

Interactive comment on “The aerosol-climate model ECHAM6.3-HAM2.3: Aerosol evaluation” by Ina Tegen et al.

Anonymous Referee #1

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General comments:

The present study shows an update version of ECHAM-HAM model with considerable effort to improve the schemes and verify results. The manuscript is well written, and it will be acceptable for publication. However, I have two concerns about the present study. The first is a sea salt. The simulated sea salt results are greatly changed. The global burden and emission flux in the updated version are significantly reduced with previous version and references, as shown in Table 3. The physical process in the updated version may be improved, but the simulated sea salt results are not well improved. It is much better to add evidence of sea salt improvement to the present manuscript. Another point is that the evaluation of the simulated number concentrations is a bit rough. Quantitative evaluation using statistic metrics is necessary to verify the

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results.

Specific comments:

P.6, L9-11: Which species are emitted from a model level corresponding to a specific altitude?

P.7, L26-31: Is the correction factor a tunable parameter?

P.11, Table 2: Are all tuning parameters set to the same among these three experiments?

P.12, Section 4.3: Why are the authors not using measurements in Asia? EANET (<http://www.eanet.asia/index.html>) can be useful for the model evaluation of sulfate.

P.13, L17-19: In section 5.5.2 and 5.5.3, the authors pointed out the possibility of the underestimation of OC and BC emitted from combustion sectors. This can be also mentioned here.

P.13, Section 5.2: Do the authors compare AOT with AERONET under the clear sky conditions? Or whole sky? Please clarify it.

P.14, L10-19 (Figure 5): Could you add the statistical metrics to Fig 5 or new table?

P.16, L5: The authors insist “Overall the agreement is good in most cases”, but there is no quantitative discussion. Could the authors add some statistic metrics to this analysis? Also, how about the error bar of the measurements?

P.19, L1-3: As I mentioned in general comments, the uncertainty of the emission fluxes over the United States is expected to be relatively low, but the authors mention the possibility of the lack of local emission. Do the authors have any evidence?

P.21, Table 4: The values are different from that shown in Figure 21. Why?

P.21, Section 5.7: As I mentioned in general comments, the improvement of the module for seasalt is very interesting, but the evaluation may be inadequate and should be

more validated. For example, how about the AOT over the Southern Ocean? In addition, how about the evaluation of fields for cloud and precipitation, which can strongly affect the scavenged process?

P.24, L4-6: The issue of the model grid size can be also important to resolve such local emission. Do the authors have a plan to use finer horizontal resolution?

Figure 3: Why is the region (>40S) in the Southern Ocean shaded? I think MODIS can capture these regions, even though the uncertainty may be large.

Figure 11: IMPROVE also measures 2003-2012, but why do the authors select the specific years of 2000-2004?

Figure 19: How about the statistic metrics like the correlation coefficient?

Technical corrections:

P.6, L33: I think the resolution is 0.1 degree in the latest GFAS (version 1.2), but do the authors use the older version?

P.7, L1-4: In Veira et al. (2015), the scaling factor is recommended in version 1.1. Now, the latest version is 1.2. So, do the authors use GFAS version 1.1? Please clarify it.

P.18, Table 3: It is much better to add the range of AeroCom estimates.

Figure 1: What is the grey circle?

Figure 2: What is the purple in Kalimantan Island? What is the blue around Japan?

Figure 5: What is the grey?

Figure 9: Some panels (FKL, CMN and JFJ) include yellow lines. Please remove the lines.

Figure 10: How about the error bar of the measurements?

Figure 19: dust AOT → AOT in the dusty region?

Figure 22: In the panels (c) and (d), which station is it?

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