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# *Interactive comment on* "An assessment of aerosol optical properties from remote sensing observations and regional chemistry-climate coupled models over Europe" *by* Laura Palacios-Peña et al.

**Anonymous Referee #1**

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Interactive comments on the manuscript "An assessment of aerosol optical properties from remote sensing observations and regional chemistry-climate coupled models over Europe" by Palacios-Peña et al.

General Comments

The manuscript compares aerosol optical depth and Ångström exponent retrieved from satellite platforms and simulated by distinct online coupled chemistry-meteorology models with AERONET databases during biomass burning and Saharan dust episodes



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Discussion paper

in Europe during 2010. It also analyzes if the inclusion of the aerosol-radiation and aerosol-cloud interactions improve model skills in simulating the aforementioned aerosol optical properties. The subject is of scientific relevance and within the scope of ACP. However, there are some major deficiencies, particularly concerning methodological issues and scientific arguments which must be explored in order to consider this study suitable for publication.

Specific comments

1) The authors claim to evaluate model skills comparing simulations neglecting the aerosol radiative effect with simulations performed with the aerosol direct effect and the aerosol-cloud-interactions. But in order to do so, they compare AOD and Ångström exponent fields either from AERONET or satellite retrievals. The first question is why considering or neglecting the aerosol direct effect or the aerosol-cloud interactions could improve modeling skills to reproduce AOD which depends primarily on the aerosol concentration in the atmospheric column? I would expect AOD to depend strongly on source strength (fire characteristics such as combustion phase, intensity, burnt area, injection height during the biomass burning episode and wind speed and humidity during the dust episode, for example). A better reasoning, discussing the physical mechanisms to justify how AOD field would be modified by the aerosol direct effect and the aerosol-cloud-interactions is necessary. The authors mention feedback mechanisms but a more detailed discussion on these processes should be presented. The way it was introduced is too vague (page 3, lines 10-16).

2) At page 6, line 5, it is mentioned that the heat released by the fires was not taken into account by the models. This can explain why AOD was underestimated by the models. According to Freitas et al. (2006), the heat released by the fires is responsible for the strong updrafts, transporting the emitted tracers aloft which can reach rapidly the free troposphere and even the stratosphere where they can be transported horizontally for long distances.

3) Was level 2.0 AERONET data used in the comparisons?

4) When comparing satellite retrievals with AERONET results, page 7, line 28, the authors mentioned that daily data were used, but none of the satellites are geostationary and aerosol optical properties can change throughout the day making no sense to compare instant values from satellite overpass time with daily mean data from AERONET. One possible methodology to follow was proposed by Ichoku et al. (2002), based on spatial mean (for satellite data) versus time mean (for AERONET data) around AERONET geographical location and satellite overpass time respectively. Since in the present study the authors have the advantage of the model results, wind speed and direction from the models can help to define the best area coverage and time interval in estimating the mean values to be compared.

5) Also concerning satellite versus AERONET results, apparently the spectral dependence was not taken into account appropriately. From Figure A1, OMI and SeaWIFS AOD at distinct channels are compared with AOD from AERONET, but AOD from AERONET was kept fixed for varying OMI/SeaWIFS wavelengths.

6) When comparing model results with either MODIS or AERONET data, it is not clear how clouds were excluded from model results, since the retrieved data either from MODIS or AERONET are available under cloud free conditions only.

7) At the Results section (page 10, lines 20-21), the physical meaning why ARI contributed to improving AOD estimation especially over the areas with high AOD values should be explored. Although the authors mentioned this improvement at regions of high AOD, I particularly cannot observe it, looking at the maps from Figure 2. High AOD areas correspond to locations with yellow color in Figure 2.a and improved results are colored in yellow to red in Figure 2.c and such areas are located in a more systematic way further to the west. In the areas of high AOD, blue and yellow/reddish colors are randomly distributed in Figure 2.c.

8) From Figure 2.a, AOD values higher than 1.5 were observed during the Russian fire

episode, but in Figures 3.b and 3.c, when comparing time series of modeling results with AERONET AOD values, the authors chose AERONET sites with AOD lower than 1.0, which we conclude are outside the smoky region. And from those graphs, when high AOD values were simulated, AERONET data did not show such enhancement, also contradicting the statement that including ARI and ARI+ACI improved modeling results.

9) In the comparison between model simulated and AERONET data for the Saharan dust episode, the time series at Figures 7.b and 7.c (page 31) and 9.b and 9.c (page 33) for the chosen AERONET sites present many days without AERONET retrievals. At Toulon, data for only 3 days were available and at Helgoland only 2. How could the results be statistically significant with so few data available to compare?

10) Page 36: The title of the table must express clearly what it shows. For example, why MBE has minimum and maximum values if, by definition, it represents a mean value?

Technical corrections

Page 2, line 27: there is a typo after "Earth";

Line 30: remove "with" between "instruments" and "onboard";

Page 3, line 8: remove "of" between "instruments" and "onboard";

Line 26: remove ";" after "demonstrate;

Line 32: replace "This" by "The";

Line 33: Please insert the correct unit for temperature in "between 0.2 to 2.6°". Note that if the unit is Kelvin (K), there is no degree symbol (°);

Page 4, lines 5-6: replace "led a" by "led to a";

Line 7: The authors mentioned a drop in the mean temperature, but it is not clear if it refers to surface temperature or air temperature. If air temperature, please specify at each level or altitude. Moreover, as commented previously, the correct unit for temperature in Kelvin is K, without the degree symbol;

Line 20: Add a white space after "(2)";

Line 26: The correct spelling is "Ångström". Please, check throughout the entire manuscript.

Page 5, line 18-19: remove comma symbol (,) after 60° and 55°;

Line 32: remove tilde symbol (∼) above 7;

Page 7, line 4: MODIS stands for Moderate Resolution Imaging Spectroradiometer;

Page 9, line 24: Figures 2 show (remove "s" from "shows");

Page 9, line 27 and page 11, line 17: remove "s" from "surroundings": surrounding areas;

Page 11, line 4: Replace "As indicated Palacios-Peña" by "As indicated by Palacios-Peña";

Line 8: Do you mean "a rough overestimation of about 50% of the emission from the total biomass burnt used here"?

Line 20: Add a white space between ")" and "indicated";

Page 12, line 3: Add a white space between "," and "the";

Line 16: replace "are" by "area";

Page 27, Figure 3: It would be helpful to identify where the sites used to generate figures (b) and (c) are located in the map of (a). This can be done using a distinct symbol for them in the maps of Figure 3(a).

References

Freitas et al. 2006, GRL 33, L17808, doi:10.1029/2006GL026608. Ichoku et al. 2002, GRL 29, doi:10.1029/2001GL013206.

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