

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

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We thank the reviewer for the very detailed comments and suggestions. Our responses to specific comments follow.

» 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 evaluation consists largely of one unspciated 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.

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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.«

We included the figure (Sect. 1 §7; Fig. 1 in the revised manuscript) showing the comparison of the operational forecast with measurements. We excluded it in the first time, because it is a special evaluation including several forecast simulations to identify biases as a function of forecast time. Therefore it cannot be technically reproduced with the presented sensitivity simulations. Nevertheless, the magnitude and daily cycle of the bias can be compared with the temperature differences found in our simulation to identify possible reasons for the bias.

To further evaluate the simulated aerosol distributions, we included a comprehensive comparison with PM10 measurements for the whole model domain. (Sect. 4 §2; Fig. 4 in the revised manuscript)

» 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. «

The individual simulations for domain D1 do not differ significantly in terms of runtime, because the full set of processes related to aerosol-cloud-radiation interaction is used in each of them and only the different contributions of dust are switched on or off. The runtime of the model is not applicable for longer periods in the used setup. (28 CPU-hours for 1 h simulation time on a parallel HPC system, HP XC3000 <http://www.scc.kit.edu/dienste/hc3.php>) Our intention was to use a comprehensive representation of the processes related to the interaction of aerosol with the atmosphere to resolve as many potential feedback processes (on the daily scale) as possible. Nev-

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ertheless, we are currently developing a less expensive version for precisely seasonal 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. «

The additional computational burden of the iterative solution for s_{max} is negligible in comparison to processes like gas-phase and aerosol chemistry, for instance. Nevertheless we agree with the referee that every improvement in run-time is important in order to apply the model for longer periods. But, in the presented study the iterative solution is necessary because the activation of very large particles (like mineral dust or sea salt) is strongly influenced by kinetic effects which cannot be represented adequately in e.g. the parameterization of Abdul-Razzak and Ghan (2000) (which is also included in COSMO-ART for the reasons mentioned by the referee).

» 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 «

Kumar et al. identified the physics behind CCN activity of freshly emitted dust to ad-

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sorption and corrects an erroneous assessment that solubles in the dust are responsible. We added an additional sentence and reference to clarify this (Sect. 1 §6).

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

We reformulated the sentence to be more specific.

» 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.«

We added a more detailed description of the treatment of dust emissions (Sect. 2 §1).

» 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? «

We added a sentence to clarify the treatment of dust aging (Sect. 3 §2).

» 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? «

As shown by Kumar et al. 2011 the CCN activity of dust particles with a soluble fraction in the order of 10 % depends on both the hygroscopicity of the soluble content (described by Köhler theory) as well as adsorption effects (described by FHH theory). For this reason the unified approach is necessary to describe the activation of coated dust adequately in simulation C*.

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

We included the line in Figure 2 (Fig. 3 in the revised manuscript).

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

We highlighted the Atlas Mountains in Figure 1 (Fig. 2 in the revised manuscript).

» 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 «

We added some text for clarification. (Sect. 4.2 §5) We cited Kumar et al. to point out that coated dust particles have a larger potential impact on the maximum supersaturation. In the following lines we quantify the impact in the presented study. The results show that there is an impact on supersaturation, especially the number of gridpoints with high s_{\max} is decreased significantly. Grid points reaching high s_{\max} in the reference case have typically low aerosol concentrations, as a consequence the number of dust particles is high enough in comparison to the total number of particles to have a significant effect and buffer the impact of the activated dust on cloud droplet number.

We apologize for the small labels in Fig. 5. They are very small in the discussion format (due to the small available page width), but will appear larger in the final version.

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

We added the figure showing the forecast and the observations (Fig. 1 in the revised manuscript; see response to first comment).

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

We added a comprehensive comparison of PM10 measurements in Europe to justify this statement (Sect. 4 §2; Fig. 4 in the revised manuscript).

» 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. «

We did not say that dust is emitted with 10 % soluble coating, but rather it has aged and acquired it when in the domain of interest (e.g. Germany). The 10 % is treated as a sensitivity study and is consistent with coatings in aged dust sampled in the Eastern Mediterranean (e.g. Levin 1969).

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

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