# Interactive comment on "Observations and regional modeling of aerosol speciation and size distribution over Africa and Europe" *by* Laurent Menut et al.

Anonymous Referee #1

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This manuscript, entitled "Observations and regional modeling of aerosol speciation and size distribution over Africa and Europe" presents an original analysis of aerosol properties, both modeled by a regional chemistry-transport model and given by surface and column-integrated observations, in terms of speciation and size distribution. Detailed results are about different aerosol species, from both natural and anthropogenic origins, are discussed, showing the performance of this regional model to reproduce the variety of aerosols in this region. The methodology and the results are for most of them (except in Section 7) clearly presented and explained, with discussion on the strengths and weaknesses of the current version of the model. However, several corrections are needed before considering a publication in ACP.

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——————————————— Main comments :

1. Even if the comparisons between the model and observations show for different variables satisfactory scores for CHIMERE, the authors tend to overestimate its performance throughout the manuscript, notably in the following cases: - the correlations for AOD (Table 5, Section 4.1) are very weak for many stations (9 out of 28 have a correlation lower than 0.1). - the optical properties maps (Figure 3) show difficulties to reproduce correctly the Angstrom exponent in Western Europe - in the PM2.5 time series, some observed peaks are missed by the model, which is not explained (for example around 10 July in Schauinsland) I do not really challenge the performance of the model as I am aware of the complexity to compare observations and models, but rather the way it is presented by the authors (for example lines16-18 page 11). The latter should moderate their conclusions, and give possible reasons to explain the difficulties mentioned above.

2. The title does not exactly correspond to the manuscript in terms of variables (Section 4 deals with aerosol optical properties which are not mentioned in the title) and of the domain of study ("Africa and Europe" is not precise enough). I suggest to add in the title optical properties, and to replace "Africa and Europe" by "Western Europe and Northern Africa".

3. The spatial distribution of the stations used here could be a major limitation of the study. Indeed, the EMEP stations cover only northern Europe and Spain (Africa and southern Europe are missing), while the AERONET stations do not include northern Europe. To my knowledge, there are other stations which could be taken into account, which would improve the robustness of the different scores.

4. The analyses on the speciation data are very interesting (notably Figures 5 and 6), but the same kind of diagnostics is missing as far as optical properties are concerned. I suggest to the authors to add this information on speciation for AOD, in order to better understand the differences shown in Figure 3.

- Abstract lines 6-7: I don't understand why only mineral dust is mentioned in this sentence, while spatial correlation and daily variability concern all aerosols.

- Abstract line 12: "dust relative contribution": Please precise the variable (mass ?).

- Page 3 line 18: what does the ADRIMED project bring to the present study ? Are there any specific observations that could be used here ?

- Page 5 lines17-18: The authors should justify properly that "the domain was selected to be large enough to account for anthropogenic emissions". The latitude of the northern limit of the domain seems to be too low, as northern British Isles, a part of northern Sea, and Scandinavia are not included in the domain.

- Page 7 line 1: "the results are presented from 10th June to 31st July 2013". Is it 30th (as in the following sections) or 31st July ?

- Page7 line 3: The WRF model has been used with a 60 km horizontal resolution, while it is mentioned it is a non-hydrostatic model. Has the simulation really been carried out with non-hydrostatic physics ? It is surprising for such a resolution.

- Page 7 line 5: To explain this method of "spectral nudging", it would be useful to provide the approximate limit (in km) of the horizontal scales which are nudged towards NCEP analyses.

- Page 8 section 3.2.2: there is a confusion between POM (Primary or Particulate Organic Matter) and PPM. Please clarify the difference and correct the acronyms (between the text and Table 3).

- Page 9 lines 17-18: "all kind of anthropogenic and natural sources are taken into account on an hourly basis". Please clarify the time frequency of the meteorological forcing given by WRF, and the emissions in CHIMERE. Does this hourly frequency also concern natural emissions (sea-salt, dust) ?

- Page 10 Table 4: Please confirm that there is no coarse mode for sulfate. The text is misleading (line 8).

- Page 12 Section 4.1 and 4.2: how have the optical properties for the different aerosol bins been fixed ? This may explain partly the difficulties of CHIMERE to reproduce AOD and Angstrom exponent.

- Page 13 line 12: which aerosols could explain this "thin plume modeled over the Atlantic" ?

- Page 13 line 20: "high AE values" in western Europe. Contrary to what the authors affirm, this result is not found in the model (about 0.5 against values higher than 1 in the observations).

- Page 14 Section 5.1: In the PM10 series, are dust and sea-salt particles included ? Both for models and observations ? It is unclear for me, as these aerosols are not detailed in Section 6.

- Page 14 Section 5.2: The authors do not use the information on altitudes given in Table 1. As the comparison is done with first vertical level in the model, this could explain the difficulties of CHIMERE in the Alps for example.

- Page 17 line 13: K-puszta is not "the only station with a very poor correlation".

- Page 20 lines 3-5 (Section 7.1): I don't understand if this calculation is done on the model bins (as mentioned line 3) or on the AERONET bins (line 4), and in the second case, why the finest and coarsest sizes should be added, while they are not represented in Figure 9. Please clarify this method.

- Section 7.2: This diagnostic is original, and the authors draw several conclusions from Figure 11. However, it is not easy to understand. In particular, I suggest the authors to clarify the methodology presented in the beginning of this section and the caption in Figure 11.

- Page 23 line 14: please clarify "to integrate the dv/dlog(r)".

- Page 24 Figure 12: Please detail the caption.

———————————————— Technical corrections:

- The line numbering should be corrected (several identical numbers in the same page).

- Please pay attention to the choice of American/English spelling, and keep it in the whole manuscript (for example modeled or modelled).

- Please remove all the articles "the" before dates.

- Abstract line 4: PM is not defined.

- Page 3 line 5: description (without s)

- Page 3 line 6: aerosol (without s)

- Page 3 line 10: please rephrase "the aerosol's composition behavior understanding"

- Page 3 line 27: Aerosol (without s)

- Page 5 Table 1 (caption): aerosol (without s)

- Page 7 line 18: concentration (without s)

- Page 9: 3.3.2 Emission distributions in aerosol bins

- Page 13 line 12: the Atlantic Ocean (and not sea)

- Page 23 line 20: underestimates

- Page 23 line 23: the

- Page 25 line 12: aerosol (without s)