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Geoscientific Model Development Discussions

Interactive comment on "A new approach to simulate aerosol effects on cirrus clouds in EMAC v2.54" *by* Mattia Righi et al.

Anonymous Referee #2

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Review of "A new approach to simulate aerosol effects on cirrus clouds in EMAC v2.54" by Mattia Righi et al.

This manuscript presents a new version of a Chemistry Climate Model, focused on developments of the ice cloud scheme, and integrates previous work. It is a not to insightful model description paper, that doesn't really describe any thing in detail. The analysis methods are not state of the art, and somewhat inconsistent. The paper needs major revisions if it is to be suitable for GMD.

General concerns:

1. The paper is all about clouds and aerosols, but I cannot find how activated aerosols are connected to cloud and crystal number. Please state that explicitly.

2. The information in Appendix A is only referred to in passing, and should be a larger part of the manuscript

3. The sensitivity tests and tuning exercises are laudable, but the description of optimizing the model and what parameters are chosen is ad-hoc and subjective. It doesn't seem like any formulation matched the data, but no rigorous method was used to get the final tuned model.

4. Some of the plots show significant artifacts (oscillations) in averaged model fields. I am hoping that this is just a product of the 11 hour time sampling, but it means there is either something fundamentally wrong with either the model itself or the analysis methods.

5. Since this is about ice clouds and aerosols, please determine the ice cloud ERF from anthropogenic aerosols. Techniques exist for this.

6. Substantive comments below on particular aspects of the data analysis also need to be considered in a revision.

Specific Comments:

Page 3, L16: this does not go back far enough. Liu et al (2005) developed a parameterization of homogeneous and heterogeneous freezing that was implemented in CAM by Gettelman et al (2010).

Liu, X., and J. E. Penner. "Ice Nucleation Parameterization for Global Models." Meteor. Z. 14, no. 499–514 (2005).

Page 3, L34: The abstract mentions the mixed phase, but there is no discussion here. This seems to be all cirrus clouds.

Page 6, L33: please state how the aerosols and clouds are connected. You detail how INP are derived. But once you have INP how are they linked with the cloud scheme? Do you just set the cloud drop and crystal number to be at least as large as the activated

number?

Page 7, L14: since the INP parameters are the new part of this study, I think a bit more detail is warranted here, maybe a paragraph, and not left for the appendix.

Page 8, L4: so what are you really focusing on? Not clouds? Not aerosols? What do you mean by the 'interaction'. At this point as a reader I still have not idea. Please be more specific.

Page 8, L23: please at least mention what the data sources are (CERES, MODIS, etc)

Page 8, L24: so how many simulations were conducted? 20? How long are they? Why not use a more sophisticated method for sampling that can better sample a wider parameter space following Carslaw, Regare, etc.?

Page 9, Fig1: LWP needs a range of uncertainty.

Page 10, L5: so was there an objective way the parameters are chosen? It's not clear how you made a decision. Wouldn't a root mean square error be better than means? How do you know there is not cancellation?

Page 11, L4: Cloud fraction should only be compared if the model and the data are thresholded identically, usually requiring use of a satellite simulator.

Why is there a significant oscillation in mean cloud fraction and LWP fields over the subrropical oceans in the model? That needs to be explained.

Page 13, Figure 3: gray does not stand out from dark blue. I can not distinguish the DACCIWA point. Also, can you give some idea of regime or location, maybe another plot with difference from the 1:1 line as a function of latitude?

Page 14, L5: so the reason for the high SWCRE is likely the high CDNC? Is that a correct statement?

Page 14, L14: in figure 4, the gradients are wrong in EMAC over most of the oceans.

CDNC drop with latitude in EMAC but increase with latitude in the observations. What is going on? This seems like a fundamental error somewhere either in model or data that needs explanation.

Page 15, L5: I'm not sure figure 5 is remarkably good. There are some missing modes in the observations that EMAC doesn't have, and this is a qualitative comparison on a log scale. There are factors of 2 error here. Also, the model needs a range from variability like the observations , not just a line for the mean.

Page 18, L15: for Figure 7 and all the subsequent maps: can you calculate. Root mean square error for all the fields. This is useful for quantitatively understanding pattern biases.

Page 19, Fig 7: why is EMAC so noisy in its cloud fields? If it is sampling then you should use a different method. If it is not a sampling issue, then something is wrong with the model. I've seen this before with radiation code errors leading to jumps in solar zenith angle and incident radiation...

Page 19, L11: does this mean the cloud effect is -1,13 and he rest (-0.63) is A direct effect? That seems large.

Page 20, L23: however, Gettelman et al 2012 found that the ice cloud indirect effect in CAM and ECHAM-HAM is positive. Please document the ice cloud portion of the ACI if you can (a simple simulation where you fix the IN and one where it varies) and compare to previous work, since that seems to be the major development in this paper.

Gettelman, A., X. Liu, D. Barahona, U. Lohmann, and C. C. Chen. "Climate Impacts of Ice Nucleation." Journal of Geophysical Research 117, no. D20201 (2012). https://doi.org/10.1029/2012JD017950.

Page 21, L31: I'm not sure how the parameters were chosen. This should be more objective.

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