

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

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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 C2059

Manuscript No.: gmd-2015-147, submitted to GMD Title: New developments in the representation of Saharan dust sources in the aerosol-climate model ECHAM6–HAM2
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C3205

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

Anonymous Referee C2059 Received and published: 16 September 2015

General comment The current paper “New developments in the representation of Saharan dust sources in ECHAM6-HAM2” by Heinold et al presented a nice way to correct dust emission biases in global aerosol-climate model. They replaced the original dust source map across North Africa with that from satellite dust source activation observations. The simulated dust emission and aerosol optical thickness (AOT) during 2007-2008 across North Africa has been evaluated with multiple satellite and ground observations. Although the simulated dust emission and AOT shows subtle improvement to the original setup, which is likely due to the poorly represented meteorological field, this paper pointed to a promising direction for bias correction in dust emission.

Minor comments

RC 1: The abstract needs better specification. For example, the opening sentence said that “The model results agree well with AERONET measurements”, but in terms of what? In the second sentence of the same paragraph, “good correlations” refers to spatial correlation? Then it's better to say “spatial correlation” instead of “correlation”.

AC 1: Agreed. In order to specify, in the abstract, we restate the two sentences: “The model results agree well with AERONET measurements especially in terms of seasonal variability, and a good spatial correlation was found between model results and MSG-SEVIRI dust AOT as well as Multi-angle Imaging Spectro-Radiometer (MISR) AOT.”

RC 2: The structure of the method section can be improved. The method section includes model description and an introduction of the MSG-DSA driven dust source approach. These two parts can be grouped under subsections. Later in the results section, the authors introduced multiple satellite products for model evaluation. In my

C3206

opinion, the satellite products should all be introduced in the method (or "data and method") section.

AC 2: Following this suggestion, we split the Methods section into the two sub-sections "Model description" and "New satellite-based Saharan dust source approach". All observations used for model evaluation are introduced in an additional sub-section.

RC 3: On page 7881 line 6, the sentence about the contribution of Sahara desert to global dust load needs a reference.

AC 3: Now, the references Huneus et al. (2011) and Ginoux et al. (2012) are given.

RC 4: On page 7882, the first paragraph talked about the bias in ECHAM5-HAM compared to other AeroCom models. How about the version ECHAM6-HAM2 used in the current paper?

AC 4: Later in the paper (page 7886, line 4), we also give annual emission fluxes for the Saharan dust sources as computed with the current standard version of ECHAM6-HAM2. These are already considerably higher than the values from its predecessor. In order to document earlier model developments, in the introduction, we add the following: "The current version ECHAM6-HAM2 provides global emission fluxes of 948 Tg yr⁻¹ and 552 Tg yr⁻¹ from North African dust sources (2007 – 2008 mean from this study). Whereas the considerable increase in global dust emissions is mainly due to improvements for East-Asian dust sources (Cheng et al., 2008; Zhang et al., 2012), the dust parameterization has remained unchanged for the Sahara, and the higher values result from developments of the driving model ECHAM6."

In this context, errors were found in the computation of yearly totals of modeled dust emission (page 7886, line 4), which are now corrected in the revised paper version.

RC 5: On page 7884 line 28, the 1% threshold for masking out dust source regions needs a justification (or robust test, e.g. how about using 2%?). On page 7885 line 17, the correction factor of 0.86 needs a justification or explanation.

C3207

AC 5: Agreed. The text is complemented, respectively, by: (1) "The threshold value was determined by sensitivity runs with the regional dust model system COSMO-MUSCAT (Heinold et al., 2007; 2011), which is also equipped with the Tegen et al. (2002) scheme updated with the MSG source map. The value of 1% corresponds to four active days per year and, was first suggested by Schepanski et al. (2007). Since then the MSG-based DSA frequency map has been successfully used in the regional model for case studies (Schepanski et al., 2009; Heinold et al., 2011) and multi-year Saharan dust simulations (Tegen et al., 2013)." and (2) "As in the standard setup, a correction factor of 0.86 (Cheng et al., 2008) is applied to the threshold friction velocity for dust emission calculations in both simulations. This non-physical scaling is common in global models to compensate for the effects of resolution on dust emission processes (e.g., lower surface winds) and therefore to ensure reasonable global total dust production (e.g., Ridley et al., 2013)."

RC Technical comments: On page 7907 line 2 in the caption of Figure 4, delete ", " after "Agoufou".

AC: Done.

Please also note the supplement to this comment:

<http://www.geosci-model-dev-discuss.net/8/C3205/2015/gmdd-8-C3205-2015-supplement.pdf>

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

C3208