Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-573-RC2, 2018

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Elucidating ice formation pathways in the aerosol-climate model ECHAM6-HAM2" *by* Remo Dietlicher et al.

Anonymous Referee #2

Received and published: 6 September 2018

Dear Editor, dear Authors,

I have reviewed "Elucidating ice formation pathways in the aerosol–climate model ECHAM6-HAM2" by Dietlicher et al. The manuscript presents a method to quantify the origin of ice and liquid condensate in clouds. The authors use this method to classify ice origin by homogeneous or heterogeneous freezing and conclude that the high ice bias in mixed-phase clouds in ECHAM-HAM is mostly due to sedimentation of ice that formed by homogeneous freezing.

The manuscript is well written, and the result is both interesting and important. I recommend acceptance of the manuscript.

There are a few questions that the manuscript did not address and that the authors

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might consider clarifying in the final version.

- I found it a bit confusing that the paper tries to do two very different things at once: (1) present validation of a new ice microphysics parameterization (that has already been described in a GMD article) against observations and (2) introduce new tracers to classify the origin of cloud ice, a technique that is applicable to new and old microphysics alike. Scientifically, the second part of the paper is far more interesting, and I feel the first part might have found a better home in the GMD paper. Perhaps there is a way to tie the two parts together a bit more in Sec. 5.2, by describing whether there are significant differences between the new and old microphysics, and in particular whether the new microphysics leads to an improvement. (I realize Fig. 7 does this for the state, but I don't see analogous discussion for the pathways.)

- I agree with the sentiment of the introductory paragraph of Sec. 4 (although I would make an exception for observations that permit inference of process rates or the relative importance of various processes). Of course, this paragraph comes right after a long section that does the exact thing the authors criticize. Perhaps this is an argument in favor of shortening Sec. 3 or moving parts of it to an appendix?

- The previous point notwithstanding, in Sec. 3 (Tab. 3 in particular), I was surprised that the authors provide an uncertainty range for radiative flux observations but not for the ice water path. IWP seems like the more directly relevant variable to evaluate the ice microphysics scheme. It would be nice to see whether passive microwave, MODIS, etc. IWP estimates are as far away from the model as CloudSat/Calipso. Also, why not add the TIWP in the REF model to Tab. 3 under the assumption that the sedimentation occurs within the time step? (And likewise for CIWP in the new configuration?)

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- In the discussion of deposition acting as a sink for cloud cover via the Sundqvist cloud cover scheme (Sec. 2.2), I would have welcomed a sentence or two on whether condensation analogously acts as a sink for cloud cover or how this is avoided. Also, the sentence “However, this coupling also makes the sedimentation sink of cloud ice a sink for cloud fraction” made me wonder: isn’t that realistic, desirable behavior?
- Sec. 3.2, better agreement with GOCCP cloud cover: was this part of the tuning strategy, or did it emerge?
- Sec. 4.3, last sentence: would “cirrus-origin cloud” be less confusing terminology than “cirrus”?
- Sec. 5.1, Fig. 10: The frequencies here are defined by volume. If they were defined by mass, which I assume would be equally valid but give greater weight to warmer clouds, would the conclusions be very different?
- Sec. 5.2, l. 19–21: This seems out of place here; maybe a better place would be in Sec. 3.6?
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A few minor typos etc:

- p. 1, l. 15: “radiative forcing” $\rightarrow$ “radiative effect”, since the clouds are part of the climate system?
- p. 2, l. 21: I kept wondering for the rest of the manuscript why the homogeneous freezing threshold is -35 rather than -38° C.
- p. 3, l. 3: “eluded” $\rightarrow$ “alluded”.

- p. 3, l. 24: Can you comment on how applicable this is to other models?
- p. 4, l. 32: “lead” → “led”.
- p. 7, l. 27: “does no longer require” → “no longer requires”.
- p. 8, l. 23: “an” → “and”.
- p. 13, l. 7: “areaf” → “are”.
- p. 13, l. 15: “thereof” → “therefrom”.
- p. 15, l. 18: “does no longer have” → “no longer has”.
- Tab. 2 uses “QSW” and “HCI”, which I assume are meant to be “LIM_ICE” and “HET_CIR”.
- Fig. 2: Only color scale for differences is included in the plot.
- Koop et al. (2000): citation is missing a DOI.
- Platnick et al. (2017): citation data appears incomplete.

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