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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Stratospheric aerosol radiative forcing simulated by the chemistry climate model EMAC using aerosol CCI satellite data" *by* Christoph Brühl et al.

Anonymous Referee #1

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The manuscript presents some simulation results for aerosol optical properties obtained with different configurations of the EMAC model and compares them to a variety of satellite products. Clearly, the topic of atmospheric aerosols is appropriate for ACP. My recommendation is, however, to not accept the manuscript for publication in ACP, mainly because its scientific aim is unclear. The introduction of the paper doesn't state an aim, and doesn't formulate a scientific question. It provides some background information in particular on stratospheric aerosols, satellite data, and on earlier studies with EMAC. Furthermore, it makes some general statements, e.g. concerning the usefulness of specific satellite data "to validate and optimize assumptions ... in the model", of which it is unclear if they are a conclusion of some earlier work or just the opinion

of the authors. No knowledge gaps are mentioned, no strategy how to tackle gaps, and how this work makes progress in comparison to the rich body of literature on the topic. Even the reference to an earlier paper with involvement of several of the authors remains vague and it is unclear what the present study may add ("Some aspects ... of this study have been addressed in Bingen et al. (2017)"). In section 5 (and also in the abstract) some "conclusions" are actually drawn, but in several cases they do not seem to be backed up thoroughly by the main body of the manuscript. In addition, there are very few statements that could be understood as knowledge gained on the atmosphere. And if they can (like "The total AOD in the visible ... is very sensitive to aerosol water and the composition of sea salt."), they tend towards being very general and again it is unclear how potential discoveries in this study relate to earlier works. Most of the concluding statements relate to model tuning ("simply assuming a factor of 2 for conversion ... is too crude"), it is unclear how general or model specific they are, and like in this case they are not well developed in the rest of the paper. Because I understand the manuscript as mostly related to model evaluation and tuning I would suggest the authors consider resubmitting it to a more model development related journal like GMD or JAMES, but even in this case I think large parts of the manuscript would need to be rewritten. In the following I will provide a list of further issues I see with the manuscript:

- Abstract: The abstract contains some statements that can be considered as conclusions, e.g. "sulfate particles from ... volcanic eruptions dominate the interannual variability of aerosol extinction ...". But if this is considered important enough to make it to the abstract: why doesn't it appear in the "conclusions"? And is it a new discovery?

- P3L3: "The development work of these CDRs showed ..." The formulation is odd. How did the work show this importance? And why does it come to this non-linearity in the averaging process? Wouldn't this depend on the way averages are built? Any reference for it?

- P3L25: "size and optical thickness on a 10 km grid". Shouldn't size be provided with some vertical resolution?

- Section 3: Model Setup. It is not sufficiently clear from this section how observations are used in the simulations. Vague formulations are used in many places, like “use of data . . . for input and validation”, “aerosol module parameters . . . were optimized on the basis of satellite data”. Which aerosol parameters are actually prognostic quantities, which data are prescribed how (e.g. as boundary or initial conditions?) in the simulation process, which are used how to tune which model parameters. It is important to be very specific here, also to understand how dependent or independent the simulation results are from the data used for evaluation. (see also below)

- P4L6: “we used different model resolution to improve the dust simulations”. Sounds odd. I guess one can assume that higher resolution might improve simulations, but this is not what is said, here.

- P4L16: “. . . particle radius of 1.6 um to avoid too fast sedimentation . . .” Again an odd formulation. Would any other parameter lead to too fast sedimentation? And would this parameter be chosen differently based on observed particle size distributions?

- P4L25-35: “. . . superimposed to the simulated SO2 . . .”; “boundary conditions are taken from . . .”; “Marine DMS . . . is also included . . .” Again, these formulations are not specific enough. What does that mean? Are simulated fields simply updated at eruption times? For which boundaries are the observations used? How is marine DMS used? In terms of emissions? Is this important when SO2 concentrations are anyhow newly “superimposed” after every eruption? And what does all this use of observations mean for the evaluation of the simulations?

- P5L5 “due to transport” Which transport is meant, here? From the troposphere to the stratosphere?

- Fig. 1 shows comparisons of extinction profiles for different wavelengths. However, this is not discussed in the text. What do we learn from the different comparisons?

- P5L16 “account for dust in a proper way, also with respect to particle size”. What would be a proper way to account for the particle size of dust? And if you say also, what else?

- P5L17 ff: The way the “downscaling” is described here is very misleading. Only much later in the manuscript one learns that the authors have just multiplied the extinction with some factor to obtain a better comparison to observations. It is also not clear where this sensitivity to model resolution comes from. Why does the convection lead to very different transport for a relatively modest change of horizontal resolution? What are the tuning parameters? And how does the tuning of the convection parameterization for fitting sulfur transport influence other important quantities like the radiative balance?

- P5L22, reference to Bingen et al. (2017). It is nice to know that other things are shown elsewhere, but it would be more important to get to know what has been learned elsewhere and what additional knowledge is provided by the figures in this paper.

- P7L3, “our findings that desert dust is also important for the UTLS”. A very vague finding. One could try to quantify it. A first step could be to look at difference plots of Fig. S1 (center and bottom panels).

- P7L10, “global radiative forcing” I guess this is probably instantaneous forcing from a double radiation call? It would be important to spell this out.

- P7L13, “Green lines and symbols show . . . observations like . . .” There are no symbols in the figure. And what means “observations like”? Be specific. This is supposed to be a scientific paper.

- P7L18, “This is clearly seen in Fig. 4 . . .” How do I see in Fig. 4 which part of the AOD is transport-related? And how do I see the monsoon effect if only 20S-20N and 45N-70N are shown, not the monsoon region?

- Fig. 3 and 4: Legends would be nice.

- Fig. 4: How is “stratospheric AOD” defined?

- Caption of Fig. 4: All other caption don’t include interpretation. And what means “differ mostly”?

- P9L6: “a clear signal from biomass burning organic aerosol. It’s not clear to me from just looking at the figure. Please explain why this is clear.

- P11L10ff: There is no motivation provided for the change to resolution T106. How would the problem of tuning convection mentioned for T42 vs. T63 affect T106?

- P13L8, “additional (ongoing) simulations”. What do I make of the “ongoing”? Either the simulations are ready to be used for scientific interpretation or not.

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