

Interactive comment on “Aerosol optical properties over Europe: an evaluation of the AQMEII Phase 3 simulations against satellite observations” by Laura Palacios-Peña et al.

Anonymous Referee #3

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General comments

This is an interesting work that attempts to evaluate modeled AOD and AE over Europe using six model simulations performed under the AQMEII3 framework. The AQMEII initiative has provided a great opportunity for air quality model evaluation and model intercomparison across two continents that allows the community to assess the accuracy of the modeling systems, the drivers of their differences and make suggestions for future model improvements.

The main objection regarding the scientific methodology is the use of only one MODIS product to conduct model evaluation and intercomparison. The authors should take

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advantage of all possible AOD/AE observations in the model domain over Europe, to enhance their understanding of model behavior. The paper needs grammatical editing to improve the language and some restructuring to improve the flow. There are a lot of clarifications needed for the methodology and discussion of the results. I am in favor of publishing this paper with Atmospheric Chemistry and Physics with Major Revisions. The specific comments that follow will help improve the discussion of the methodology and significance of the findings so that the overall quality of the manuscript is enhanced.

Specific comments

Abstract:

1. Line 13: “this variable” refers to AOD? Please be specific
2. General comment: It would be more beneficial to have a quantitative description of the conclusions. If AE is predicted more erroneously than AOD, add some quantitative measures to that statement in terms of errors/biases etc. Descriptive characterizations like “more serious errors” do not add any substantial information.

Introduction:

1. Line 1 and elsewhere: please refrain from using adjectives as “gravest”. Keeping the discussion on scientific terms is more appropriate.
2. Lines 4-5 and elsewhere: please remove the phrase “so-called”. The aerosol-cloud-radiation interactions are widely known; the definition of the acronym is enough.
3. General comment: The introduction includes a lot of information on past and recent work similar to this study. What is not clear is how this study is different from others. What is the new contribution made by this study to the scientific knowledge of modeled AOD and AE? A high-level brief statement would be appropriate in the introduction.

Methodology:

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1. Page 6, IT1 simulation: “. . .Meteorological inputs were generated using WRF-Chem version 3.4.1. Anthropogenic emission were MACC and biogenic were computed by MEGAN.WRF-Chem was adopted to predict GOCART (Goddard Chemistry Aerosol Radiation and Transport) dust emissions (Ginoux et al., 2001) along with meteorology.” What is the role of WRF-Chem in the WRF-CAMx simulations? It is not clear how those two modeling frameworks are combined. It doesn't make sense to use WRF-Chem for meteorology while WRF is already the modeling system used combined with CAMx.
2. Table 1 should include one more column that describes the origin of AOD/AE calculations: prognostic (i.e. during runtime) or diagnostic (i.e post-processing).
3. Description of dust sources for each simulation is very important since the domain covers North Africa and the signal from the satellite is much stronger there. Prescribed dust emissions (as total PM), online dust sources, etc. This information can be added to Table 1 under Aerosol Mechanism.
4. It's a surprise that the authors did not make use of AERONET data for such comprehensive AOD and AE model evaluation. This is strongly recommended for the revised version.
5. In addition, the merged AOD product has been used in a number of recent publications with one of the most important being Sayer et al. (2014) (<https://agupubs.onlinelibrary.wiley.com/doi/pdf/10.1002/2014JD022453>) which is a point of reference for the usage of these products.
6. Observational data: The authors state that “The selection of this observational data was due to results found by Palacios-Peña et al. (2017a)”. More detailed explanation is needed for the selection of observational data than the reference to a previous publication. The readers should not have to read other papers to understand the basics of the approach.
7. Observational data (page 8, line 26): The description of the threshold (mask) is not

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clear. Is it applied to AOD and AE? Why specifically 10% of the maximum? Does that mean that the threshold varies with location, depending on the maximum observed satellite retrieval? Why?

8. Figure S1 shows no satellite observations over land for JFM and limited data for OND, assuming that grey colored areas have zero observations. The picture is worse for AE where no observations are seen over land. With these data points available for evaluation, how can the authors estimate seasonal statistical metrics? This is a clear case where AERONET data and/or additional satellite AOD/AE products should be used to cover for these gaps (MODIS, MISR, or whatever is available for that specific period),

Results

1. Page 9, line 24: The phrase “The seasonal break down is presented besides named.” is confusing. Please rephrase.

2. The results section needs some restructuring to allow a nice reading flow. Paragraphs should contain more than one sentence and it will be beneficial to keep the discussion on one topic/figure/statistic in one paragraph. For example, devote one paragraph describing spatial patterns of MODIS AOD, one paragraph for each season and so on.

3. The entire AOD evaluation section does not include an interpretation of the under- or over- estimation seen for each model simulation. Also, why is there a difference between model simulations? This is much more important and interesting than just presenting the statistical findings.

4. Section 3.2, page 18, line 6: “In this section, the simulations run with the available data were less than they were for AOD.” What is the meaning of this sentence? Wasn't there one annual simulation performed by each modeling system? This is confusing; please explain.

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5. Section 3.2 (Variability): page 22, line 4: why is ES1 showing a remarkable AOD representation? The PDF for ES1 shows that the simulation underrepresented low AOD and overestimated high AOD for the majority of the seasons (except JAS maybe).

6. Figures S5 to S9 in the supplement are never mentioned in the text. In addition, Figure S9 (annual PM_{2.5} emissions in Europe and North America) does not seem to align with the contents of this manuscript. Please specify the role of all additional figures in the supplementary material.

Summary and Conclusions

1. Page 22, line 29: What is the meaning of front observations in the following phrase “an evaluation of the simulations of the front observations was needed”? Please revise accordingly.

2. Page 23, line 7: Please rephrase the following sentence by replacing “misunderstanding” with a more appropriate characterization (“due to a misunderstanding of the simulation of the aerosol vertical. . .”).

3. A lot of the text in section 4 could be included in the respective discussion of the results (see comment #3 in the Results section above). I suggest that the authors discuss here the main key results drawn by their analysis in a clear and concise way.

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