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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Aerosol water parameterization: long-term evaluation and importance" *by* Swen Metzger et al.

Anonymous Referee #1

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The stated goal of this manuscript is to evaluate the importance of aerosol water for AOD calculations in a long-term (∼one decade) climate simulation using the EQuilibrium Simplified Aerosol Model V4 for climate (EQSAM4clim). The authors argue this modeling parameterizaition is computationally efficient and does not degrade model performance when evaluated with AOD (e.g., AERONET and EMEP) measurements in climate modeling applications relative to a more explicit approach, i.e., ISORROPIA. The implemented EQSAM4clim in the climate model, EMAC, compares reasonably with other AOD comparison results in the literature.

I find the results are supportive of the abstract's stated main conclusion, that aerosol water is important for climate applications. The paper needs work prior to final publication and some results contradict the literature. In Figure 8 the authors find that aerosol

water mass concentrations are highest in the western desert of the U.S. Liao and Seinfeld 2005 (https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/2005JD005907) and Carlton and Turpin 2013 (https://www.atmos-chem-phys.net/13/10203/2013/) find it is highest in the eastern U.S. where sulfate mass concentrations and RH are higher. Can the authors provide a context for this discrepancy? The authors stress the importance of 'aging' in their manuscript and this is not well described. I think they mean changing particle hygroscopicity with time but this is not clearly stated. Also, 'aging' is a not a precise term. Do they mean increased oxidation due to longer OH exposure in the atmosphere, and the subsequent changing chemical composition important water uptake? The authors do not make a compelling case in the introduction for their work and I found this confusing. Below I provide comments that think help address my concerns and I hope the authors find them useful.

The introduction is not directly linked to the premise that aerosol water is crucial. I find it difficult to understand why the introduction starts with the importance of desertification and subsequent dust emissions to properly describe AOD when their title and abstract focus on aerosol water. Do the authors mean to say that even in arid regions, AOD is not properly described in models unless water uptake is considered? That is a compelling argument and would help to connect the introductory desert discussion with aerosol water.

Sentence 1: "providing realistic projections of climate change is one of the most difficult tasks of climate modelers..." I would state "Providing realistic projections of climate change is difficult due to many unknowns and large uncertainties . . . " As written the sentence is awkward, the authors seem to say, conducting climate simulations is the hardest thing for climate modelers to do.

The authors might not be aware of this paper using actual measurements of particle-phase ions and meteorology coupled with ISORROPIA-II to calculate aerosol water to better connect surface particle mass measurements to satellite AOD by Nguyen in Geophys. Res. Letts.:

https://agupubs.onlinelibrary.wiley.com/doi/10.1002/2016GL070994 I think it would help with their argument to link the importance of aerosol water with AOD and then subsequently radiaitvie forcing calculations important for climate modeling efforts.

Page 4, Line 13: is dry deposition based only properties of the surface? Do different chemical species all deposit at the same rate?

Page 4, Line 9/25: Why are some subroutines listed together, while others are separated out? For example CLOUD, CVTRANS, JVAL, TROPOP, H2O, ORBIT, and RAD are listed below in a similar fashion.

Page 4, Line 16/17: what do "...water isoprene concentration" and "methanol water deposition. . ."? Perhaps the authors mean isoprene concentration in ocean water? Do they mean dry deposition of methanol to water? I read this sentence multiple times and I am still not sure.

Page 4, Line 20: Does "Our chemical mechanism for the troposphere is similar to the one used in poz" mean the mechanism is the same as used in Pozzer et al., 2006, cited earlier? Sometimes the authors write "poz" and "Pozzer". I am not sure if they mean the same thing.

Page 5, last sentence and continuing to the next page: "It was shown by Metzger et al. (2016b) that the i -approach allows to analytically solve the gas-liquid-solid partitioning and the mixed solution water uptake by eliminating the need for numerical solutions . . ." Is the Metzger approach not a numerical solution?

Page 16, line 6: take out 'only'

The text regarding "Kindly" provided emissions seems like language that should be in the acknowledgements.

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