Cirrus cloud thinning using a more physically-based ice microphysics scheme in the ECHAM-HAM GCM (acp-2021-685)

Colin Tully, David Neubauer, Nadja Omanovic, and Ulrike Lohmann

Referee #4 Author Response

Thank you for taking the time to review our manuscript and providing useful comments on improving this study. We have quoted each of your comments below with our response.

1. **Comment:** It might be helpful to take a step back and better validate both the shortwave (SW) and longwave (LW) cloud radiative effect (CRE) in the model. After model tuning, it was mentioned on lines 275-277 that the net CRE was too negative and that the 5-year global LW CRE is weaker than the observed range. What is this "structural issue within the model" on line 276 referring to and why does it cause a presumably more negative SW CRE? What is the cause for the CRE biases? Is it due to differences in cloud fraction, cloud height or cloud optical thickness?
    a. While SW as well as LW CRE global mean values are within observational ranges the net CRE is not. This was recognized in different configurations of ECHAM-HAM (e.g. Dietlicher et al., 2019; Neubauer et al., 2019). Furthermore, this holds for many different parameter configurations (not shown) and therefore points towards a possible structural error (e.g. Johnson et al., 2020). Neubauer et al. (2019) report an underestimation of stratocumulus clouds but the exact nature of the possible structural problem is not known. Therefore, we amended the text between lines 275-276 to read:

"We also note a too negative net CRE after tuning. Dietlicher et al. (2019) state this points to a possible structural problem within the model, which relates to the coarse vertical resolution that results in the under-prediction of low-level clouds (Pelucchi et al., 2021)."

    b. In response to your question on what could cause this structural issue, Dietlicher et al. (2019) report an improved vertical structure and high-level cloud fraction in ECHAM-HAM with the P3 scheme but an underestimation of mid- and low-level cloud fractions. They further report an underestimation of cloud ice compared to satellite observations, but part of this underestimation is due to the satellite observations, including convective precipitation, which was not included in the ECHAM-HAM-P3 total ice water content. This underestimation is related to a known problem in ECHAM, involving the coarse vertical resolution employed in the model. A recent study by Pelucchi et al. (2021) reported that low-level stratocumulus clouds extent is underpredicted due to poor representation of the vertical relative humidity profile, and low-level cloud occurrence frequency.

c. A few of your comments focused on SW and LW CRE. Therefore, we decided to collate our explanations related to the changes in the manuscript here for ease. This relates to Comments #1, 4d, 5a, and 7a. Tables 2 and 3 (now Tables 4 and 5 in the revised manuscript) were reconfigured to reflect the SW and LW CREs as well as the 95% confidence interval, see the example layout below. We decided to keep Figure 3 as is to avoid a plot that is too cumbersome. Lines 449-461 were reworked to reflect the changes to the Tables.

Reconfigured Tables 3 and 4 (values provided in revised manuscript)

Seeding Concentration [L^{-1}]		0.1	1	10	100
D19	Net TOA				
	Net CRE				
	SWCRE				
	LWCRE				
S89	Net TOA				
	Net CRE				
	SWCRE				
	LWCRE				

2. **Comment:** Although P3 is a more physically-based ice microphysics scheme, does it result in ice removal processes that are more *realistic*? Please include a discussion in the context of snowfall.
    a. We partially agree with this comment. The vertical velocity of hydrometeors is no longer tuned using the P3 scheme as it is in the default microphysics scheme (Lohmann et al., 2007), and is instead based on particle mass-to-size relationships. Therefore, in the context of sedimentation velocity, we can state that P3 uses a more realistic approach. However, we cannot state whether the P3 scheme is more realistic in the context of other microphysical processes (e.g. accretion, aggregation, etc.). Therefore, we can merely state that the P3 scheme uses a more physically based representation of ice microphysics, which leads to much larger radiative responses due to slower ice removal processes as compared to the default EHCAM microphysics scheme (Lohmann et al., 2007).
    b. **Changes in the text at lines 15-16 in the abstract, and Point #2 under in conclusions:**

"This effect is amplified by longer ice residence times in clouds due to the slower removal of ice via sedimentation in the P3 scheme."

"The prognostic treatment of sedimentation in the P3 microphysics scheme, leading to slower and more physically-based ice removal, is likely the reason why we find such large seeding responses compared to the study by Gasparini and Lohmann (2016), using the default ECHAM 2M scheme. Our model produces smaller and more numerous ice particles

that amplify the already longer ice residence times within clouds to induce a strong positive TOA forcing."

3. **Comment:** The tuning in the model appears to be quite arbitrary. To reduce the overseeding effect in the model, the authors increased Si,seed to 1.35. Why was this particular value chosen, e.g. why not 1.4 or 1.45?
   Response to critical Si value: We chose to increase the critical seeding ice saturation ratio (Si) from 1.05 to 1.35 for two reasons. First, at this value we avoid impacting heterogeneous nucleation on mineral dust as much as possible, which can occur via immersion freezing at a minimum Si of 1.3; dust deposition freezing can initiate at lower Si values. Second, we did not want to make the seeding Si value higher so that seeding particles remain competitive with homogeneous nucleation in our cirrus model, which can occur at a minimum Si value of roughly 1.4. We believe this is justified as this is the first time in a CCT study using a GCM that the sensitivity to the critical Si value was tested. As our results show that this in fact appears to be an important factor determining CCT efficacy, we argue it is justified as a new finding relative to previous CCT studies that could be used to inform further work into this geoengineering proposal.

4. **Comment:** I disagree with the statement that the model "agrees remarkably well with the Kramer et al. (2020) measurements for in-situ formed cirrus" (lines 340-341). The discussion comparing the modelled and measured ICNC appears to be only based on the median values. It appears that there is a large discrepancy in the 215 K to 250 K range for relatively low ICNC (bottom right of plot) which is unexplained. Also, did the Karcher et al. in situ measurements account for the ice crystal shattering effects on probes? Lines 452-454 also seem inaccurate because a small cooling effect is not seen for all seeding concentrations other than S89 Seed100 in Table 3--- it is also small and positive for 5 other values too.
   a. **Response:** This appears to be three separate comments. Therefore, we have divided them into the following sub-points:
   b. **Response to first statement on missing explanation for low ICNC values:** We agree that this explanation should be included in the manuscript. The model agrees well for median values but misses lower ICNC values because we plot annual mean data, whereas the in-situ measurements are instantaneous. **Changes in the text at lines 340-341:**

"The model does not capture the wide variability of ICNC values as seen in the in-situ measurements, as we compare five-year annual mean model data to instantaneous values recorded during various aircraft campaigns. However, for the purposes of our CCT analysis we find that the model median ICNC as a function of temperature agrees well with the Krämer et al. (2020) measurements for in-situ formed cirrus."

c. **Response to ice crystal shattering:** Yes, Krämer et al. (2020) considered ice crystal shattering in their results and aimed to minimize its effect on older datasets where possible. See their Appendix A2.4.
d. **Response to second statement on cooling effect:** We disagree with this comment as what you are referring to is the net CRE anomalies in Table 3. What we cover in Lines 452-454 is the net TOA anomalies, of which all the mean values show a slight cooling effect except S89 Seed100. This is also consistent with Table 3. We agree that there should be some discussion of the net CRE anomalies in line with Figure 3 and Table 3 in this paragraph to make it clearer. For ease, please see the response under Comment #1.

5. **Comment:** Given the competing effects of CCT on both the SW and LW CRE, I would recommend including the breakdown of these effects (as opposed to only the net CRE) in Table 2, Table 3 and Figure 3.
    a. **Response:** We agree with this assessment. The breakdown of the SW and LW CREs is useful in order to understand the impact seeding has on cloud properties. In order to avoid a cumbersome figure, we refrained from adding it to Figure 3, but instead expanded Tables 2 and 3 (now Tables 3 and 4 in the revised manuscript) to show this effect. For ease, please see the response under Comment #1.

6. **Comment:** Figure 5: Please carefully explain the unexpected result of the heterogeneous change in ICNC.
    a. **Response:** We agree that this is not covered in enough detail. This was amended in the text to include a better description of this heterogeneous signal, which also links to the Stratospheric Impacts section further down in the results.
    b. **Changes in the text at lines 371-382 and 418-422:**

"The ICNC anomalies are much clearer and certain for the extreme case, Seed100, than for the Seed1 anomalies (Figure 5c-d). Positive ICNC anomalies exceeding 200 L^{-1} are shown at all latitudes throughout the troposphere, and into the lower stratosphere at higher latitudes. The anomaly heterogeneity around the tropics is likely due to the proficiency of seeding particles to nucleate ice and hamper homogeneous nucleation in convective outflow regions around the tropopause. …"

"… The shift of homogeneous nucleation to lower pressure levels (Figure 6a-b), is likely due to increased LW cloud-top cooling from thicker cirrus cloud following seeding (Possner et al., 2017). This also impacts heterogeneous nucleation on mineral dust particles in the lower stratosphere. As this latter process is not sufficient at consuming water vapor, homogeneous nucleation proceeds to form additional ice crystals. This cloud top cooling effect likely also explains the heterogeneity of the total ICNC anomaly around the tropical tropopause (Figure 5). As there is a clear separation between the troposphere and the stratosphere, these phenomena point to a complex impact on the stratospheric circulation, which we discuss in Section 3.4."

7. **Comment:** Does the intended side effect of CCT on mixed-phase clouds dominate the intended main effect on CCT? The impact on mixed-phase clouds in Figs 7 and 11 seem quite large. Please discuss. I would also recommend adding this result to the Abstract as well.
    a. **Response:** This was discussed in lines 417-427. In the revised manuscript, this is discussed now discussed between lines 429-442, and 560-576. For Si = 1.05 with a seeding particle concentration of 100 L^{-1} we find an impact on lower-lying MPCs through less efficient MP processes that enhances the SWCRE. However, this is outweighed now by the overseeding effect on LWCRE from more numerous and smaller ice crystals in cirrus clouds. For ease, please see the response under Comment #1. In terms of the abstract, we included this with the line: "*due mostly to rapid cloud adjustments*". However, as this is ambiguous, we amended the text in the abstract at **Line 24-27:**

"Our results also show feedbacks on lower-lying mixed-phase and liquid clouds through the reduction of ice crystal sedimentation that reduces cloud droplet depletion and results in stronger cloud albedo effects. However, this is outweighed by stronger longwave trapping from cirrus clouds with more numerous and small ice crystals. "

8. **Comment:** What is the reason for the isolated southern hemisphere cooling effect in the summer due to seeding with Si,seed = 1.35 in Fig. 10?
    a. **Response:** We are unsure whether this refers to the summer quoted on the figure (second row) or southern hemisphere summer (top row). For the former, the isolated areas of SH cooling were due to weaker LWCRE as there is no SWCRE during this season. This points to wintertime seeding having the desired effect in these small regions. If you are referring to the latter (SH summer), which is what we have assumed for the revised text, then small regions of cooling are related to the feedback we find related to MPCs. We agree this is not appropriately covered in the manuscript and have revised the text between lines 546 and 528 to cover more of what we find in Figure 10. Here we quote the text referring to our new results.
    b. **Additional text after lines 528:**

We also find smaller regions of cooling with net negative TOA responses for Seed1 during NH winter in the SH (summer) around 45 °S, and between the Equator and 30 °S (Figure 10a). The net TOA response is driven mainly by negative SW anomalies, indicating either a shift in cirrus formation pathway or an impact on lower-lying mixed phase clouds.

During NH summer the net TOA response is smaller overall than during NH winter. For the Seed1_1.35 zonal mean anomaly we find only small regions of cooling in the NH and in the SH polar regions. However, the uncertainty is wide enough in this case that we cannot determine exact radiative impact in these regions. The small amount cooling shown towards high latitudes in the SH is driven by LW reductions due to a lack of SW radiation in this region during the period, but like the net TOA anomaly is highly uncertain. The few regions of cooling we find in the NH are driven by SW anomalies, highlighting a potential feedback *on cirrus cloud formation or on mixed-phase clouds, but are compensated by positive LW anomalies. This is especially noticeable in the northern hemisphere tropics around the location of the Intertropical Convergence Zone (ITCZ). Thicker in-situ cirrus clouds to some extent reflect more SW (Krämer et al., 2020), similar to the Twomey effect for lower-lying liquid or MPCs. However, they also induce a strong compensating LW effect as a result of seeding.*

Minor:

1. **Comment:** Please include letter labels for every panel of all multi-panel plots.
    a. **Response:** This is a good point. After double-checking, you are referring to Figures 4, 6, and 9. We amended our plotting scripts for these figures to include lettering for multiple plots and have adjusted the text where necessary to reference a specific plot.
2. **Comment:** Line 302: "cannot not" double negative. I think you mean "cannot"?
    a. **Response:** Thank you for pointing that out. We do mean "cannot". The manuscript was edited to delete the double negative.

References

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