure about it only after completely introducing secondary aerosols in the model.”

A short discussion on this issue could be added to the discussion section.

r: In the discussion section we added a paragraph (from line 752):

“Secondary aerosols can certainly account for the discrepancies between the model and the observations in the zones where anthropogenic aerosols have a major influence, as already discussed. However, the so-called unspecified primary anthropogenic aerosols can also play a role, but the secondary aerosols should have a stronger influence on AOD. The unspecified primary anthropogenic aerosols are not implemented in the configuration of the model used in this study, because they are not present in the emission inventories that we used, but they can be found in some models (e.g., Matthias et al., 2008).”

2 General description of the model

– p.5, end of second paragraph: Please change to aerosol specie

r: We thank the reviewer for their careful reading, however, we respectfully disagree: We left ‘aerosol species’ as it is since it is also a singular form of this noun.

– p.5, last paragraph: Please give a reference for the GADS data

r: The reference is Koepke et al., 1997.

3 Aerosol parameterization in the model

3.2 Sedimentation

– Could you please give a reference for the ‘assumed value of the kinematic viscosity’ or explain shortly how there are defined.

r: This was an approximation used in our model in SIM1. The kinematic viscosity was considered constant, and from it, the dynamical viscosity was calculated. We added the adjective ‘constant’ to limit the miscomprehension.

3.3 Wet deposition

– This section is much clearer now and the difference between the ‘new’ and ‘old’ scheme is also explained better.

– It might be helpful to include some short headings (without a chapter number) as for example for the fraction of cloud cover of precipitation forming clouds

r: We added different un-numbered headings throughout the text: “Cloud cover of precipitation cloud cover” (line 223), “Implemented schemes” (line 248), “Phoretic and electric effects” (line 312).

– p. 10, l.1 This sentence is confusion please rewrite

r: We have rewritten this sentence (line 274).

3.4 Emissions

– p. 13, 3.4., ll. 4-6. Please rewrite the sentence.

– p. 13, 3.4., ll. 6-10 This sentence is confusion, please rewrite.

r: We have rewritten these sentences (from line 309).

– Why is the reference year 1996 chosen? There should be emission databases from more recent years available. Please explain.

r: AeroCom inventory is based on Bond et al. (2004), which used 1996 as the reference year. In Lamarque et al. (2010) emissions that are implemented in SIM2, the base reference is Bond et al. (2007), which used 2000 as the reference year. In the rewritten form, this should be clear.

3.4.1 Sea-salt source function

– p. 15, l.18, please change sentence: *The difference between the two can be as large as ...*

r: We have changed this as suggested.

3.3.2 Desert dust emission schemes

– p. 16, l.14, please change to: *Laurent et al. (2006)*

r: We corrected it.

– p. 16, l.20, please change to: *[...] in different the model configuration [...]*

r: We did it.

4 Observations

– Please mention also that the EMEP stations are background stations.

r: We added to the description as suggested (line 409).

6 Results

– p.18, last sentence, *Where in the text can the information on the shift in the size distribution be found? If there is no figure please mention that.*

r: The shift of the initial size distribution towards smaller sizes is presented in Sect. 3.4.2. We added the reference to this section in the sentence (line 530).

– p. 18+19, *Figure 3 is only briefly mentioned in the text but not explained. If there are important findings from this figure please describe them, so far the conclusions described in the text can be also be drawn from figure 2.*

r: We have added to the text (from line 536):

“Figure 3 confirms these findings and, although a number of effects influence the mass mixing ratios, one can see that the updates generally produced more desert dust and sea salt aerosols toward higher altitudes. Regarding black and organic carbon aerosols, Fig 3 shows their higher concentrations in the free troposphere in SIM2 than in SIM1. This is the result of the weaker wet deposition in SIM2 than in SIM1, and of the shift in the wet deposition vertical distribution by having a weaker below-cloud scavenging and a stronger in-cloud scavenging in SIM2 compared to SIM1.”

– p. 19, l. 14, *where does these numbers come from? There are not in the table or the figure.*

– p. 21, l. 11+15, *Where do these numbers come from? Is that averaged over the whole time period?*

r: These numbers were remains of statistics in terms of bias and RMS, that were used in the first version of the manuscript. They are now expressed in the new metrics (following the suggestions of Reviewer 1), and redundant information is removed. The metrics in the abstract are changed as well.

– P. 21, l. 4, *Please change ‘Figure 3’ to ‘Figure 6’*

r: We corrected it.

– p. 22, *Fig. 10c is mentioned first in the paragraph (before a, b) please consider reordering the figures.*

r: We reordered the figure as suggested.

Figures:

- *Figure 5, What is the green color representing, what are the numbers in the legend? Please clarify*
r: We explained better in the figure caption what is represented, and we added the color bar titles in Fig. 5 and Fig. 10.