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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Density currents as a desert dust mobilization mechanism" *by* S. Solomos et al.

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Response to Referee Douw Steyn

This modelling study investigates the mechanisms whereby dust is suspended and transported in the Western Sahara by density currents initiated by cold air downbursts - localy 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.

[REPLY]We thank Dr Steyn for the thorough review and useful comments that have substantially improved the manuscript. Replies to the general and specific concerns

are following. The reviewer's comments are in bold and followed by our response.

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.

[REPLY]We would like to thank the reviewer for this point. We made a series of changes in writing style and the manuscript has been revised for language corrections.

2. The paper is substantially flawed by the authorsÊij 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.

[REPLY]The model findings in this work are in agreement with density current theory and laboratory experiments. As expected, the behavior of the system is following the main concepts of a dense fluid moving through a less dense one. The lack of observations inside the propagating density current makes a direct comparison of model findings difficult. However, comparison with satellite and in-situ data exhibited the ability of the model to reproduce the main atmospheric processes throughout the simulation period and domain. A new figure showing the Tinfou station observations from SAMUM campaign and a relevant discussion section has been included in the revised manuscript.

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.

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[REPLY]It is true that the use of both lat/lon and km as horizontal coordinates can be misleading. In Figure 2a the projection of the satellite data is in a lat/lon grid so it is more appropriate to use geographic coordinates. In all other places lat/lon indicators have been replaced with grid km.

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.

[REPLY]Corrected.

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.

[REPLY]All the simulated cases exhibited similar behavior. This particular case was selected because Knippertz et al. (2007) provide a detailed analysis of experimental data for comparison with model results. As stated also in their paper this is a good example of haboob formation in the area mainly due to the isolated nature of the density current which allows a more in depth examination of its main characteristics. The corresponding section has been rephrased in the manuscript and the observational data of the event have been included in the revised version.

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?

[REPLY]Yes the ratio 5:1 is considered as a bit large but for the scales we are looking it can be considered as acceptable. In our test runs we checked for reflection at the boundaries but we did not see such evidence. The ratio 5:1 is recommended in RAMS

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manual under certain circumstances.

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.

[REPLY]Corrected.

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.

[REPLY]The observational data have been plotted in the revised manuscript.

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.

[REPLY]This is a useful suggestion. The significant decrease of the equivalent potential temperature (theta-e) is a good indicator of evaporative cooling. We have included a new plot of theta-e during the formation of the cool pool and the relevant section has been updated in the revised article.

7) Page 21587, line 27: 12 ms-1 seems arbitrary. Why was this value chosen? Would the conclusions be different if a different value were chosen?

[REPLY]We agree that this line can be misleading and we have rephrased it in the text. The direction of the system in Figure 7 is towards the reader. As discussed in the text (e.g. page 21586 line 11, line 25) the speed of propagation remained close to 11 m/s throughout the simulation. The increase in wind speed between 900-880 km in Figure 7 is attributed to the arrival of the density current.

8) Page 21589, lines 5 to 7: Can this be demonstrated by analysis of model output?

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I note the difficulty of showing that this “occurred mainly” here since this would imply quantifying its occurrence in all of the modelling domain.

[REPLY]The area of the density current head exhibited increased TKE values throughout the simulation, implying an increase in turbulent mixing between the density current and the free troposphere. This is also supported by the results extracted in Figure 8d, Figure 9a and Figure 13 that refer to different locations and stages of the density current development.

9) Page 21589, line 14 and elsewhere and Figures 4 and 9: Dust vertical flux is given in μgm$-$2 . Emissions fluxes are usually given in μgm$-$2s$-$1 . I cannot understand why time has disappeared. Note that the caption of figure 9 gives flux μgm$-$3 .

[REPLY]The dust vertical flux is computed as the produced minus the deposited dust flux per model time step. This has been corrected in the text. The caption of figure 9 has also been corrected.

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.

[REPLY]The observations from Tinfou station have been included in a new figure in the revised article and the data-model comparison is discussed in the relevant section.

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.

[REPLY]The manuscript has been modified accordingly in line 12, page 21590 and elsewhere when referring to model results.

12) Page 21591, lines 10 to 12 and 17-18: Both statements about dust sources should be substantiated by analyses of model output.

[REPLY]The absence of local dust production in the model close to Tinfou station in-

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dicates that the abrupt increase in dust concentration during the passage of the system was due to transportation of particles from remote sources. The increase in dust concentrations above 1km after 19:00 in Figure 12 is attributed to both uplifting of pre-existing dust and to dust that was forced outside of the density current head. It is true that the relative contribution of the two mechanisms is difficult to be distinguished. Uplifting of the pre-frontal dust merely explains the decrease in dust concentrations below 1km after 19:00 while the contribution of "density current" dust explains the increase in dust concentrations above 1km.

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.

[REPLY]Done.

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.

[REPLY]Done.

15) The lat.lon. indicators on Figures 5, 7 and 11 should be replaced with grid km.

[REPLY]Done.

16) The lat.lon. indicators in the caption to Figures 6, 8 and 9 should be replaced with grid km.

[REPLY]Done for Figure 9. In Figures 6 and 8 the values are interpolated from the nearest grid points to the specific lat/lon locations, so geographic coordinates are more appropriate.

17) Figure 7: Wind speed contours are barely legible.

[REPLY]The respective figure has been improved for clarity.

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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.

[REPLY]Corrected.

2) Page 21581, lines 6 & 7: “....there is still limited number...” is grammatically incorrect.

[REPLY]Corrected.

3) Page 21581, lines 12 & 24: Emmel et al (2010) is not in the reference list.

[REPLY]Done.

4) Page 21582, line 18: “at” should be in.

[REPLY]Done.

5) Page 21584, line 1: The acronyms should be written in full.

[REPLY]These abbreviations have been declared in page 21581, line 1.

6) Page 21584, line 3: “.. takes in the account...” is ungrammatical.

[REPLY]Corrected.

7) Page 21585, line 7: “mobilization at the region” is ungrammatical.

[REPLY]Corrected.

8) Page 21587, line 23: “two well distinct flow areas” is ungrammatical.

[REPLY]Corrected.

9) Page 21588, lines 5 & 7: The two vorticity values should be expressed in decimal OR scientific notation, but not both.

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[REPLY]Done.

10)Page 21590, line 3: are, not is.

[REPLY]Corrected.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/12/C7986/2012/acpd-12-C7986-2012-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 12, 21579, 2012.