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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The effects of mineral dust particles, aerosol regeneration and ice nucleation parameterizations on clouds and precipitation" *by* A. Teller et al.

Anonymous Referee #2

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The manuscript titled 'The effects of mineral dust particles, aerosol regeneration and ice nucleation parameterizations on clouds and precipitation' by Teller et al., presents a model sensitivity study regarding the role of airborne dust on cloud processes. The study focuses on a specific event at the area of eastern Mediterranean that combined convective activity and transportation of dust. Several important issues have been addressed, including the role of aerosol concentration and size distribution on cloud development, sensitivity of ice production towards two different ice-formation parameterizations as well as the importance of aerosol regeneration from the evaporation of cloud droplets. However, the lack of comparison of model output with observational data does not allow the extraction of general conclusions regarding the above inter-

actions and limits the scope of the manuscript to a model intercomparison study. I recommend the publication of this manuscript after the authors address the following comments including technical and language corrections.

Major comments:

1. In the introduction, the authors describe the modeling study as a "real mode three dimensional set up". However it seems that the WRF model version used in this study does not include an online dust scheme and aerosol properties were implicitly imposed by the users. The bin-model was actually initialized with a homogeneous radiosonde derived from the coarser grids.

2. Moreover the downscaling modeling technique that was adopted does not allow the feedbacks of the high resolution domain to interfere with the coarser ones thus limiting the possible dynamical effects (cold pools, wind shear etc.) to the small area where the bin-modal approach was used. A two-way nesting approach would be preferable.

3. The location of the aircraft measurements in Levin et al., 2005 does not coincide with the location where the high resolution model was applied. These values could be considered as typical values during such episodes in the area – however this should be clarified in the text.

4. The relatively small size of the outer domain and the use of FNL $1^\circ \times 1^\circ$ data results in a somehow weak boundary forcing (about 9×20 boundary grid points every 6 hours). This could probably lead to improper description of atmospheric properties during the case study. Although in the end all model sensitivity tests were initialized with the same single radiosonde, it would be useful to provide a short evaluation of model output for consistency.

5. "The aerosol size distributions at the boundaries are kept constant....". This is probably not the case during an intense dust storm as these values are expected to vary significantly in space and time. What is the frequency of aerosol boundary conditions

update?

6. "... the inclusion of aerosols in the bin microphysics calculations resulted in significant differences between the bulk and the bin cloud microphysics simulations." To my understanding, the authors here are comparing model output from the high resolution (1x1 km) bin-model domain with output from the external bulk-model domains. If this is the case, these differences could be as well attributed just to the increased model resolution. It is probably necessary to perform one more run using the bulk microphysics scheme for the high resolution grid and use this as a reference.

7. I found it difficult to interpret the results presented in Table2. "....the lowest normalized variability values were calculated for the cloud top heights (only 0.7% variance compared to the average)...." , but the corresponding value in Table2 is 0.01 (1%). "....The normalized variability of the average precipitation per grid box is 2 %,...." but the value in Table2 is 0.03 (3%). Please clear this out.

8. How is temperature included in the construction of Fig.9? Is it the average domain temperature? Please clarify.

9. The concept of aerosol regeneration is interesting and not often addressed by similar studies. Inside the zoom rectangular area it seems that cloud conditions dominated throughout the modeling period. For the comparisons referring to "no-cloudy" conditions (for example Fig.13) what is the percentage of these model grid points with regards to the "cloudy" ones?

10. In general, the study is somehow limited to model sensitivity tests. A comparison with any available observations (sattelite, ground measurements etc.) would strengthen the authors arguments and improve our understanding on such complex processes.

Minor comments:

1. Several technical corrections and language revision by a native speaker would im-

prove the quality of the text.

2. Line 25: "southern North America"

3. Section 3.1: Replace "domain of 244X136X40...." with 244$\times$136$\times$40" (use symbols)

4. In section 4.2 replace BC,G,M,reg with BC_G_M_reg – similar with the next cases.

5. Sections 4.2, 4.3, 4.4 have the same titles.

6. Figure 3: The color contours are practically unreadable. Please use less contour increments and replace the linear color bar with a sectional one with one single color for each contour value.

7. Figure 4 : Same as in Fig.3. It would also help to zoom in the selected rectangular area.

8. Figure 11: Same as Fig.3. Use single color for each contour value. Also remove "Fig.12" on top of the figure.

9. Figure 13: Same comment for the color bar as in Fig.3. Also extend the maximum value to more than 300 cm-3 so that the time when the maximum difference occurs can be easier identified.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 8225, 2012.