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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Can explicit convection improve modelled dust in summertime West Africa?" *by* Alexander J. Roberts et al.

Anonymous Referee #2

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The paper's goal is to understand whether including explicit convection improves the modeled dust in summertime West Africa. In the model used, there is an improved diurnal cycle, but the average dust aerosol optical depth is only slightly modified with explicit convection because the increased evening dust is balanced by a reduction of morning dust (associated with the breakdown of the low-level jet). The results show increases in the frequency of the strongest winds but they are still weaker than observed. Finally, the authors conclude that their study is limited due to other model problems such as the poor representation of the land surface condition in the Sahel, where haboobs are frequently generated in summer.

Although some of the results of the study are interesting, I have some concerns with respect to the formulation of the research question, the experimental set up and the

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interpretation of the results. I summarize these concerns below.

1) Research question: In my opinion, the content of the paper cannot respond to the question posed in the title: "Can explicit convection improve modelled dust in summertime West Africa? What the study shows is that errors from neglecting explicit convection are of second order compared to other model errors, and the conclusion can only apply to the limited area version of the UM of the UK Met Office used in the paper. There may be other models with a similar behavior but this is not shown in the paper, and extrapolating would be speculative. It would be convenient to modify the title, otherwise it can be confusing for the reader (in terms of what the reader expects by reading the title). The same happens in the abstract and the paper: it has to be clear that these results are specific to the UM.

2) Figures 2 and 6 show a very poor behavior of the model (regardless of explicit convection) when compared to observations. For example, there is a factor 3 to 4 difference in the AOD and an uncorrelated seasonality when comparing the model to the MODIS observations. I have several questions in this respect:

a. To what extent the retrieved AOD form MODIS is reliable over land? Some measure of uncertainty in the observation is needed when using AOD products over bright surfaces.

b. Have the model outputs been spatially and temporally collocated with the MODIS data in order to perform the comparison (i.e. did you select the modeled times and places corresponding to the availability of the MODIS data?) If not, to what extent your comparison can be affected? My experience is that it matters a lot. Would this make sense with your current model set up, i.e. a regional climate run only fed by the analysis data though the boundaries?

c. Why AERONET stations were not used? There are quite a few stations in the domain for 2011. AERONET is reliable and is the main tool used to evaluate model performance. Without the AERONET evaluation is difficult to judge the performance of

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this model compared to other models. Nowadays many regional models represent reasonably well the seasonality of dust in AERONET stations (daily correlations between 0.6 and 0.8 when reinitialized daily and without dust data assimilation). There are also available high resolution PM10 surface observation concentrations for the Sahelian Transect (Marticorena et al 2010) that would really help evaluating the model.

d. Concerning the previous point: in the introduction the authors claim that both winds and dust should be explored together with observations. It is surprising that the authors do not use the most reliable resources of dust measurements besides the more uncertain satellite products.

e. Concerning the general decrease of dust in the model from May to September (compared to the observations showing a peak around July): Given that the model is not reinitialized every, has the humidity in May 1 been warmed up for at least 1 year? If not, this could be a reason for such behavior (the model could be showing a trend in dust because of a drift in the soil humidity). Has the model been evaluated for the same time period reinitializing the atmosphere and the soil every day from the parent domains?

f. More details should be given on the emission scheme. Do the authors use a preferential source? Do they use estimates of aerodynamic roughness length? This may also at least partly explain such a mismatch with observations.

3) A major question: because convection seems to be a second order error in this study, can we really respond to the question posed in the title?

4) What can explain that the 12 Km explicit and 4 km explicit are so similar?

5) In Figure 7, the 12-km explicit has a more prominent tail of high winds compared to the 4-km explicit. This behavior is surprising to me. What does explain this behavior? Is the frequency at a specific location comparable using different model resolutions? That is not really clear to me.

6) Figure 12: Does it make sense to compare the model for a specific day for this experimental set up? Reproducing a specific episode requires (recent) and accurate initial conditions and the model is running in a regional climate mode only constrained through the boundaries.

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