

Interactive comment on “Modelling of primary aerosols in the chemical transport model MOCAGE: development and evaluation of aerosol physical parameterizations” by B. Sič et al.

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We would like to thank to the reviewer for the constructive reviews and comments which contributed to improved readability and quality of our manuscript. We took into account remarks and suggestions pointed by these comments. Regarding the general comments, in introduction, we added more references in order to better embed our study with other recent research studies in the topic of model uncertainties. At the beginning of the Sect. 3 we defined SIM1 and SIM2 configurations, and used this notation throughout the text afterwards. This should make the parameterization description easier to understand and the text much clearer. A list of projects in which MOCAGE

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is involved is added to the introduction. Having the model output, in these projects, usually used in terms of aerosol optical depth and PM_{2.5}/PM₁₀ concentrations, we agree that for the complete performance evaluation of the CTM MOCAGE it would be necessary to evaluate its performance also in terms of PM_{2.5}/PM₁₀ concentrations. Thus, we added in the results section the evaluation of the model performance relative to EMEP measurements, with a new figure and the description of results. Also, we agree that for the clearer image of the model differences between SIM1 and SIM2 it would be advantageous to present the vertical and horizontal distribution of aerosols. Thus, two figures are added, and their description and discussion are added to the Results section.

The meteorological input field from the NWP ARPEGE model are used in our model and the used parameterizations also depend on them. The ARPEGE is an operational model, but as well a state-of-the-art model, and it is in the state of frequent changes and improvements. For that it is difficult to have a reference to the state of the model used for our input fields or to shortly describe the necessary characteristics. But, due to its importance for the sea salt (and desert dust) emission scheme and because its uncertainties were mentioned in our paper as a possible source of features seen in the model, the wind field from our meteorological input is directly compared with the satellite QuikSCAT measurements. The figure of this comparison is added and commented in Sect. 2.

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.

Regarding the physical basis of the sea salt emissions, the description of the influence

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of sea surface temperature is added to the Sect. 2

Specific comments:

The small additions, errors and necessary clarification remarks in this part 'specific comments' were taken into account and the manuscript is corrected, but we do not necessarily comment them all in this response. For other suggestions and remarks, the answers follow:

Abstract

- Please mention that this is a global CTM
- Please specify which primary aerosols are included in the model
- p. 2746, l. 10: add "the" in-cloud scavenging
- c: We changed the manuscript as suggested.

1. Introduction

- Please mention that the simulations are on the global scale and for the year 2007
- c: Done. Further, we added the description of the projects, related to aerosols, in which MOCAGE participated. Also, we added more references in the introduction.

2 General description of the model

- Please add a short description on the studies/projects the model is used for.
- p. 2750, l.16: change "in" Meteo-France to "at"
- c: The description of the projects, related to aerosols, in which MOCAGE participated was added in Introduction. This information is introduced earlier in the text, to justify the types of observations used in the study.

3. Aerosol parameterization in the model.

- Please clarify the difference between the current and the updated calculation of the sedimentation in the model
- c: At the beginning of this section (Sect 3), we added an introductory paragraph where

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the references to the SIM1 and SIM2 configurations are added earlier in the text, and they are referred in the whole Section 3. This should lead to a clearer image of the described model changes throughout the text. This also applies for the mentioned differences in sedimentation scheme in SIM1 and SIM2.

- p. 2753, l.4: The sentence structure is confusing, please rewrite the sentence.
- c: We have rewritten the sentence in order to be clearer.

- This section is difficult to read, please reorder it. It would be helpful to have an introductory paragraph which describes the current version of the model and the updates. Please add the according chapter numbers where it is described in more detail. Please also explain a bit more why the new schemes are chosen.

- c: The comment given for the sedimentation differences applies also for this remark.

- p. 2753, l.10: Please add a reference if available

c: Added.

- p. 2753, l. 18-19: Does this mean that the rate of precipitation formation in (5) and (6) is calculated following Xu and Randall (1996)? Please clarify this in the text.

c: We clarified the description of the scheme that is used to calculate rates of the precipitation formation. Precipitation rates are calculated by the diagnostic scheme that uses the cloudiness scheme of Xu and Randall (1996) and the autoconversion scheme of Kessler (1969).

- p. 2754, l. 1-3: Why do you use the Brost et al. (1991) value if there are updated values?

c: The most recent values to our knowledge are of Brost et al. (1991), and we did not find any updated values after that publication. In the text we were referring the Brost et al (1991) value as the updated value. This sentence is changed in the text in order to be easily understood.

- p. 2754, ll. 17-18: Why do you update the model with an older scheme (1986) than

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the one used before (1991)?

c: The scheme of Giorgi and Chameides (1986) separates stratiform and convective precipitation, which Langner and Rodhe (1991) scheme do not. Also, the scavenging efficiencies for different aerosols types, of which the Langner and Rodhe (1991) scheme would be advantageous are parameters which are not well established and still bring significant uncertainties. Moreover, the scheme of Giorgi and Chameides (1986) is now used in the model for scavenging of the gases and aerosols. This is not crucial for this study, but it is helpful for the consistency and unification of the parameterisations in the model.

- p. 2755, ll.6-10: It would be helpful to get this information earlier in the text.

c: This information concerns the in-cloud scavenging schemes and the scheme are described in details in this subsection. Therefore, the information regarding the schemes' complexity is put here. We also rewritten these sentences to be clearer.

- p. 2755, ll. 15-16: What does "the considered time step" mean in this context?

c: We reformulated the sentence so that does not mention this syntagm and to lead to a clearer explication of the trigger of below-cloud scavenging in the model gridbox.

- p. 2758, l. 19: Please rewrite the sentence; it does not get clear whether there are wider sets of snowfall parameters needed or available.

c: We talk about necessary snowfall parameters and this is now clarified.

3.3 Emissions

- Usually emission inventories for aerosols also include unspecified anthropogenic primary PM. Is this also included in the chosen database? If so why is it not used in the model?

c: With this question we dealt in the general comments.

3.3.1 Sea-salt source function

- p. 2761, l. 10: What is B?

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c: The B in the sea salt source function is the unnamed parameter. We changed the sentence to make this clear.

- Which sea surface temperature data set is used?

c: The used sea surface temperature dataset is from Reynolds (2002). This is now clarified in the text as well.

- Please describe in the beginning of the paragraph what the updates for the desert dust emission scheme are.

c: We added the paragraph which sums the changes on one place, before explaining them in more details.

4 Observations

- p. 2763, l.23: "less than 5 observations" per what?

c: We clarified in the text that we talk about 5 "level 2 observations" per each "level 3 gridbox".

5 Experiment design

- p.2765, l. 7: Don't understand this sentence, please clarify.

c: We clarified it.

6 Results

- pp. 2765 + 2766, ll. 25-28 etc.: Is there a possible explanation for that feature?

c: Wind speeds in the model are evaluated against QuickSCAT measurements, and we updated these sentences to reflect this addition.

- p. 2766, ll. 7-9: Where is this explained in the first paragraph? Please elaborate this a bit more.

c: We added the horizontal and vertical distribution graphs and we discuss this point in more details now.

- p. 2766, ll. 20-25: Please add the information that these numbers can be found in

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table 3.

c: We mentioned it in the manuscript (original manuscript p. 2766, ll. 18)

- p. 2766, ll. 26: To verify this it would be interesting to have more information on the quality of the input meteorological fields, especially on the wind fields as they are very important for dust and sea salt emissions. A short paragraph on this could be added to chapter 2.

c: We added the comparison of wind fields in the model with satellite measurements from QuikSCAT satellite. The figures are presented and discussed in Sect 2.

- p.2767, l. 6: Please add the numbers for the correlation to the text.

c: We quantified the changes in the revised text.

- p. 2767, l. 26: Which date is it?

c: We specified the date in the revised text.

- p. 2768, l.14: But the sea salt lifetime in SIM2 is shorter compared to SIM1 and the burden is much higher than in AeroCom. How does this fit with the observation that the sea salt overestimation is reduced in SIM2 described on p. 2767?

c: When we talked about overestimation of sea salt in SIM1, we were mainly thinking on AOD overestimation in the mid-latitude southern hemisphere oceans. The structure of this sentence was changed which should lead to clarification.

7. Discussion

- p. 2771, ll. 4-5: sentence structure, please rewrite

- p. 2771, l. 17: sentence structure, please rewrite

- p. 2772, l. 3: "Longer mean atmospheric residence time" compare to what?

c: We made necessary clarifications.

8 Summary and conclusion

- p. 2773, l.23: The dust size distribution is not shown in the results chapter – why not?

c: In the model output we do not expect that the model updates of wet deposition and

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sedimentation produce the important changes in the mass distribution between bins. The in-cloud scavenging schemes in SIM1 and SIM2 are not size dependent. The below-cloud scavenging scheme from Slinn (1977) is used in both SIM1 and SIM2 with different subcomponents, but the size dependency is not altered. The mass between bins in our changes is significantly changed only by the changes in the emissions by the way how we defined them as explained in the text. For this reason, we did not look closely at the at the bin distribution in the described simulations. When talking about the evolution of mass between different bins in the model during time, this is explored in Martet et al. (2009) for the case of desert dust aerosols.

- P. 2773, l.26: What is the physical reason for that?

c: The mechanisms how sea surface temperature influences the sea salt production is explained in Jaeglé et al. (2011). It is connected with kinetic viscosity of water and the gas exchange efficiency which leads to stronger whitecaps coverage in warmer waters. We added this explanation in the text.

Tables and figures

Table 2: This table is hard to read. Please consider reorganizing it, e.g. by scheme (in-cloud scavenging, below-cloud scavenging, sea-salt emission etc.) in the row and simulation names in the column.

Table 3: It would be interesting to also see the absolute number of MODIS and AERONET AOD at the individual station in the table.

Figure 2: What exactly is shown in this figure? Is it one dot for every grid cell?

Figure 3: It would be helpful to see the areas somewhere in a map. May be it is possible to mark them in figure 1.

Figure 5: Why is the figure for SIM2 shown first?

c: Table 2 is reorganized and this should improve the readability. In Table 3 we added the column with a number of observations for all datasets. In Fig. 2 we have one dot

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for every grid cell. The boxes showing regions used for Figure 3 are added to Figure 1. In Figure 5, the we now show SIM1 before SIM2.

Table 4: Is it averaged over the whole year and area? Why is OC not shown?

c: In Table 4 the values are global annual. The OC is omitted because in AeroCom data we find POM (particulate organic matter), and it is suggested that it can be converted to OC by using a fixed presumed POM/OC ratio. We decided not to put OC in this table because, first, we believe that the AeroCom OC that we could calculate from particulate organic matter (POM) and the fixed presumed POM/OC ratio, does not reflect exactly the ACCMIP OC. POM in AeroCom models, used to calculate median values in the model inter-comparison, are treated in variety of different ways, and the fixed presumed POM/OC ratio does not necessarily represent well the POM/OC relation. Second, we believe that the BC comparison reflect well the difference in the model performance of the carbonaceous aerosols in SIM1 and SIM2.

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