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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The mineral dust cycle in EMAC 2.40: sensitivity to the spectral resolution and the dust emission scheme" *by* G. Gläser et al.

Anonymous Referee #4

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Overall assessment:

The manuscript aims at an evaluation of two dust emission schemes and the sensitivity to the model resolution in the ECMAC model system. The authors provide a clear recommendation on the model setup regarding dust simulations. However, the manuscript has considerable weaknesses that need to be addressed before this paper can be recommended for publication in ACP.

General comments:

1. The major problem of the manuscript is the lack of quantitative comparison with observational data. There are many statements that one model setup leads to 'more realistic' results compared to a different setup, but this is not based on any sound eval-

uation with observations. In fact, for the only two stations for which the dust concentration simulated by the model is actually compared to measurements, only results for a single model setup are shown. For these cases the results from different model setups could be informative regarding the ability of the model to resolve dust processes.

2. Data that are suitable for comparisons of global dust model results include e.g. satellite retrievals (from MODIS, MISR, TOMS/OMI AI, ...), sunphotometer data from the Aeronet network, dust concentrations from the U. Miami surface network and others, see e.g. Huneeus et al. 2011. Such data are mostly easily available and would help to evaluate the model results, supporting the decision on the use of the best model setup.

3. Such comparisons should also be extended to evaluate the seasonal cycle of dust distributions for the different regions of the world. The seasonal cycle is well known for the different dust source regions, and in particular the results for the different model setups would be of interest.

Specific comments:

4. Section 2.1: The dust emission scheme by Balkanski et al. 2004 should be explained in more detail, particularly because the reference is a book chapter which is not generally accessible. Especially it would be of interest how the fields for threshold velocity and source strength factors were derived, since these factors appear to cause the unrealistically high emissions in the Thar desert. The discussion of the different dust emission schemes is not very useful if this information is not provided.

Section 3.1:

5. Stier et al 2006 already showed comparisons of the different dust schemes (TG and BK) in ECHAM5-HAM, it was already noted there that the BK scheme produces very high dust emissions in the That desert. Proper reference should be given to that publication.

6. The section contains a lot of qualitative and vague statements on the quality of the

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model results, which are not useful for the reader. Some examples:

- ". . .dust emission in India . . . in clear contradiction to published values" Please provide a reference to the mentioned published values.

- "unrealistic high dust loads in polar regions" – on which grounds is this unrealistic? The result is only compared to the mean result of Huneeus et al., but global models can have a considerable range in the transport of trace substances to high latitude. If no measurements are available it should at least be noted if the simulated model value is within the range of Aerocom model results rather than compare to the mean value.

- "The global distributions . . . look quite similar and appear reasonable" – a very crude assessment of the results. On what is this assessment based?

- "Hence we cannot decide which simulation is closer to reality" – If at least an attempt would be made to evaluate the model results with available observations from satellite remote sensing or network data (U. Miami surface concentration data, AERONET sun-photometer data) then at least some chance for a decision on the best model setup based on actual data might be be possible.

"- Because the dust load . . . exceeds the ragne of the Aerocom models . . .." What is that range? More reference to the results of the Huneeus et al. paper should be given.

7. The provided possible explanation for the decreasing dust life times with increasing model resolution is not obvious. Why would the dust particles need longer to reach a precipitating cloud at coarser resolution? Instead, does the precipitation distribution change with model resolution? Are there different rain rates, which may explain different washout efficiencies?

8. As mentioned in the 'General Comments' there are many possibilities to evaluate the dust simulations with observations that are suited for global models, as e.g. done in the Huneeus et al. publication. Many of those data are freely available as the products from the NASA satellite instruments (MODIS, MISR, TOMS/OMI AI), the optical thickness

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data from the AERONET sunphotometer network and the surface concentration data from the University of Miami network.

9. Section 3.2: A closer look at some emission regions is of some interest for the model evaluation. But what is the motivation of choosing to do this only for the Thar and Central Asian deserts? While the Asian deserts are mentioned, why is the model ability to capture the spring maximum not shown? Also, the authors could easily make a direct comparison with TOMS AI, which is mentioned but not shown. Because the comparisons with station data in the next section is for Saharan dust, it would also make sense to evaluate the modelled seasonal changes of dust emissions for the Sahara. In general, evaluation of the full seasonal cycle of the dust in the model would be of interest as this is usually well known for the different regions of the world. Here it would be of particular interest to evaluate the results from the different model resolutions.

Section 4:

10. Timmreck et al., 2004 (JGR, vol. 109, D13202) have compared the performance of the 'free' and the nudged version of the ECHAM model with respect to the effect on dust emission simultions. They found considerable differences, in particular lower wind speeds in the nudged model version lead to partly considerable reductions in dust concentrations compared to the non-nudged version. If this is also the case for this model version then the evaluation of the model performance from the nudged version is only of limited use for the evaluation of the full model.

11. The authors themselves question the usefulness of comparing results form a global model with local, short-term measurement from an individual station. While the model provides astonishingly good results in comparison with the observations, the representativeness of these results must be questioned, particularly for the near-source case in Morocco.

12. The evaluation of the individual episodes could be of some interest if the performance of the different model setup would be compared rather than just showing the

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results for the favoured setup.

13. What is the motivation of comparing to the DREAM results? In particular in Figure 7 it appears that the DREAM model predicts much higher dust loads compared to EMAC, and since it is argued that EMAC concentrations are somewhat too high for this event then the DREAM results must be way too high. So what is learned for the EMAC performance from this comparison?

14. The comparison of the results with the MSG dust index (Figure 7) is not useful. The IR dust index can give an indication of the presence or absence of dust, but the strength of the signal is not necessarily related to the dust load present at a particular location. The IR signal strongly depends on temperatures, humidity and the vertical extension of the dust layer and should therefore no be used for quantitative or even qualitative comparisons of relative dust concentration and emission strengths..

15. The dust episode from the SAMUM period is rather complex, and too much room is given to explain the episode itself, which has been done in detail by Knippertz et al 2009 (please note also the large number of publications on dust measurements during SAMUM published in the Tellus 2009 special issue – available are not only ground measurements but also aircraft data, sunphotometer and lidar measurements of dust distribution) Since different mechanisms are responsible for dust emission during this period it would be of some interest to test which of the processes can be resolved by the model at the different resolutions. But again, it is questionable if a global-scale model is suitable for such detailed comparisons. E.g. dust mobilization by density currents that occurred twice during the SAMUM period are problematic to simulate even with much higher-resolved mesoscale models (see Reinfried et al., 2009, JGR vol 114, D08127)

16. Are LAGRANTO trajectory results computed with EMACS fields or are those results unrelated to the EMACS model? If the letter is the case those model results should be left out here, since they are not informative regarding the EMACS model performance.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 27285, 2011.