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Atmospheric Chemistry and Physics Discussions

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.

Anonymous Referee #1

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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 prestine, and a hazy environment, including the effects from topography. They then ran a test case to simulate a dust storm over the 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-nulceation 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 meterology 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. 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. 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.

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

ACPD
10, C8859–C8863, 2010

separate subheading for the 9 scenarios.

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. - 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. - Fig.4 and Fig.5 convey the same message. Only one is needed. - Fig. 7: Labels on the x- and y- axis are too small. What is the maximum height of the topography. - Fig. 8: the arrows on the streamlines are too small to be seen. The streamlines over the continent are invisible. - 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 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. - 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 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. - 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. - Fig. 14: the ice-mixing ratio contours are too dense; the labels on the x- and y-axes are too small, and unreadable. - Fig. 17 is unnecessary. The mean bias scores can be specified in paranthesis, after the labels in Fig. 16.

Editorial comments: 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.

ACPD

10, C8859–C8863, 2010

Interactive Comment

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 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) P. 4, Line 103-106: Here the authored 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.

1. Kim, M. K., K. M. Lau, Mian Chin, K. M. Kim, Y. C. Sud, G. Walker, 2006: Atmospheric Teleconnection over Eurasia induced by aerosol radiative forcing during boreal spring,. J. Climate, 19, 4700-4718. 2. Lau, K. M., M. K. Kim, and K. M. Lau, 2006: Aerosol induced anomalies in the Asian summer monsoon: The role of the Tibetan Plateau. Climate Dynamics, 26 (7-8), 855-864, doi:10.1007/s00382-006-0114-z. 3. Lau, K. M., and K. M. Kim, 2006: Observational relationships between aerosol and Asian monsoon rainfall, and circulation, Geophys. Res. Lett.. 33, L21810, doi:10.1029/2006GL027546. 4. Sun, D., K. M. Lau, M. Kafatos, Z. Boybei, G. Leptoukh, and C. Yang, 2009: A numerical simulation of the impacts of African dust aerosols and associated Saharan air layer on Atlantic tropical cyclone development. J. Climate, 22, 6230-6250, doi:10.1175/2009JCLI2738.1.

5. Lau, K. M., Kim, K. M., Sud, Y. C., and Walker, G. K., 2009: A GCM study of the response of the atmospheric water cycle of West Africa and the Atlantic to Saharan dust radiative forcing, Ann. Geophys., 27, 4023-4037, doi:10.51941 Angeo-27-4023-2009. 6. Wilcox, E., W. K.M. Lau, and K. M. Kim, 2010: A northward shift of the

ACPD

10, C8859–C8863, 2010

Interactive Comment

Inter-tropical Convergence Zone in response to summertime Saharan dust outbreak. Geophys. Res. Lett, 37,L04804, doi:10.1029/2009GL041774

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

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.

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