Reply to anonymous Referee #2

Remo Dietlicher

November 1, 2018

Thank your for carefully reading our manuscript and the positive review. Below we elaborate on the points that you mention and outline how we addressed them.

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 dont see analogous discussion for the pathways.)
We completely agree that the validation part of this paper would have fit also together with the technical evaluation in the GMD paper. However, we chose this composition to segregate the idealized single column simulations which highlight the technical aspects of the new scheme from the global evaluation presented here.
Technically, one big improvement that the new microphysics scheme brings is a more readable and manageable code-base. This allowed to easily implement the formation pathway diagnostics. Porting this to the old microphysics code would have been a considerable effort which is why we cannot compare the pathway analysis between the models. The touching point is Fig. 7 where we see a similar phase ratio and thus assume that probably the same mechanisms are in place. Especially since we could trace the high frequency of ice clouds at high temperatures back to the vertical structure of clouds (thick category) which is a result of parametrizations that are similar or even identical in the two models. We briefly addressed this problem now in the introduction to Section 5.

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?
We don't see a viable way to evaluate GCM output other than comparing to climatologies derived from (satellite-)observations. Comprehensive case-studies which allow to infer microphysical process rates usually only target specific clouds and meteorological conditions which cannot easily be generalized to be used in a GCM. We therefore don't want do abandon spatio-temporally averaged model output but rather highlight the fact that in this kind of output a lot of valuable information is lost. In light of your comment, we

have rewritten this part of the introductory paragraph of Section 4 to be more precise.

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?)
You are right, it makes a lot of sense to include the uncertainty range for T/CIWP in Table 3. It has been added. We agree that IWP/C is the most relevant variable to evaluate the new (ice) microphysics scheme. Nevertheless, a direct comparison remains difficult due to uncertainties in the retrievals and the heterogeneous representation of ice in models. In our reference model ice is split up into in-cloud ice and stratiform and convective snow. The new model can uniformly describe stratiform precipitation but the uncertainty from convective ice still remains.
We do not think that we can use a 'diagnostic trick' to homogenize model output. The reference model diagnoses the snow mass flux as $P_{snow} = \int_0^{p_s} Sources - Sinks\, dp$ for the surface pressure p_s. The mass mixing ratio of snow therefore relies on the sedimentation velocity of snow which is rather uncertain since there is no prognostic information on the snow particle size. Similarly, computing CIWP for the new model would require a threshold size or fall velocity above which ice crystals are considered to be snow. This goes directly against a main benefit of the P3 scheme which is eliminating such threshold sizes.

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?
For cloud water we do not have this problem as condensation/evaporation is given by Eq. (2) which is a form of saturation adjustment and does not allow supersaturation w.r.t. liquid water by definition. There is only a problem for cloud ice which either forms from a liquid cloud or nucleates directly from the vapor phase. Both pathways require substantial supersaturation w.r.t. ice. Therefore we need to specify what happens once the initial ice crystals have formed.
Regarding your second point we agree in principle. Our concern with this is mostly the increase of in-cloud ice crystal number concentrations and the resulting feedback loop involving the coupling of aggregation, sedimentation and cloud cover. This is discussed in Section 3.4 paragraph 3 where we argue that it explains the lower cloud cover found in the 2M as compared to LIM_ICE simulation.

Sec. 3.2, better agreement with GOCCP cloud cover: was this part of the tuning strategy, or did it emerge?
The main goal of the new cloud cover parametrization was to consistently extend the notion of the subgrid cloud fraction to the cirrus regime. In the reference model there is a mismatch between how the cloud fraction is diagnosed ($b = 1$ at ice saturation) and the cirrus cloud formation processes (efficient nucleation only at $RH =\sim 140\,\%$). Tying the

cloud cover and cirrus nucleation parametrizations together there is no freedom to tune the parametrization. We now highlight this in Section 3.2.

Sec. 4.3, last sentence: would 'cirrus-origin cloud' be less confusing terminology than cirrus?
This is a philosophical question that came up in the process of this project as well. In my eyes, a cirrus cloud does not lose its 'cirrus'-ness when it crosses a certain temperature threshold. I also like to be very cautious when using real-life intuition on model output. These readily sedimenting cirrus clouds seem to be much more prominent in the model-world than in real-life. So if anything, we could call it 'model cirrus' but then again I guess the 'model' part is implied.

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?
They are actually defined by air mass, not volume. We refrained from calling it cloud mass as this could be confused with the mass of cloud condensate which would drastically alter the relative contributions. We do not believe that there is a substantial change if we use air volume or mass. If we were to use volume the relative contribution of cirrus would probably be somewhat higher.

Sec. 5.2, l. 19-21: This seems out of place here; maybe a better place would be in Sec. 3.6?
You are right in that this would fit nicely in Section 3.6 when Fig. 7 is discussed. However, we need to introduce the formation pathways (Section 4) before we can discuss the different origins of cloud ice. We extended the introductory paragraph to Section 5 to better tie the two parts together.

p. 1, l. 15: 'radiative forcing' → 'radiative effect', since the clouds are part of the climate system?
You are right!

p. 2, l. 21: I kept wondering for the rest of the manuscript why the homogeneous freezing threshold is $-35\,°C$ rather than $-38\,°C$.
In theory the threshold is close to $-38\,°C$. However, our model traditionally used $-35\,°C$ (based on Lohmann and Roeckner, 1996) as a freezing threshold which is why we use this when talking about the model. However, this number does no longer appear in any of the parametrizations of the new model since homogeneous freezing and cloud cover parametrizations have been replaced from the reference model (where a threshold value of $-35\,°C$ is used).

p. 3, l. 24: Can you comment on how applicable this is to other models?
Since it requires solving additional prognostic tracers according to Eqs. (5), (6) and (A1), it is probably unrealistic to implement in a large model intercomparison. However, these tracers are easily implemented if the cloud tendencies are accessible in the code (S-terms in the equations). This is why it is hard to do for our reference model where the computation of tendencies is often intertwined with local variable updates.

Fig. 2: Only color scale for differences is included in the plot.
This is not the case in my PDF-viewer.. Something to double-check when type-setting.

Thank you for finding various typos, they have all been corrected.