

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

Anonymous Referee #2

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The manuscript “The aerosol-climate model ECHAM6.3-HAM2.3: Aerosol evaluation” by Tegen et al. provides a documentation of the new version of the aerosol module in the ECHAM model, and a comprehensive evaluation of aerosol simulation. They use in-situ surface and aircraft measurements and satellite data to evaluate different aspects of the model, finding that the model generally perform very well. This is an excellent paper for its scientific and technical significance as well as its clear and concise presentation. I strongly recommend this paper to be published as soon as possible.

I have a few comments for the authors to consider.

In Table 3 and 4 you provided the comparison between this model with AEROCOM and the previous version of the model. I find the comparison very useful! Please consider to do similar comparison for the rest of the paper, when possible, either by provid-

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ing the values/plots from the previous version, or by providing a couple of sentences and citation to the papers documenting the evaluation of the previous model. It might also be a good idea to provide your experience on what change(s) in the model (from ECHAM5-HAM2 to ECHAM6.3-HAM2.3) cause the specific improvement. This kind of information can help other modeling centers to make similar improvements. Page 7, Line 12-31: Could you please provide a few sentences to describe what might be the cause(s) for the need of using different scale factors in different regions. Is it because the surface wind bias in different regions are different? Or, is it because the satellite-based dust map is insufficient to constrain the emission? Does model resolution play a role here? Lastly, how do you decide the scale factor? Do you use dust AOD from satellite or in-situ measurements? Page 8, Line 20-26: Since SOA is often considered poorly treated in GCMs, could you please briefly describe the uncertainty of the SOA treatment and its potential impact on the conclusion. Providing some literature review will be great. Page 10, Line 13-25: When pressure and winds are nudged, do you need to retune the model either to restore the TOA energy balance and/or to restore the reasonable surface flux (of heat, moisture, momentum, and sea salt and dust)? If the CLIM run’s surface winds and NUDGE run’s surface winds are very different, surface fluxes will be significantly different. Also, what is the relaxation time scale for nudging? Page 13, Line 13-16, The 2 nudged simulations produce very large AOD bias over subtropical ocean (Fig. 3). This might indicate that the sea salt emission needs to be tuned down. Section 5.3: I think near the source region the size distribution comparison is not very interesting since the model’s size distribution is closely related to the assumed size distribution at emission. The comparison between more interesting when it is done for remote regions when the model physics has enough time to change the size distribution. I wonder if it is possible to do the analysis that way. Could you please comment? Figure 5, 6, 19, 20, 22a,b: Please also add the R, RMSE, and mean bias in the figure.

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