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Atmospheric Chemistry and Physics Discussions



# *Interactive comment on* "Impacts of increasing the aerosol complexity in the Met Office global NWP model" *by* J. P. Mulcahy et al.

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The paper "Impacts of increasing the aerosol complexity in the Met Office global NWP model" tests the impact of aerosols on NWP. The authors not only compare the sensitivity of the meteorological forecasts in terms of the various degrees of complexity in the aerosol approaches that are employed, they also spend some effort in evaluating the capability of the modeling system to simulate the aerosol concentration with some degree of confidence. I find this paper very interesting and thorough in almost all aspects. It should be published. I have only minor comments. First, I think it should be stated clearly – at least in the conclusions – that the results for the indirect aerosol effects maybe somewhat tentative. The convective parameterization does not interact



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with aerosols. In general the impact of aerosols on convection is still grounds for much scientific discussion, and inclusion of that interaction process is only beginning to get some interest from convection parameterization developers. Second, I think a short paragraph would be helpful in section 2 that gives a little more info – and makes it easy to find for the reader - on how the interaction processes are handled. What is used to calculate aerosol optical properties? Is a look up table used or are there online calculations? What is the level of complexity of the microphysics scheme? It does not predict number concentrations of droplets, so it is a single moment scheme? I assume that the modeling system includes wet-scavenging by large-scale precipitation and non-resolved precipitation as well as settling? If not, that should be mentioned. None of my minor comments should have any influence on the quality of the paper and the finding of the authors.

Some specific comments:

Abstract: If you make a point out of improving the simulations in tropical regions, maybe you should mention that the aerosol indirect effects are not included in the convective parameterization.

Pg. 30457, line 11 – 14: That paper was not looking at aerosol interactions versus no aerosol interactions, but only at the impact of a very strong signal from biomass burning (versus no biomass burning). This was done with cloud resolving simulations that included complex chemistry. And the only improvement we could show was in the lower troposphere temperature, moisture and wind forecast. There was no comparison to CAPE with observations. So I would phrase it more like "in high resolution weather forecasting models MAY improve forecasts. . .", and take the CAPE out.

Pg 30459, line 1-4: Is wet scavenging included in the parameterization?

Pg. 30462, line 13: Aerosol ice interactions are not included. Would you expect this to make a large difference? Could it influence the results that you got for the summertime polar region?



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Pg. 30477, line 15-19: Here is the only place you bring up the possible deficiency with the aerosol indirect effect. This should also be mentioned in the conclusions and should also have an impact on the abstract.

Pg. 30482, line 20: If some type of aerosols (even simple versions) are included, volcanic ash can also impact NWP and can easily be added. It would almost have to be added, since otherwise any assimilation of AOD may lead to really strange aerosol concentrations.

Pg. 30483, line 22-25: The results for the polar regions are really interesting and revealing. Do you think that inclusion of interaction between aerosols and ice will lead to any qualitative differences?

Fig. 13: It is hard to see (for me) what really is better where. Fig. 17: I cannot read the winds on these figures.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 30453, 2013.