

## Review of ACPD 2012-624

### Introduction:

This modelling study investigates the mechanisms whereby dust is suspended and transported in the Western Sahara by density currents initiated by cold air downbursts - locally named a *Haboob*. The modelling appears to be thoroughly done, and is followed by a very revealing analysis of model output. The work is set nicely in context of larger scale dust transport from desert areas. While there are a few relatively minor weaknesses in the work, it remains worthy of publication after revision.

### General Comments:

- 1) The manuscript is weakened by a number of minor errors in written English. These can be easily remedied with careful editorial work. Some of my technical comments point to specific examples. I stopped making grammatical comments after 10.
- 2) The paper is substantially flawed by the authors' habit of referring to results from modelling as if that was exactly what happened in the world. The matter is further exacerbated by the complete absence of observational data. This is perplexing since there is substantial data from the SAMUN 2006 observational study, already published by Knippertz et al (2007), and therefore publicly available. If the science is to be well-served, those data should be used in the present study. Some of my specific comments point to places where such data should be incorporated.
- 3) There is needless uncertainty introduced by maps and cross sections using either lat./lon. OR model grid km. as horizontal coordinates. One or the other should be used. Some of my specific comments point to places where this occurs.

### Specific Comments:

- 1) Abstract, line 4: Density currents are phenomena, not "mechanisms". Page 21582, lines 21 & 23 repeat this problem. Just to clarify, a *Haboob* dust storm is a phenomenon which may provide a mechanism for dust suspension and transfer into the lower troposphere.
- 2) Page 21583, line 10: It must be explained why the particular case was selected for study. Merely saying that it is "characteristic" is insufficient. The statement "selected because of experimental data availability" clearly is not the reason since no such data are included in the present study.
- 3) Page 21584, line 21: The use of a grid factor of 5 between grids 1 and 2 seems unreasonably large. Can this choice be justified by reference to RAMS/ICLAMS properties?
- 4) Page 21584, line 26: ground to 3 km AGL is hardly the troposphere. Model output later in this paper indicates that the daytime CBL reaches that height. The troposphere is much deeper at these latitudes.
- 5) Page 21585, lines 14 to 16 and Figures 2 and 12: Data from the SAMUN 2006 observational study must be plotted in parallel with the model output. Figures in Knippertz et al (2007) plot such data but only for very limited time series.

- 6) Page 21585, lines 18 to 20 and caption to Figure 5: These statements should be substantiated by production of a profile of the (modelled) local energy budget terms. If the statement is true, then the profile should show dominant evaporative cooling at some elevated layer.
- 7) Page 21587, line 27:  $12 \text{ ms}^{-1}$  seems arbitrary. Why was this value chosen? Would the conclusions be different if a different value were chosen?
- 8) Page 21589, lines 5 to 7: Can this be demonstrated by analysis of model output? I note the difficulty of showing that this “occurred mainly” here since this would imply quantifying its occurrence in all of the modelling domain.
- 9) Page 21589, line 14 and elsewhere and Figures 4 and 9: Dust vertical flux is given in  $\mu\text{gm}^{-2}$ . Emissions fluxes are usually given in  $\mu\text{gm}^{-2}\text{s}^{-1}$ . I cannot understand why time has disappeared. Note that the caption of figure 9 gives flux  $\mu\text{gm}^{-3}$ .
- 10)Page 21590, line 26 & 27: The statement “These results ..... Knippertz et al (2007)” is simply not good enough. The data-model comparison must be shown.
- 11)Page 21590, lines 12 to 18: The authors write about model output as if that is what actually happened. On line 19 they finally state that these are “modeling results”. They should be much clearer about the difference between observations and model output.
- 12)Page 21591, lines 10 to 12 and 17-18: Both statements about dust sources should be substantiated by analyses of model output.
- 13)Figures 4, 5 and 7: The cross-sections shown on Figure 5 must be drawn as lines on Figure 4, or another more appropriate figure.
- 14)Figure 2, panels a) and b): panel a) gives location coordinates in lat/lon., whereas b) used grid km. The two must use the same coordinates since they are explicitly presented for comparison. This may also apply to left and right panels on Figure 3. This applies to all maps in the paper. I suspect grid km would be best.
- 15)The lat.lon. indicators on Figures 5, 7 and 11 should be replaced with grid km.
- 16)The lat.lon. indicators in the caption to Figures 6, 8 and 9 should be replaced with grid km.
- 17)Figure 7: Wind speed contours are barely legible.

### Technical Comments:

- 1) Abstract, lines 13 & 16: The form “uplifted dust” and “produced dust” is improper English. The modifier should come after the noun. These are just the first two examples of a problem that recurs through the manuscript.
- 2) Page 21581, lines 6 & 7: “....there is still limited number...” is grammatically incorrect.
- 3) Page 21581, lines 12 & 24: Emmel et al (2010) is not in the reference list.
- 4) Page 21582, line 18: “at” should be in.
- 5) Page 21584, line 1: The acronyms should be written in full.
- 6) Page 21584, line 3: “.. takes in the account...” is ungrammatical.
- 7) Page 21585, line 7: “mobilization at the region” is ungrammatical.
- 8) Page 21587, line 23: “two well distinct flow areas” is ungrammatical.

- 9) Page 21588, lines 5 & 7: The two vorticity values should be expressed in decimal OR scientific notation, but not both.
- 10) Page 21590, line 3: are, not is.

Douw Steyn  
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