

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

Anonymous Referee #2

Received and published: 12 July 2006

The paper of Tegen et al is an important attempt to better understand the dust emissions in hot spot areas. It brings together a unique time series of measurements in the Bodele depression and an interesting suite of three models. It certainly deserves publication, although I recommend some clarifications and revisions to be made.

General comments:

- 1) The paper would greatly benefit from a section 3.1 which contains ALL dust emission schemes and any variation of these used in the three models. At least giving the principal parameters varied. I got confused on which alphas, kinetic energies, threshold velocities and source size distributions were used in the different model experiments.
- 2) I wonder if the number of bins, the vertical layering of the different models and

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

the numerical scheme used to describe sedimentation (to name one of the vertical dispersion processes) could influence the agreement between measured and modelled DOT. A discussion is needed.

3) The critical parameters used to produce the modelled DOT time series are insufficiently documented: An appendix could present values used or found in the different experiments for - alphas (ratio dust flux/horizontal flux) - threshold velocity at the measurement site - more documentation of the saltating particles and how they transform to the transported dust - the emitted dust size distribution corresponding to the dust flux F - the atmospheric column height H - particle densities used

4) The authors have certainly done some valuable tests of variations of the dust emission scheme. The authors on page 4191 conclude then that deficiencies can be attributed mostly to "deficiencies in the surface wind speed". I would strongly disagree. This is tested in the paper but not to the extent that other deficiencies might be of relevance. The tuning of the emission model with respect to alphas, threshold velocity and binding energy shows that the bottom up modeling of dust emissions still is not general enough.

5. I was also confused by the citation of Todd 2006 on page 4177 that "dust production occurred only above 10ms⁻¹, which is a higher threshold than those observed earlier...". In the model experiments then, several attempts are made to lower the threshold velocity, and/or a smaller binding energy of the saltating particles. In order to obtain a better agreement with the measurements. I think this is a contradiction with the initial observational finding, that dust production in the area requires rather high winds to be effective.

Smaller comments: page 4175 line5: "Todd suggests 1.2 TG/day emissions": For which area?

page 4175 line23: How was this density determined?

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

page 4181-4182: What are the border conditions used in the LM Muscat runs? How can we explain the realistic drop in surface temperature (without dust) from the 8th to 10th modelled? Is that due to global model forcing at the borders?

page 4182 line 15: I could not find the Heinold submitted reference in the reference list.

page 4183 line 16: "The new parameterization": I was confused which parameterisation is now old and new. See general comment 1)

page 4184 line 15: "...results for 9 march are not used for quantitative comparison...": Well, you comment and show and thus use it. I think the sentence can be omitted and it is rather interesting to document it as well! Discussion is already there. I find rather the statistics A not well documented throughout. Could be reported in more detail in annex.

page 4184 line 19: "at higher Sun" : sentence not clear.

page 4184 line 21: "in the inverted measurement": consider rewrite

page 4184 line 25: How do you derive the statistic A for a size distribution?

page 4185 line 10: see general comment 5

page 4186 line 3-5: "maximum LM wind does not exceed 11.5 ms⁻¹ while 15% of the time measured winds are above 10" Please compute time fraction above 10 in LM as well.

page 4186 line 10: "As above, the emission factor alpha was chosen such that...": I did not find the "above" section referring to that. Is alpha equal in the LM and box model? See general comment 3

page 4186 line 19 and figure 5: "Using lower threshold velocities for initiation of dust emissions improved the model..." Can you document the statistic A? Why is the red line in the figures 5A-c not of equal length? Which threshold velocities are used in 5a

[Full Screen / Esc](#)[Printer-friendly Version](#)[Interactive Discussion](#)[Discussion Paper](#)

and 5b in m/s? Is the comparison in figure 2 really so much better? Are there enough data to conclude on the threshold velocity?

page 4187 line 17: "To account for the underestimation of peak wind speed, the threshold friction velocity was lowered." Somehow this is not logic. Such strategy acts on opposite tails of the wind distribution. It broadens the time window in which dust may be produced. But alpha adjustments and other parameters will cope for not enough high winds as well.

page 4188 line 15: I really wonder if the high winds get too much attention. The peak LM surface winds from 4-8 march are pretty well simulated during the day. The link between the choice of alpha and the wind tail is not conclusive from the experiments presented.

page 4188 line 27: "net radiative flux difference changes"... Where ? at the top of the atmosphere? model?

line 4189 line 6: "is much better matched": Some clarification on which time evolution is better matched would give more value to the two experiments. Especially the 10 and 11 are demonstrating that the dust has an effect. The other days I assume are mainly driven by synoptic conditions and a correct mesoscale model. The low temperature on the 4th of March in accordance with an overestimate of dust in the LM model might be mentioned.

figure 10: Could be omitted. Figure 8 and 9 illustrate nicely the effect on temperature. If kept: Are hourly differences shown?

figure 11: figure caption misses explanation on what is compared here exactly. Which difference between what?

page 4191 line 25 - page 4192 line 1: This discussion is repeated later on and could be omitted here.

page 4192 line 19: "This radiative effect is stabilizing..." Is that needed here??. figure

8 shows that the dust might labilize the atmosphere during night.

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

ACPD

6, S1758–S1762, 2006

Interactive
Comment

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

S1762

EGU