Geosci. Model Dev. Discuss., 8, C3209–C3217, 2015
www.geosci-model-dev-discuss.net/8/C3209/2015/


Geoscientific Model Development Discussions

# *Interactive comment on* "New developments in the representation of Saharan dust sources in the aerosol-climate model ECHAM6-HAM2" *by* B. Heinold et al.

**B. Heinold et al.**

heinold@tropos.de

Received and published: 4 December 2015

See also the attached pdf-file for the listed response to the questions raised by the reviewer, and an updated version of the manuscript. In the manuscript, the revisions are indicated by red italic text.

Authors' Response to Reviewer's Comments C2455

Manuscript No.: gmd-2015-147, submitted to GMD Title: New developments in the representation of Saharan dust sources in the aerosol-climate model ECHAM6âĂŘHAM2 Authors: B. Heinold, I. Tegen, K. Schepanski, and J. R. Banks



We would like to thank the reviewer for her/his time and constructive comments, and hope that we have responded satisfactorily to all the points raised.

Anonymous Referee C2455 Received and published: 9 October 2015

RC: In this study, the authors use a satellite-derived mineral dust source area distribution to pre-scribe preferential dust sources in the climate model ECHAM6-HAM2. The authors report a large increase (15 to 22%) in total mineral dust emissions compared to the previous version of the model. Comparisons to observations show generally modest improvements. The paper is well-written, and the analysis is straightforward. Figures illustrate the results and discussion well. I however recommend revisions to improve two aspects, as detailed in my comments below. First, to place the paper more clearly in the context of ECHAM devel-opment. Second, to improve the comparison between model and observations with a meth-od that is less influenced by different sampling of model and observations. Those improve-ments should amount to major revisions.

AC: Actually we see the paper in the context of ECHAM-HAM development in its present form, supplemented with an evaluation of the model capability to represent dust-generating winds compared to a regional model. Regarding the different sampling of model output and satel-lite observations, we think there is a misunderstanding. The difference in sampling is taken into account. Please see our response to the 2nd main comment.

RC: In addition, I encourage the authors to think about postponing the paper until they have run the free-running simulations that they mention in the conclusion (page 7897, lines 17–19). Although the paper could be published after the authors have addressed the comments be-low, it would be a relatively minor contribution to the literature. Analysing free-running simulations would give more breadth to the discussion. I also doubt that a standalone paper on free-running simulations could be published, as it would really be incremental, so if free-running results are to be discussed, it is now.

AC: We strongly disagree. The value of this publication, which we do not consider to be minor, is not only in the introduction of a new source approach to the ECHAM-HAMMOZ model and its evaluation, but also in the comparison between a global and a regional model, each with the same dust emission scheme. This provides the unique opportunity to investigate the model description of dust emission processes at different scales. Note that the evaluation of the global model results with observations is an important part of this work and so nudged mod-el runs. The results of free-running simulations, of course, would not be published in a standalone paper but in the framework of a scientific study, e.g., on interactions of mineral dust with ice clouds and radiation or a thorough evaluation of the model representation of key meteorological processes driving Saharan dust emission.

Main comments

RC 1: The changes brought by the MSG-derived DSA are essentially neutral. What decision was made in the end? Does ECHAM6-HAM2 now use the MSG DSA distributions, or did the authors keep the previous representation by Tegen et al. (2002)? The conclusion is unclear on this point (page 7897, lines 10–19). From a model development point of view, I am also surprised that revisiting the simplified assumption made for roughness length (Pages 7884 and 7885, lines 1) is not given more priority than changing the DSA dataset. A more realistic roughness length dataset is dismissed on the ground of failed tests in the past (page 7884, line 4), but that is hardly satisfactory. Why not take the op-portunity to see whether combining satellite-derived roughness length and DSA datasets yield better results?

AC 1: The overall changes may be neutral, but there are small but noticeable improvements in the placement of active dust sources across the Sahara and dust optical thickness in the southern Sahara. We believe that ECHAM-HAM users will benefit from the update of the Tegen et al. (2002) scheme, as it "compensate[s] for uncertainties in soil properties and the misrepresentation of dust generating winds", although this benefit seems to be less than expected at the moment. As we say in the "Code availability"



section, the new MSG-based DSA map will be distributed with the ECHAM6-HAMMOZ model. However, the previous representation of potential dust sources remains part of the model, and users can switch between the different versions as they can for previous development steps. We agree that roughness length is an important parameter in the dust emission process, which should be considered in a more sophisticated way. However, the challenge is to describe the surface roughness at process scale. For example, even high-resolved rough-ness length data from satellite remote sensing (e.g., Prigent et al., 2012) may be not rep-resentative, e.g., for erodible soil beds in mountain foothills that are important dust sources. Another issue would be how to use high-resolved roughness data on coarse model grids. Any kind of averaging would yield values too high to be typical of erodible soil beds. A practicable way would be to compute dust emission fluxes at the resolution level of roughness data in a mosaic approach, which however needs more effort and may be computationally too expensive for a climate model.

RC 2: Page 7891, line 7 and Figure 6: Comparing against satellite retrievals is obviously useful, but their sampling of the dust distribution is very different from that of the model. It is easy to reach misleading conclusions from such comparisons. Applying each instru-ment's retrieval mask on an hourly basis would be the best way to do the comparisons properly (especially since the model is nudged), but if that is not possible at this stage, I would at least expect a masking on a monthly basis. In any case, comparisons as they currently are do not tell much about model skill and how it has changed after using the MSG DSA. Note that the same remarks essentially hold true for comparisons against AERONET.

AC 2: We account for the difference in sampling between satellite retrievals and model by using the model output only at times, when satellite observations are available. In this case, the agreement should not depend on whether hourly or averaged values are compared. Of course it would be desirable to evaluate the model results at higher temporal resolution, maybe on a case-by-case basis. Unfortunately, this is not feasible

for a 2-year period. Instead the modeled dust emission events are evaluated in terms of their time of occurrence and related meteorological mechanisms.

Other comments

RC 1: Page 7886, lines 6–7: I understand that getting closer to a multi-model average is reas-suring, but that is not necessarily a good thing. Are there observational estimates of to-tal Saharan dust emissions that could be more usefully compared against?

AC 1: The aim, of course, is not to further develop models for the sake of matching a multi-model average. The range of model simulations is rather considered to indicate uncertainties in the understanding of key mechanisms. This obviously misleading sentence ("The new values better fit the range . . .") is skipped. Regional and global estimates of dust input into the atmosphere to date cannot be de-rived from observations, such as satellite imagery. The few available in-situ measurements of dust emission are rare and limited to specific field sites and short time periods. Estimates of dust emissions, therefore, rely on model simulations, which are constrained by continuous ground-based and space-borne remote sensing as well as campaign-based observations of atmospheric dust load.

RC 2:Page 7887, line 1: "a lower emission flux limit"

AC 2: Corrected.

RC 3: Figure 3: Isn't comparing MSG DSA against ECHAM-HAM(MSG) a bit circular? I agree that the model can (and does) still get the emission flux wrong, but I do not think that we learn much from that comparison.

AC 3: We think there is a misunderstanding. As described on page 7884, line 27-29 and em-phasized again on page 7885, line 20-24, the spatial distribution of observed DSA frequencies between March 2006 and February 2010 is only used to provide the location and extent of potential dust sources. Specific dust events, however, are not prescribed. The actual activation of a grid cell as dust source and then the magnitude



of computed dust emission depend on the modeled surface friction velocities. Therefore, the compari-son between model results and actual MSG DSAs in Figure 3 is largely independent and allows to focus on the model representation of meteorological processes driving dust uplift.

RC 4: Page 7890, line 23: Water vapour is only really a problem for infrared retrievals, not so much for MISR.

AC 4: Agreed. Now the sentence reads: "Space-borne remote sensing always suffers from the fact that dust information is obscured by clouds. Further potential issues for infrared retrievals like the SEVIRI dust AOT are high columnar contents of atmospheric water vapor and the skin temperature, [. . .]".

RC 5: Page 7893, line 22: What does the 65% figure really mean? Looking at Figure 7, I would expect smaller percentages. Is that 65% of emitted mass rather than of total events?

AC 5: This is the relative contribution of DSAs between 0600 and 1200 UTC, averaged over the 2-year period. We agree, the 65% figure does not match Figure 7. The value was acci-dentally taken from Tegen et al. (2013). However, most likely due to remapping to T63 resolution, here, we yield an average contribution of 40% and 45% by morning and af-ternoon dust events, respectively. The main message remains unaffected though. The text is corrected accordingly: Page 7893, lines 22: "[. . .] average contribution of 40% by emission events during morning hours [. . .]". Page 7893, lines 28/29: "Accordingly, more dust emission events are computed between 1200 and 1800 UTC with approximately 45% on average,[. . .]".

Technical comments

RC 1: Page 7884: Typo "budget"

AC 1: Corrected.

RC 2: Page 7885: "two-fold ways" → "two ways"

AC2: Changed.

RC 3: Figure 5: Legends and insets are not legible.

AC 3: The legends and insets are enlarged.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/8/C3209/2015/gmdd-8-C3209-2015-supplement.pdf

---

Interactive comment on Geosci. Model Dev. Discuss., 8, 7879, 2015.



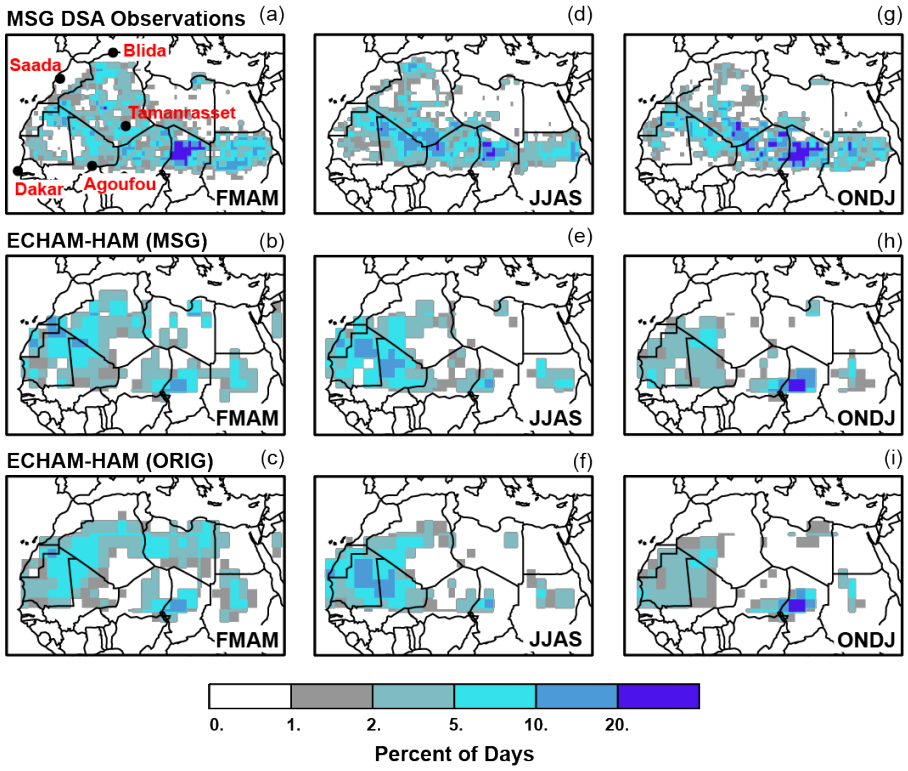


**Fig. 1.** Improved Figure 3

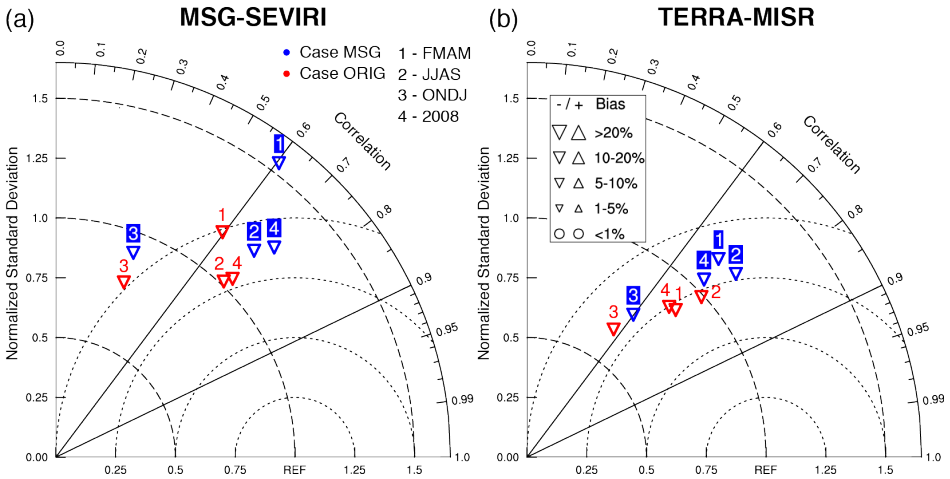


**Fig. 2.** Improved Figure 5