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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The MACC-II 2007–2008 reanalysis: atmospheric dust evaluation and characterization over Northern Africa and Middle East" *by* E. Cuevas et al.

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Please find below a detailed response to the each of the general and specific comments.

General Comments (GC):

GC1: There is a general concern whether dust forecasts are sufficiently accurate to be a part of weather prediction operations. By performing a 2-year reanalysis of the MACC model and by its thorough validation against aerosol observations, this study provides very useful detailed insight on the model performances and its effectiveness

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to predict the atmospheric dust process. The uniqueness of the study is that examines spatial and temporal (seasonal and interannual) variability of dust, specifically over or very close to dust sources. The authors successfully managed to present a complex analysis of large amount of information addressed to the model comparison to different types of dust measurements. The manuscript is well organized, it is also clearly written. Presenting many of the results in Supplement makes easier readers to follow the major text.

RGC1: We thank this referee for the thorough review of this manuscript. We really appreciate the positive comments and suggestions.

GC2: This study covers only two years, although the recent MACC atmospheric composition reanalysis (Inness et al, 2013) covers much longer period. Why 10 year period was not selected? Is this done as an initial work that would be extended later? Or, this is because records of some data (MODIS Dark Target AOD?) were too short? Please, explain in more details and justify.

RGC2: The present study evaluates and analyses the MACC-II short reanalysis dust simulation for the period 2007–2008 over Northern Africa, Middle East and adjacent regions using ground-based and satellite observations. This new (and short) MACC-II reanalysis incorporates an improved dust parameterization scheme which was not implemented in the previous the 10-year MACC reanalysis. This difference is highlighted in the Introduction (P. 27801, L. 9-13) and in more detail in Section 2 ("The MACC-II Aerosol Prediction System: 2007–2008 aerosol reanalysis") from P. 27804, L. 20 to P. 27805, L.11. However, in order to make clear that this is an "dust-improved" short reanalysis, this fact will be highlighted in the introduction of the final manuscript.

Specific Comments (SC):

SC1: Pg. 27800, line 4. Giannadaki et al 2014 (dust impact on mortality) to be included

RSC1: We agree to include the following reference: Giannadaki, D., Pozzer, A., and

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Lelieveld, J.: Modeled global effects of airborne desert dust on air quality and premature mortality, Atmos. Chem. Phys., 14, 957-968, doi:10.5194/acp-14-957-2014, 2014.

SC2: Pg. 27800, line 11. Include Nickovic et al, 2013 (iron/dust deposition over the ocean) to be included

RSC2: We agree to include the following reference: Nickovic, S., Vukovic, A., and Vujadinovic, M.: Atmospheric processing of iron carried by mineral dust, Atmos. Chem. Phys., 13, 9169-9181, doi:10.5194/acp-13-9169-2013, 2013.

SC3: Pg. 27800, line 18. Not only past but current and future climate are/would be affected by direct effects. Also, weather is influenced by dust-radiation feedback (e.g. Peres et al,2006 to be included, as early evidence)

RSC3: Pérez et al. (2006) paper deals the implementation of mineral dust radiative effects into a regional atmospheric dust model (DREAM). So, it doesn't really fits the idea of the paragraph which is devoted to general aspects of mineral dust impacts. Pérez, C., S. Nickovic, G. Pejanovic, J. M. Baldasano, and E. Ö zsoy (2006), Interactive dust-radiation modeling: A step to improve weather forecasts, J. Geophys. Res., 111, D16206, doi:10.1029/2005JD006717.

SC4: Pg. 27802, line 13. The dust reanalyses made by Barcelona Supercomputer Centre to be mentioned (Perez et al??)

RSC4: This paragraph is focused on previous MACC reanalysis, so it does not seem appropriate to include this reference here. Dust models run by BSC have been previously referenced in P. 27801 (L.15-17).

SC5: Pg. 27804, line 26 Written in the text: ": ::A revision of the dust emission potential: : :" Please explain what kind of revision is made

RSC5: The main revision of MACC-II short reanalysis (2007-2008) consists on performing a dust emission estimate for 2007 and 2008 in four sub-regions (marked in

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Figure 1) in the Sahara-Sahel region, instead the single large Sahara-Sahel region considered in the previous MACC reanalysis version.

SC6: Pg. 27805, line Written in the text: ": : :18 at 06, 09, 12, 15 and 18UTC in the period 2007–2008: : :" Why 0/24UTC is missing?

RSC6: Yes. Indeed a clarification is needed here. The 00UTC MACC output is not considered because for model evaluation we have to limit to sunlight period when both AERONET and satellites can provide AOD and AE. A sentence explaining this fact will be introduced in the final version of the manuscript.

SC7: Pg. 27814 line4. Written in the text: ": : :An important objective of the MACC-II reanalysis evaluation is to examine its ability to reproduce aerosol spatiotemporal variability: : :" The objectives of the study should be also mentioned in the Introduction as well.

RSC7: We fully agree with the Referee. The objectives of this study will be included in the Introduction section in the final manuscript.

SC8: Supplement S4, S5: Does it make sense to show a scatter diagrams, in addition to the existing images?

RSC8: Including the scatter diagrams would be redundant since the agreement / disagreement between observations and MACC-II are perfectly seen in S4 and S5. In addition, these graphs show day-to-day and seasonal AOD/PM10 variations

SC9: Pg. 27831 line 22. Written in the text: ": : :we only selected those extinction profiles corresponding to AE < 0.35 provided by the Dakar AERONET sunphotometer (located some 80 km from M'Bour): : :" It would be useful to explain somewhere in the beginning of the manuscript why AE thresholds in the study vary from 0.35 to 0.75

RSC9: AE thresholds are different at M'Bour and SCO for the following reasons:

1) M'bour experiences a high number of days with presence of mineral dust, but very

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often it is mixed with biomass burning aerosols on some level, so we must impose restrictive conditions (AE<0.35) that filter as much as possible the presence of biomass-burning and retain well almost-pure mineral dust conditions.

2) On the contrary, IZO is completely free from biomass burning aerosols and the number and intensity of mineral dust intrusions is much lower than at M'Bour. So, we use a less restrictive AE threshold (AE<0.75) which assures a minimum number of mineral dust days to perform the model evaluation.

We fully agree with the Referee that this fact must be clarified in the text.

A sentence summarizing the above given reasons will be included in Section 4.2., just after the following paragraph: "The profiles on those days with predominant dust aerosols over each station (AE < 0.35 at M'Bour and AE < 0.75at SCO) are averaged for different seasons, and compared with the corresponding averaged extinction profiles from ground-based lidar and MACC-II at each site".

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/14/C10579/2014/acpd-14-C10579-2014-supplement.pdf

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 27797, 2014.