

***Interactive comment on “An integrated modeling
study on the effects of mineral dust and sea salt
particles on clouds and precipitation” by
S. Solomos et al.***

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Response to Anonymous Referee #1

General Comments: In this paper, the authors presented results of a modeling study of the effects of dust and sea salt mixture on clouds and precipitation, using a region climate-cloud resolving model with sophisticated microphysics that include dust and sea salt CCN nucleation processes. They first ran an idealized case to compare the evolution of precipitations in a pristine, and a hazy environment, including the effects from topography. They then ran a test case to simulate a dust storm over the

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Eastern Mediterranean to compare the processes of cloud and precipitation formation with aircraft observations. Further, they carried out sensitivity experiments to test the importance of hygroscopicity of dust, and ice-nucleation in affecting rainfall and cloud formation. Finally, they carried out 9 cases to test the sensitivity of their model accumulated precipitation to various combinations of aerosol and aerosol-cloud and aerosol-radiation interactions. They concluded that increasing the percentage of dust salt mixture can result in more vigorous convection and rainfall rate, and that including realistic dust-salt parameterization reduces the model bias for predicting 24 hour accumulated precipitation significantly. . . . This is an important paper, demonstrating for the first time the need for inclusion of interaction of meteorology with realistic aerosol parameterization, and feedback processes in assessing the impacts of aerosols on regional weather and climate simulations. However, the paper is difficult to read, because of poor organization, and bad figures. A large number of experiments were carried out, and they are described continuously in the body of text, without subheading breaks. Following the discussion in relation to the figures is very frustrating. The paper needs major revisions and rewrite for clarity, before it can be accepted for publication.

[REPLY] We would like to thank the reviewer for the thorough and thoughtful comments. As suggested, the paper has thoroughly restructured, shortened at parts and the figures have been improved. The bulk of the model development section has been transferred to an appendix, leaving a brief summary for the main text.

Specific comments:

1. The paper is too long. A reader has to go through more than 10 pages of background discussion and model details before the experiments and the results are presented. The abstract reads like an introduction, and includes only brief mention of results. The results on impacts of topography, GCCNs, and the East Mediterranean simulations are not mentioned. The detailed description of the model configuration and dust and sea salt parameterization, including Table 1 and 2 could be put in Supplementary Material, to reduce the distraction to readers, who are interested in the main results, . . . but not

necessarily the model details.

[REPLY] The results for GCCN and topography are now included in the abstract. Table 2 is now omitted in the revised manuscript. Although RAMS is a well known atmospheric model, RAMS/ICLAMS includes many novel features that require some description before the simulations are presented. We do understand that the length and placement of the model description in the original manuscript was distracting. So, the descriptions of dust, salt and deposition schemes have been moved to Appendix, where the interested reader can find this useful information while the one who is familiar with it is not bothered

2. The objective of the paper is not clear. The discussion seems to teeter between model documentation/ testing, and attempt at unraveling new science. The authors should state clearly, before presenting their results, what are the objectives of the paper, their approach, and rationale for each set of experiments.

[REPLY] The main objectives of the paper have been stated clearly now at the end of the introduction section.

3. The description of result should have subsection headings to break up the separate discussion to make it easier for the reader to follow: Section 3.1 deals with three subtopics: pristine v. hazy cases, GCCN, and topographic effects. Section 3.2 should have separate headings for: comparison with aircraft data; Exp 1 through Exp 3; and a separate subheading for the 9 scenarios.

[REPLY] Good point. The revised manuscript is now divided into more sections as suggested.

4. The paper has too many figures. Many of them are of poor quality, and some of them are not necessary. The authors should examine each figure and make a real attempt at reducing the total number, and ensuring each present a clear message. The following are suggested revisions.

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[REPLY] The figures have been redrawn for clarity. Three figures have been removed (Fig5, Fig10 and Fig17) while two have been combined (Fig4 and Fig6).

Fig.3: contours are too tight, the wind arrows are too small. The horizontal axis are missing or covered up by the overlapping panels in the version I downloaded.

[REPLY] Fig.3 has been redrawn.

Fig.4 and Fig.5 convey the same message. Only one is needed.

[REPLY] Fig5 has been removed.

Fig.7: Labels on the x- and y- axis are too small. What is the maximum height of the topography.

[REPLY] Fig.7 has been revised accordingly. The maximum topography height (700m) for the “complex terrain” simulation is now mentioned.

Fig. 8: the arrows on the streamlines are too small to be seen. The streamlines over the continent are invisible.

[REPLY] Fig.8 has been revised accordingly.

Fig. 10: The scale of Fig. 10a and b are different; the dark, green and light blue regions are not defined. The ocean land boundary is ill defined. The exact geographic locations depicted by the two regions are not clear. The dust concentration cannot be seen coincident with the humidity surface. The one-sentence description (line 468-470) refers to clouds? Where are the clouds?. This figure is not acceptable. It can actually be omitted, or redraw with more clarity.

[REPLY] Fig.10 has been removed from the revised manuscript.

Fig. 11: The figure could be misleading because the vertical and horizontal scales are not the same in Fig. 11a, and 11b, and the sea salt concentration contours are too dense. There is no discussion of the why the maximum concentrations of dust and sea

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salts are where they are. The only reference to this figure is that the dust and sea salt did not elevate higher than two kilometers. The authors can simply state this result, without the figure.

[REPLY] Although we understand the point made, we would like to keep Figure 11, as it provides valuable information on the vertical structure of the dust storm and the coexistence of dust and salt particles near cloud base. We have improved the quality of the figure, and added more discussion in the text.

Fig. 13: Labels for x- and y- axis are missing. The geographic location is not clear. Is the solid black line the land-sea boundary? The symbols for the aircraft location are too small.

[REPLY] Issues addressed.

Fig. 14: the ice-mixing ratio contours are too dense; the labels on the x- and y-axes are too small, and unreadable.

[REPLY] Issues addressed.

Fig. 17 is unnecessary. The mean bias scores can be specified in paranthesis, after the labels in Fig. 16.

[REPLY] Fig.17 has been removed from the revised manuscript while the mean bias scores are specified after the labels in Fig16.

Editorial comments

1. P2. Line 32-40: This part should be shortened or absorbed in the introduction. More detailed description of the results should be included in the abstract.

[REPLY] Done.

2. P. 3, Line 82-89: Here the authors discussed literature on dust impacts on global and regional climate. They missed reference to a study that showed that Saharan dust

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radiative heating can induce a teleconnection pattern spanning Eurasia and the North Pacific (Kim et al. 2005). Also missing are references to radiative effects of dust on the Asian monsoon (Lau et al. 2006, Lau and Kim 2006), and impact of Saharan dust on West African monsoon and Atlantic climate (Lau et al. 2009)

[REPLY] Thank you for pointing out these references. The current paper focuses on the ICLAMS development and the effects of dust and salt particles on clouds and precipitation. Although very important, some studies mentioned (e.g. on teleconnection) are outside the scope of this paper. We added the most relevant from the suggested ones.

3. P. 4, Line 103-106: Here the authors described the absorption of solar layer by dust, and resulting heating of the dust layer and resulting modification of the “thermodynamic” structure of the atmosphere. The modification should be both “thermodynamic and dynamical” structure of the atmosphere, because not only temperature and moisture, but also clouds, rainfall and circulation and are modified by Saharan dust outbreaks. Please refer to Lau et al., (2009) and Wilcox et al (2010) which provided modeling results and observations, attesting to impact on the regional water cycle by Saharan dust.

[REPLY] “Thermodynamic structure” has been replaced by “thermodynamic and dynamical structure”. The suggested references have been added in the revised manuscript.

4. P. 14-15, line 390- 422: The result here indicating that topographic forcing is more important than aerosol microphysics. This is an important result and should be stated in the abstract. It follows that validation of aerosol effects in models should be taken over flat terrains. In the simulations, the authors stated that they used a 3 ms⁻¹ “western flow”. Should it be “westerly flow”? If so, why is there no precipitation at the upwind side of the idealized hill, where there should be forced upward motion? Or, does the induced flow go around the hill, and leading to moisture convergence in the lee side of the hill?

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If the mountain height is raised, what happens to the location of the precipitation? A plot of the streamlines of the induced flow pattern will be helpful.

[REPLY] “Western flow” has been replaced by “westerly flow” in the revised manuscript. The reviewer is also correct about the location of maximum precipitation. Due to the relatively small size of the hill there was no significant perturbation at the upwind side of the hill. The flow diverged around the hill and moisture convergence occurred at the lee side of the hill (see the attached figure at the end of this text). Many more relevant tests and experiments have been conducted in order to examine the response of the system to various surface properties (topography, landuse etc). However, our purpose here was just to illustrate the dramatic change in precipitation due to topographic variability. A more in-depth analysis of each particular case is out of the scope of this paper.

5. P. 15, line 445: Please state which results described in the papers, are based on the 15-km grid, the 3-km grid and 750m grid respectively.

[REPLY] The comparison with aircraft measurements (Fig.9 on the revised manuscript) is based on the 3-km grid as implied also by the location of the measurements (Fig.10 on revised manuscript). The cloud vertical profiles of Fig.11, Fig.12 and all precipitation results are based on the 750m grid. This clarification has been added in the paper.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 23959, 2010.

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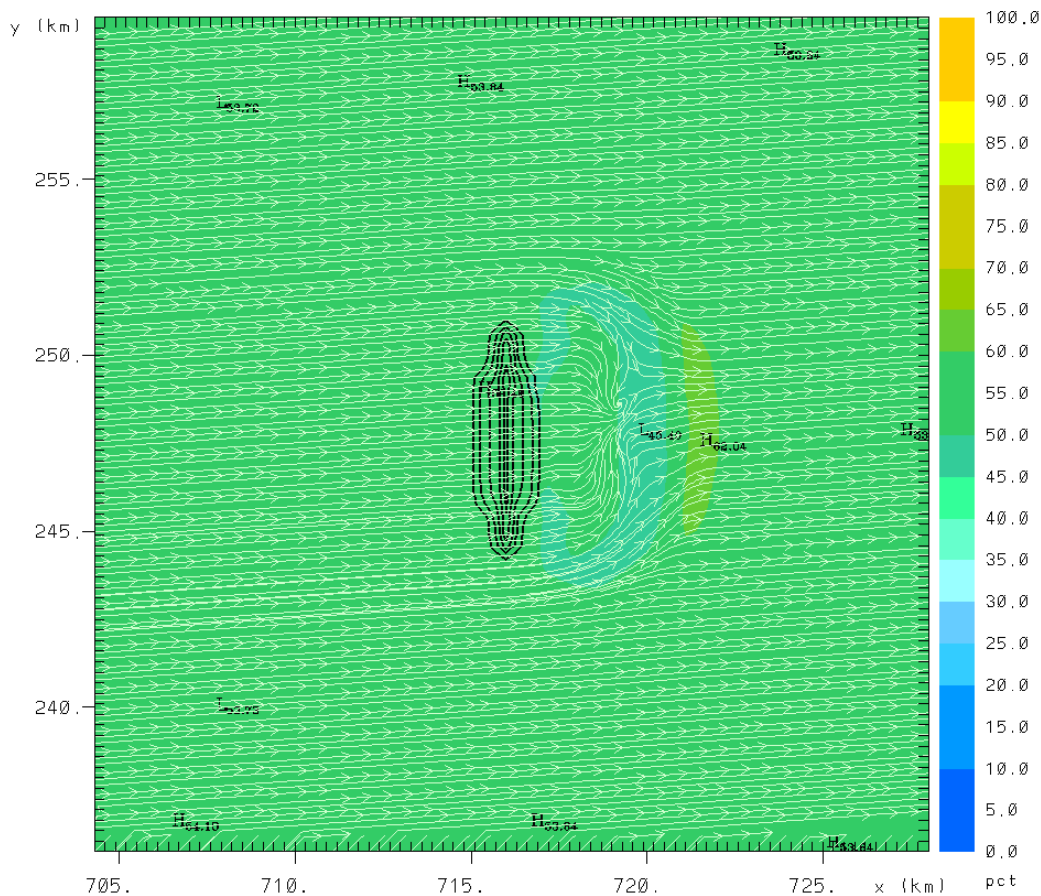


Fig. 1. Relative humidity (color palette), topography (black contours) and streamlines at 925 mb relative to Figure 5 of the revised manuscript

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