

Interactive comment on “Saharan dust event impacts on cloud formation and radiation over Western Europe” by M. Bangert et al.

Anonymous Referee #2

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The Bangert et al. manuscript describes simulations of the impacts of a Saharan dust event on cloud formation and radiation in Western Europe using a regional coupled model with advanced cloud microphysics parameterizations. The manuscript does a nice job of describing the microphysics parameterizations and how they impact cloud droplet and ice number concentrations as well as radiative transfer. The manuscript is clearly written and is a great example of the application of a coupled regional model to a high PM event. It would be a nice contribution to ACP once the comments below are addressed.

General comments

While the paper does a good job of describing differences among model simulations, very little attempt is made to compare predictions with observations. The model eval-

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uation consists largely of one unspiciated PM₁₀ time series comparison (Figure 2) and a qualitative cloud-top temperature comparison with MODIS data. I have no sense if the many detailed model predictions for various number concentrations and radiation fluxes are in the range of the values that actually occurred during the episode. In several places, it is mentioned that the model bias for 2-m temperature at unspecified site(s) is improved compared to a weather forecast not presented in the manuscript. The improvement in bias is somehow based on a personal communication and the observation data is not presented in this manuscript. I would highly recommend that the authors do their best to find any available data to test aspects the model simulation. Without such an evaluation, the manuscript seems to be more of a model intercomparison and sensitivity study than an examination of an actual atmospheric event.

Based on the relatively short spin-up time used, I am curious about the runtimes associated with the various model simulations. Is this model computationally efficient enough to be applied for seasonal or longer periods? I recommend adding runtime to the table that list the simulations.

Related to the question of computational efficiency, I wonder if the level of detail associated with some of the microphysics parameterizations is warranted considering other model simplifications. For example the model apparently does not simulate the chemical aging of the dust particles (i.e., dust particles seem to be assumed dry or 10 percent soluble at emission). Considering this simplification as well as uncertainties in emissions, transport, updraft velocity, temperature, size distributions, etc., is the iterative solution for $s_{\max}$ described on p. 31945 warranted? I think the approach of Abdul-Razzak and Ghan avoids this iteration at the expense of deviating somewhat from what would occur in a Lagrangian cloud parcel. If the cloud microphysics calculations significantly reduce computational efficiency, it may be worthwhile to simplify them for some applications. Related to my point above about the lack of model evaluation with observations, it is unclear whether the highly detailed parameterizations truly improve predictions given other modeling uncertainties.

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Specific comments

p. 31941, lines 6-9: This statement suggests that Kumar et al. discovered the behavior in question whereas studies explored the CCN behavior of insoluble dust prior to the Kumar et al. paper

p. 31942, lines 23-24: Clarify what is meant by "are allowed to interact with anthropogenic emissions of particles and gases"

p. 31942 lines 26-27, p. 31943 lines 1-4: Accurately simulating dust emissions and the associated meteorology for a major dust event seems like a challenge task. The authors should expand the description of dust emissions and its validation since it is central to the study and does not seem straightforward.

p. 31950 para. 1: Please clarify the treatment of dust aging in the model. Does the model simulate dynamic condensation of acids onto dust and chemistry with reactive dust components that can enhance particle solubility during transport?

p. 31950 line 16: Is the unified approach of Kumar et al. needed here? Is the FHH theory approach invoked when the dust particles are emitted with significant soluble content?

p. 31951 lines 7-10: Why not show a line for total PM10 as predicted in the D1 simulation domain in Figure 2?

p. 31951 line 18: It would be good to show the Atlas mtns in Figure 1

p. 31954 lines 14-18: I have not read the Kumar et al. paper, but earlier studies have found that water competition involving dust is insignificant when the number of dust particles is small compared with the total number of particles. It seems that the authors are suggesting otherwise here, although this section is somewhat confusing because the labels in Figure 5 are too small for me to read in either of the ACPD print formats.

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p. 31960 lines 9-10: Please add the observations to the figure to support the statement that observations and predictions converge.

p. 31961 lines 9-12: As discussed above, the model evaluation in the manuscript is not very thorough to support this statement.

p. 31961 line 23-25: The assumption that dust particles are emitted with 10 percent soluble coating is not realistic so this conclusion is not overly meaningful.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 31937, 2011.

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