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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Chemically aged and mixed aerosols over the Central Atlantic Ocean – potential impacts" *by* M. Astitha et al.

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Reply to the reviewer's comments

Interactive comment on "Chemically aged and mixed aerosols over the Central Atlantic Ocean – potential impacts" by M. Astitha et al.

Anonymous Referee #2 Received and published: 31 March 2010

This work explores the origin and fate of continental aerosols transported over the Central Atlantic Ocean in terms of number and size distribution and chemical composition. The methods used involve chemistry-transport modelling, the use of aerosol measurements and data retrieved from satellite observations, focusing on August 2005, a month with intense hurricane and tropical storm activity over the Atlantic Ocean. The

results indicate rather small amounts of accumulation mode desert dust, sea salt and chemically aged dust aerosols in this Atlantic Ocean region. Aerosols of smaller size (Aitken mode) are more abundant in the area and in some occasions sulphates of anthropogenic origin and desert dust are of the same magnitude in terms of number concentrations. The anthropogenic sulphate aerosol can be transported within a thick layer and enter the cloud deck through multiple ways (from the top, the base of the cloud and entrainment). The sodium (sea salt related) aerosol is mostly found below the cloud base.

Generally, I believe that the objectives pursued are clearly defined and I think the contribution strongly deserves publication in ACP after some minor revisions (detailed below). My overall impression of the manuscript is quite satisfactory, especially as a reference for further developments related to studies considering aerosol influences on cloud processes and storm development. All the sections are well described and defined; however I miss a discussion about the description of how some physico/chemical processes are included in the modeling system presented. Also, further research should be devoted to study aerosol/radiation/cloud processes, which are not featured in this manuscript.

[Author Reply]: We would like to thank the reviewer for the positive remarks on the manuscript. We address all the questions and suggestions in this text and in the revised version of the manuscript. A description of the heterogeneous reactions included in the modelling system has been added in section 2 (Models and data used) of the manuscript.

One of the new topics included in the manuscript is the use of SKIRON/Dust fluxes, which are converted to emissions of crustal material and they imported into the CAMx model for the simulation set up. However, there is no interaction between CAMx and the meteorological driver SKIRON/Dust. As also pointed by Dr. Levin in his review (and also shown by Alpert et al., 1998. Nature 395, pp. 367-370 or Perez et al., 2006. JGR, D162006, doi:10.1029/2005JD006717), the inclusion of mineral dust radiative

effects could lead to a significant improvement in the radiation balance of numerical weather prediction models with subsequent improvements in the weather forecast itself. It should be clearly stated in the manuscript.

[Author Reply]: We agree with the reviewer and we have added a discussion about this issue in section 2 (Models and data used) of the manuscript. We have also added a reference to a recent work (Spyrou et al. 2010) where these issues are discussed extensively (dust-radiation interactions for SKIRON/Dust model).

As pointed by the authors, the resolution used somehow hampers the validation efforts carried out. The 0.24_ resolution of the modelling system may not be fine enough to describe the aerosol concentration in the area analysed, especially when using daily PM10 measurements in the Canary Island (ES0886A), Tenerife (ES1133A) and Madeira (PT0133A and PT0135A). Due to their location in the Atlantic Ocean, these stations are important for identifying the dust transport towards the west. But at this model resolution, the orography of the Canary Island or Madeira is not accurately captured and therefore the concentrations may not be precisely reproduced by the models. Also, futher discussion is required about the criteria for the selection of the stations where the model validation is performed.

[Author Reply]: We agree with the reviewer's comment on the limitations in the comparison with the measurements due to the coarse resolution of the simulation. This is evident from the statistical analysis as the PM10 and PM2.5 are underestimated from the model. One reason for this result is the rather coarse horizontal resolution. For the criteria of the stations selected we have a detailed discussion about the EEA stations (page 5193, lines21-28). We have added more details about the EMEP stations in the same page. For the validation of the number concentration we just took all the freely available stations with data for the studied period of August 2005.

Minor comments

1) Sometimes there is some confusion for the reader when using "concentration" (be-

tween ass concentration and number concentration). This should be addressed in the revised version of the manuscript.

[Author Reply]: We have added “mass” or “number” to the “concentration” to all the relevant places in the manuscript according to the reviewer's suggestion.

2) I miss further discussion on how the model and satellite observations are compared, and if any statistical comparison has been carried out, trying to quantify the errors.

[Author Reply]: The comparison between model and satellite retrievals is done in a qualitative way. In the text we discuss the reasons why we did not perform a statistical comparison (page 5199, 1st paragraph). To avoid the confusion on this matter we have tried to strengthen the text in the beginning of the satellite's discussion.

3) There are sentences, especially in the introduction section, needing improvements in order to be clarified. I recommend a further revision throughout all the manuscript.

[Author Reply]: A revision of the manuscript has been carried out in order to address all the issues raised by the reviewers.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 5185, 2010.