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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Modelling soil dust aerosol in the Bodélé depression during the BoDEx campaign" *by* I. Tegen et al.

**I. Tegen et al.**

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The authors thank the reviewers for their very helpful and constructive comments.

Reply to comments by Reviewer 1:

1. We agree that at least in areas with high dust optical thickness including the direct radiative effect of dust will improve the simulation of meteorological variables like T2m. The results in temperature change by dust forcing are likely to be most sensitive to the optical parameters of dust (which were taken from inversion retrievals from the sunphotometer measurements in this paper, but certainly still contain considerable uncertainties). While it would be highly desirable to show sensitivities of the temperature changes due to dust to choices in parameters controlling emission and particle properties, these can only be computed with the full regional model, and current computer

constraints do not permit us to carry out such sensitivity studies. This is however a priority for future investigations, as new computer resources become available.

2. H can vary in the box model between 500 and 1500m.

3. Following to the reviewer's suggestion this sentence will deleted in the revised version.

4. Vegetation cover that inhibits dust deflation plays a minor role in the model region. The vegetation cover is calculated using satellite observations from the Global Inventory Modeling and Mapping Studies (GIMMS) normalized difference vegetation index (NDVI) data sets (Tucker et al., submitted). Soil texture data outside of the preferential source region, where they are derived from the best match of the box model results, are taken from Zobler (1986).

5. The section describing the box model should be clearer with the new additional information provided in the revised version (see also the response to the comments of Reviewer 2). In particular, the parameters used for the 'best case' shown in figure 2 are now documented in the figure caption. In this case we avoided providing a final recommendation, as the results are likely to be specific for the Bodele region, as the box model results partly show only subtle differences for the choice of different dust emission parameters. It may be that the model results match the measurements for the wrong reasons, namely even though the model is able to simulate dust emissions for the Bodele with the chosen parameterization, the observed dust production by self-abrasion of airborne diatomite flakes will requires a new emission model to describe the process physically correct, as mentioned in the conclusions.

Reply to comments by Reviewer 2.

General comments:

1. The different models use the same emission routines described in section 3.1; just the parameters used by the model are changed. Rather than adding a new section, we



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add information in sections 3.2, 4.1 and 4.2 of the revised manuscript to clarify which types of model experiments were performed, and which parameter combination were used.

2. Agreed. The number of layers has some influence on the modelled DOT, as the layer height implies the scales, at which the particles are uniformly mixed. With the simplified box model parameterization the height of the lowest layer must be chosen high enough such that the settling velocity plus dry deposition is does not empty more than the lowest bin, and the height to which dust is vertically mixed is reasonable. The results change with the layer number when less than 6 layers are chosen, while are unaffected in the choice of larger numbers, thus we chose 6 layers as number of layers in the box model. The sedimentation scheme may also affect the DOT, however this was not varied in the model. It may indeed cause some bias as the computation, as the gravitational settling computation expects spherical particles, while the diatomite flakes expected at the field site would be non-spherical.

3. As for comment 1, we did not add an appendix section but added information in the main text to clarify which parameters were used for the model experiments. We hope that this is a satisfactory solution. As in reply to the comment 5 of reviewer 1, we do not wish to overstate the results of the box model at this point, as they are based on a necessarily limited number of observations. While the model matches the observations well, it still may be that the emission model that we use is not really appropriate for the dust emission processes occurring at the site of the Bodele diatomite deposits. Hopefully once additional filter sample data from the BODEX field study become available, the question for the most appropriate dust emission model to be used for this location can be answered with more confidence.

4. The reviewer raises concerns that the importance of the disagreement of the model wind speed with measurements is overstated in this paper, and that discrepancies between DOT observations and model results also the parameters describing the dust emission parameterisation are insufficiently known and tested. While we still feel that

the wind speed is of greatest importance, it is true that the other parameters are insufficiently well determined given the limited measurements that were possible during BODEX. For clarification: the box model experiments presented in Figures 2 and 3 were obtained with using the surface wind speeds that were observed at the field site and thus represent ground truth, and only those experiments were run with varying the other parameters used in the emission model. The text will is being modified in the revised version of the paper to put the statement in a more general perspective.

5. To clarify, the adjustment of the threshold velocities was only done for the surface wind speeds from the regional model, not the observed winds. The tests with different soil particle sizes and binding energies were performed using the locally observed winds without lowering the threshold friction velocity. This should now be made clearer in the modified text in response to the criticism above. The lowering of the emission threshold when using the modelled surface winds is indeed not the ideal solution, but was more successful than using the chosen subgridscale parameterization of surface winds in this case, nevertheless the latter will be the focus of future model developments, as this clearly is the more consistent method to overcome deficiencies in modelled surface wind speeds.

Smaller comments: page 4175, line 5: The value for dust emissions per day during the BODEX event was estimated in Todd et al. (in press) for the area of exposed Diatomite in the Bodele, which is 10,800km2.

page 4175 line23: The value for density of diatomite is taken from Todd et al, in press. It was determined as bulk density of surface samples using non-solvent chloroform as a suspension fluid.

page 4181-4182: The missing reference is:

Heinold, B., J. Helmert, O. Hellmuth, R. Wolke, A. Ansmann, B. Marticorena, B., Laurent, and I. Tegen: Regional Modeling of Saharan Dust Events using LM-MUSCAT: Model Description and Case Studies J. Geophys. Res., in review

page 4182 line 15: The initial and boundary conditions for the regional model LM are taken from the global model GME of the Deutsche Wetterdienst (German weather service). The temperature drop that is found in the model is thus a consequence of larger-scale air movement of the global model.

page 4183 line 16: See reply to general comment 1.

page 4184 line 15: This sentence was changed in the text, to reflect that the size distribution for this time is not used for the computation of statistics A, while I agree with the reviewer that this case is still interesting to show.

page 4184 line 19-21: To be replaced by: 'The retrieval at this day is problematic because of the possibility of stray light saturation of the sky radiances corresponding to small scattering angles. Also, later on March 9, the presence of cirrus clouds was evident, i.e. the presence of cloud contamination in the inversions of the sunphotometer measurements may have biased the results at this time.'

page 4184 line 25: The particle size distributions from the sunphotometer retrievals are reported as volume distribution at 22 particle radii, the comparison with the model results was performed by comparing the relative volume distribution at those radii where the results of the model size bins were interpolated to the radii reported for the retrieval to compute A.

page 4185 line 10: See reply to general comment 3

page 4186 line 3-5: In the model only 3% of the time the wind events exceed 10m/s

page 4186 line 10: 'As above' to be replaced by replaced by 'as described in Section 3.2'; the factor alpha in the regional model was chosen to match the factor alpha from the box model results.

page 4186 line 19 and figure 5: The caption of Figure 5 contained a confusing error, which has been corrected - panel 5a does not show the dust optical thickness computed with observed winds, but with surface winds extracted from the model at the location of the field site. The later start of the red line in 5a is due to the fact that the dust deflation did not start before March 4 in the model when the regional model winds were taken directly as input for the box model. The threshold velocities were 7.3m/s for the regular and 4.8m/s for the reduced threshold velocities at the location of the field site for initializing soil particle movement. The available observations are indeed not necessarily sufficient to give a definite answer to the threshold velocity, as the observations at the field site would reflect local emission as well as transported dust. In the results in Figure 2 which show the results from the box model using observed winds with the threshold wind speed of 7.3m/s the maxima between the daily maximum DOT between model and observations are well matched except for the afternoon of 9 March, when in fact the observations may have been biased by the presence of clouds. The statistics A for the results shown in Figure 5 were 1.0 (5a), 0.69 (5b) and 0.71 (5c), only for the AOT comparison, not size distributions, i.e. the results in 5a are clearly least well matched (case with unchanged threshold velocity). Still, we found the results shown in Figure 5b sufficiently similar to the results from Figure 2 to feel confident that the results with the parameterisation chosen in the regional model simulate the observations sufficiently well such that we feel justified in the conclusions.

page 4187 line 17: The adjustment of the threshold velocity to account for too few high wind events is certainly not ideal, how the reviewer correctly states, the low end of dust emission events is affected as well (ie, dust production may start too early). Still, we prefer this method over increasing the alpha values to adjust the dust emission fluxes in order to get a better agreement in the number of dust events (higher alphas would result in fewer but stronger dust storms in the model for longer time series). The best solution would be to use an appropriate parameterization of subgridscale variability in modelled surface wind friction velocities, which will is an upcoming task.

page 4188 line 15: The text is adjusted according to the reviewers comment, see also above. The reviewer is correct that over a large fraction of the time period considered here the model surface winds match the observations well, however there is a consistent underestimate for high wind speeds when the observed winds exceed 10m/s.

page 4188 line 27: Meant is here radiative forcing at the top of atmosphere.

line 4189 line 6: Sentence to be replaced by: ‘If dust aerosol is included and the radiative forcing by dust permitted to influence model temperatures, we find that the measured daytime temperatures and their change with time is much better matched by the model compared to the model results without dust forcing after March 9 when the DOT was greatly increased (Fig. 9). The underestimate of daytime temperatures on March 4 for the model with radiatively interactive dust is explained by the overestimate in modelled DOT compared to the observations.’

Figure 10: The reviewer is right in that Figures 8 and 9 already show the effect of temperature change through the dust aerosol. However, we would like to keep this figure as it additionally shows the nearly linear change of (hourly) temperatures with dust optical thickness, even with AOTs reaching far above 1 in the model.

Figure 11: The lower panel shows the difference between the modelled annual dust optical thickness for the year 1987 computed with the assumptions in this paper, and the annual dust optical thickness for the same year computed with the parameterization from Tegen et al. (2002). The difference map indicates the changes from using the new parameterization.

page 4191 line 25 - page 4192 line 1: Following the suggestion by the reviewer we will delete this sentence.

page 4192 line 19: This is correct, the effect of the dust is to stabilize surface layer during daytime, while labilizing at night.

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Interactive comment on Atmos. Chem. Phys. Discuss., 6, 4171, 2006.