Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2020-467-RC2, 2020

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A complex aerosol transport event over Europe during the 2017 Storm Ophelia in CAMS forecast systems: analysis and evaluation" *by* Dimitris Akritidis et al.

Anonymous Referee #3

Received and published: 13 July 2020

The authors analyse a remarkable event of combined desert dust and biomass burning aerosol transport over Europe using observations and model results, and in the process evaluate some aspects of the CAMS air quality forecast system. Studying such events is important because of their quite drastic impact on the European air quality, potential implications on clouds and weather, and impairment of solar energy yield. Their distinct air quality signatures are good benchmarks for forecast systems. The subject is relevant and within the scope of ACP (there is some overlap with the GMD scope regarding the model evaluation aspect), the article is well organised and written so that I recommend publication after addressing the following comments:

Page 1, line 11: Throughout the manuscript you use the correlation (coefficient) r, here you present the shared variance (and I assume you are referring to r^2). Because the term is less common or even used with different meaning, and to be consistent with the main text, I suggest to stick to r.

Page 1, line 13: Please expand IFS here.

Section 2.1: Some more information about the aerosol representation in the model would be helpful: does the model assume the different aerosol types to be externally mixed, both regarding the aerosol optical properties and regarding any chemical or physical interactions between different components? This would be interesting to know especially since the event under consideration involves dust, biomass burning aerosol and sea salt.

Page 4, line 8 and 9: The units should be micro metres not metres and it has to be mentioned that the numbers represent radii not diameters. What bins are used for the other aerosol types, e.g., what is BC1 vs. BC2?

Page 4, Eqs. (1) and (2): I understand these equations follow recommendations elsewhere, but could you indicate in the text where the factors come from? Also, SS3 is not considered for PM2.5 (and PM10), because the radius is > 5 um, but according to the intervals provided above, not considering DD3 for PM2.5 seems to ignore dust particles between 0.9 and 1.25 um radius.

Page 5, line 10: Please expand LT once

Page 5, lines 28ff: Which selection criteria for the stations where used exactly?

Page 7, line 26: The FGE deserves to be introduced by an equation, moreover uppercase is more common.

Page 7, lines 30 to 34: How can this conclusion be aligned with Fig. 4, where in many regions with low AOD the AOD is enhance by data assimilation?

Figs. 9 and 10: I find the similarity of the PM10 and PM2.5 composition surprising, normally I would expect a higher dust and sea salt fraction in PM10 than in PM2.5. Does this indicate some limitation of the model? After all, from Eqs. (1) and (2) it is clear that the contributions in Fig. 9 and 10 cannot be that different, and the largest particles (SS3 and DD3) are not relevant at all, but is that realistic? Do the stations provide the PM2.5 and/or PM10 composition for comparison, or is there any other suitable data source to validate this aspect?

Generally, I believe links in footnotes and within the text are supposed to be be moved to the References section to comply with the journal standards.

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