Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-573-RC1, 2018

Atmospheric Chemistry and Physics Discussions

ACPD

Interactive comment

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

Anonymous Referee #1

Received and published: 6 August 2018

Decision: The authors study the impact of a new microphysics scheme in a GCM on the representation of cloud and cloud phase partition in the present-day climate. They introduce the differences between their new microphysics scheme and the default one along with some additional hybrid versions of their new scheme. Then, they evaluate the cloud and radiation fields of their new model against a large set of observations. Finally, after identifying a cold bias in the liquid and ice partitioning between 0 and -35ĖŽC (the mixed-phase temperature range), they investigate the reasons of this bias using a Lagrangian method, which allows one to tracing the origin of the overestimated ice clouds. It appears that these extra-ice clouds within the mixed-phase temperatures mostly comes from ice cloud formed at temperatures below -35ĖŽC, i.e., out of the mixed-phase temperature range.

Printer-friendly version

Discussion paper

This paper is within the scope of the journal and generally well written, clearly presented and easy to understand. The topic is of particular interest as it investigates a persisting problem in state-of-the-art GCMs, i.e., the lack (excess) of liquid (ice) clouds. The authors use an original method to tackle this problem and add up interesting results to previous literature. However, the model evaluation part suffers from important flaws (further described below), which should be addressed before final publication. In addition, I believe extra-analysis could be accomplished to strengthen the conclusion of the paper and I provide some way of doing it below. Therefore, I would recommend a major revision before considering the paper for publication.

Main comments: In the model evaluation section, the authors do not describe the dataset at all and do not explain why they chose these specific datasets to evaluate their model. For example, three datasets are used for the fluxes and no reason whatsoever is given to justify this choice. The authors must define/introduce the datasets, even briefly, and explain why they use these. Also, the observed interannual STD can be used as an uncertainty estimates when nothing else is available. In addition, their model evaluation is more of a qualitative comparison than a quantitative one although it is possible to quantify the bias more precisely (see specific comments for further details). It also looks like (it is not specified in the manuscript) they didn't make a consistent comparison between the CALIPSO-GOCCP cloud phase dataset and their model outputs, i.e., they didn't use the simulator for the cloud phase diagnostics, which make the results difficult to interpret. In the second part of the manuscript, I feel like reducing the number of categories in the final part of the study would help to better determine the origin of the ice bias. I'd like the author to either do that or better explain why they chose these categories and what is the added value of making this choice. Finally, the authors could easily check at least one of the mechanism that supposedly lead to the overestimation of the ice cloud occurrences (the overlap assumption).

Specific comments (Introduction): The goal of the paper is a little bit confused and not clearly stated. Also, it is not clear to me how "As has been eluded to above, the formation history of a cloud plays a decisive role, both for mixed-phase and cirrus clouds". The authors should state clearly than one expect biases to come either from ice behavior with respect to liquid within the mixed-phase temperature range or from ice formation at temperature below -38C, and better explain the reasons.

Define the acronyms (e.g., CALIPSO, GOCCP, COSP, CERES etc. . .)

P2 L18-19: The sentence could be re-phrased, "K14 found that even. . . among a some (or number, I believe they used 6) GCMs was not reduced".

P2 L6: I believe ice-containing clouds would be more appropriate. Why not to compare the snow water content + IWC between the REF and 2M models?

P8 L8: but may indirectly affect the cloud in the tropics, especially considering the large amount of high clouds removed

P8 L10 version night time? The authors do not explain what is a simulator at all and why using cosp here. The sentence does not tell much. The SQRT(Xˆ2) of the bias and the correlation pattern number would help better assess the improvement of the new model version.

Fig. 3: There is no height in Fig3 Adding the contour of the difference in the original cloud cover on the bottom plot (i.e., the contour of the blue color, -5% in bottom right plot of Fig. 2) could help identifying areas of improvement. It seems like there is no change at all in middle cloud, which are lacking even in areas with no overlying high-cloud which could cause shielding effect of the lidar.

P8 L18-22: I'm not sure I understand the sentence: "The fact. . ." The authors state that changing microphysics does not affect CRE, that is not true (e.g., Cesana et al., 2017; their Fig. 3). The authors might get similar CREs because they tune the TOA fluxes. Also in their Fig. 4, it is clear that there are regional differences in the GCMs' CREs, i.e., over the Southern Ocean. This bias is worsened by the new GCMs, probably because of less supercooled liquid sustained in the mixed-phase clouds. The authors do not explain why they chose these particular observation datasets. For the fluxes, I believe CERES-EBAF is the most relevant dataset for model evaluation also the longest period of time available (therefore a better climatological estimate of the present-day mean state), which is not defined either. Same thing for the cloud cover, no reason for these specific datasets and while it is mentioned that the simulator is used before (although it is not mentioned why) here no information is given whatsoever. I would recommend using only simulator-derived model outputs against GCM-oriented observation datasets, e.g., ISCCP, simulator Klein and Jakob, 1999 and dataset: Pincus et al., 2012, MODIS, simulator and dataset Pincus et al., 2012, cloudsat simulator Marchand et al., 2008 and dataset Marchand et al., 2010, CALIPSO, simulator Chepfer et al 2008; Dataset Chepfer et al., 2010. The interannual STD may be used as an uncertainty. . .

P8L26: I would suggest adding “In the new scheme (i.e., 2M, 4M). . .” to avoid confusion.

P8L31: Again, it is not quantified at all, so hard to say. With these 2D quantities (i.e., cloud cover), it is easy to compute means, biases and correlation, so please do so and compare to CERES-EBAF.

It is striking to see how little change there is between 2M and REF in terms of cloud cover whereas the vertical cloud fractions are tremendously different. Did the authors look at the high-cloud cover as well? Can they give a hint of why such a small difference in the cloud covers? The cloud overlap may explain this.

P9L5: Again very little information is given about the observational dataset and its weaknesses/strengths.

P10 Sec. 3.6: Is the simulator used in that comparison or do the authors compare CALIPSO-GOCCP to the direct outputs of their models?

P11 Sec. 4: While I agree that the method used here to determine the origin of the overestimation of cloud ice is good, it is not new and it has been used in the past for different topics and referred to as “tendency” (i.e., Brient et al., 2016). It is usually not possible to do so when comparing multiple models – unless a specific experiment is designed to tackle a problem and requires these such as in Brient et al. (2016) -, which is why it does not often appear in multimodel studies.

I do not understand what justify the use of so many types of clouds. The question is where does this ice come from? The answer is threefold from what I understand. Therefore, there should be three categories: Fraction of ice from heterogenous processes Fhet, from homogenous processes Fhom and from nucleation Fnuc. The total would be 100% and figures would be easier to understand.

P13L15: But how to define unrealistic pathways when no observations are available to compare to?

P14: Again, a fraction compare to the total would make more sense.

P14L20: If the simulator is used, then the same weaknesses should affect the model outputs. Also, in the mixed-phase temperature regimes, the undef-phase category can be considered as mixed-phase “likely”. By using ice/total cloud frequency you are considering these undef-phase clouds as being liquid clouds, which is true in the tropics at warm temperature but unlikely at freezing temperatures. Once again, this section raises the question of whether the lidar simulator was used in Fig. 7.

One could also look at particular latitude bands to avoid the influence of these thick clouds and see whether it impacts the Phase-T relationship, e.g., in the Arctic where these clouds are less frequent.

L13: Did the authors mean sedimentation of ice at warmer temperature? i.e., the mixed-phase temperature range?

L15 I’m not sure simplifying ice category from ice crystals and snow ice to only ice can be called as an “improvement”, I’d rather use the word “difference”.

L18-20: No cloud bias below -35ËŽC is shown in this paper and the biases are not well quantitatively quantified. I don’t understand the expression “arguably more reasonable tuning parameters”. This should be clarified.

P16 L5-6: Checking this out by changing the sedimentation overlap to random (or even minimum) overlap and running a short 1yr or even a few month simulation should be relatively easy to do and would strengthen the conclusions.

References

Brient, F., Schneider, T., Tan, Z. et al., 2016 : Shallowness of tropical low clouds as a predictor of climate models’ response to warmin, Clim Dyn 47: 433. https://doi.org/10.1007/s00382-015-2846-0

Cesana G., K. Suselj and F. Brient, 2017: On the dependence of cloud feedbacks on physical parameterizations in WRF aquaplanet simulations, Geophys. Res. Lett., 44, 10,762–10,771, https://doi.org/10.1002/2017GL074820

Chepfer H., S. Bony, D. Winker, G. Cesana, JL. Dufresne, P. Minnis, C. J. Stubenrauch, S. Zeng, (2010), The GCM Oriented Calipso Cloud Product (CALIPSO-GOCCP),J. Geophys. Res., doi: 10.1029/2009JD012251

Chepfer, H., S. Bony, D. M. Winker, M. Chiriaco, J.-L. Dufresne, and G. Seze, 2008: Use of CALIPSO lidar observations to evaluate the cloudiness simulated by a climate model, Geophys. Res. Lett., 35, L15704, doi:10.1029/2008GL034207.

Klein, S.A. and C. Jakob, 1999: Validation and Sensitivities of Frontal Clouds Simulated by the ECMWF Model. Mon. Wea. Rev., 127, 2514–2531,https://doi.org/10.1175/1520-0493(1999)127<2514:VASOFC>2.0.CO;2

Pincus, R., S. Platnick, S.A. Ackerman, R.S. Hemler, and R.J. Patrick Hofmann, 2012: Reconciling Simulated and Observed Views of Clouds: MODIS, ISCCP, and the Limits of Instrument Simulators. J. Climate, 25, 4699–4720, https://doi.org/10.1175/JCLI-D-11-00267.1

Marchand, R. T., J. Haynes, G.G. Mace, T. Ackerman, and G. Stephens, 2010: A comparison of simulated cloud radar output from the multiscalemodeling framework global climate model with CloudSat cloud radar observations, J. Geophys. Res., 114, D00A20, doi:10.1029/2008JD009790.

Marchand, R. T., G. G. Mace, and T. P. Ackerman, 2008: Hydrometeor detection using CloudSat-an earth orbiting 94 GHz cloud radar. J. Atmos. Oceanic. Technol., 25, 519-533, doi: 10.1175/2007JTECHA1006.1.

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