Does prognostic seeding along flight tracks produce the desired effects of cirrus cloud thinning? (egusphere-2022-1238)

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28th March 2023

<u>Referee #2 Author Response</u>

To David Mitchell,

> Thank you for taking the time to read and review our manuscript, and to provide useful feedback on aeras of improvement of our study.
>
> I quoted each of your comments below with our responses and changes in the text where applicable.

Sincerely,
Colin Tully (on behalf of all co-authors)

<u>General Comments</u>

This paper is very well written and organized, and the Introduction is particularly well done. Within the context of global climate modeling, there is a lot of interesting analysis, but whether it illuminates the behavior of real cirrus clouds remains in doubt. As stated at the end of Conclusions: "Overall, however, with such high uncertainty surrounding INP perturbation effects on cirrus, we recommend that more observational evidence is needed on cirrus formation mechanisms and the impact that natural as well as anthropogenic aerosol have on cirrus properties before further modeling studies proceed with assessing CCT."

Response: Thank you. We agree that it is unclear whether this is reflective of real cirrus and highlighted this in our discussion and conclusions, as you state.

1. **Comment:** As stated at the end of "Discussion", some of this uncertainty "is partly due to background assumptions in our cirrus model pertaining to the role of pre-existing ice crystals" which makes CCT less effective. I completely agree and would like to draw the authors attention to a recent ACPD paper by Dekoutsidis et al. (2022). This study evaluates lidar-based water vapor measurements made during the ML Cirrus airborne campaign and describes the distribution and temporal evolution of RHi in cirrus clouds. A key finding was that "The uppermost parts of the clouds are mostly supersaturated with RHi frequently above 140%. That is where new ice crystals form", and where RHi is "reaching the threshold for homogeneous nucleation". That is, homogeneous ice nucleation or hom is likely occurring in a relatively thin layer near cloud top and seems to occur only during the "mature" stage of the cloud. Thus, aircraft measurements are likely to miss these hom events both spatially and temporally. Moreover, spiral descents by aircraft through cirrus (e.g., Mitchell, JAS, 1994) show IWC near cloud top ~ 1/10th the IWC near

cloud base, suggesting the pre-existing ice assumption may be flawed if it invokes the model layer mean IWC. A typical cirrus cloud might be ~ 1.5 km thick, comparable with a model layer in the UT. The pre-existing ice treatment described in Shi et al. (2015, ACP) is based on the supersaturation development equation that can be written as:

$$\frac{dSi}{dt} = a1\ Si\ W - (a2 + a3\ Si) \left(\frac{dqi,nuc}{dt} + \frac{dqi,pre}{dt} \right)$$

where qi,nuc is the ice mass mixing ratio due to nucleation and qi,pre is the ice mass mixing ratio of pre-existing ice, parameters a1, a2, and a3 depend only on the ambient temperature and pressure, Si is the supersaturation with respect to ice, W is the updraft velocity and t is time. From this equation it is seen that the greater qi,pre is, the smaller the increase in Si is. This study by Dekoutsidis et al. implies that qi,pre may be overestimated in GCMs since qi,pre is based on layer mean IWC or q values, whereas the actual qi,pre should correspond to a thin layer near cloud top (where qi,pre < qi,mean) that model vertical resolution cannot accommodate. The study by Diao et al. (2015, JGR) shows that ice nucleation in cirrus occurs near cloud top. The modeling results of Spichtinger and Geirens (2009, ACP) appear consistent with these considerations, showing ice crystal production near cloud top and crystal growth at lower levels, which lowers RHi and quenches hom.

For this reason, I question the results in this study and agree with the authors that “more observational evidence is needed on cirrus formation mechanisms”. That is, an inflated qi,pre will depress RHi and generally prevent the RHi from reaching the threshold for hom, forcing heterogeneous ice nucleation to occur much more than it otherwise would. According to Shi et al. (2015), “The pre-existing ice crystals significantly reduce ice number concentrations in cirrus clouds, especially at mid- to high latitudes in the upper troposphere (by a factor of ~ 10). Furthermore, the contribution of heterogeneous ice nucleation to cirrus ice crystal number increases considerably.” The authors do a good job of mentioning how the pre-existing ice treatment promotes het, but they can also mention the limitations noted above.

a. **Response:** This is a good point that we did not consider. Based on that study you cite it does appear that pre-existing ice in our model could be over-predicted, thus impacting the efficacy of CCT. However, our model does not have the necessary vertical resolution at cirrus levels to accurately represent the vertical structure of humidity in cirrus. We extended the discussion section to include this point.
b. **Changes in the text:**

“This is partly due to background assumptions in our cirrus model pertaining to the role of pre-existing ice crystals. Gasparini et al. (2020) and Tully et al. (2022) note that the inclusion

of vapor deposition on to pre-existing ice crystals makes CCT less effective than models that did not include this process (e.g., Storelvmo et al., 2013; Storelvmo and Hereger, 2014; Storelvmo et al., 2014), due to saturation quenching that reduces S_i and prevents homogeneous nucleation from occurring as frequently in the unseeded cirrus. Recent in-situ measurements suggest that the inclusion of pre-existing ice in our model may be over-predicted. Dekoutsidis et al. (2023) analyzed lidar water vapor measurements to assess the in-cloud relative humidity with respect to ice (RH_i) in cirrus. They found that RH_i values often reached the homogeneous nucleation limit (140%) near cloud-top, which coincides with the region within a cloud where new ice crystal formation preferentially occurs. After new ice crystals form, they may grow quickly and sediment and not necessarily have a large impact on in-cloud RH_i at cloud top. Our model does not include sufficient vertical resolution (roughly 700 m at cirrus levels, Gasparini et al., 2016) to resolve the vertical humidity structure in cirrus. This represents a motivation for future work that could aid in resolving the role of pre-existing ice in cirrus, which would have large implications on the efficacy of CCT."

2. **Comment:** Since hom is sensitive to the cooling rate that is determined by the cloud updraft, the treatment of cloud updrafts is critical. The updraft in this ECHAM GCM can be resolved into three components: large scale lifting, TKE turbulence and lifting by orographic gravity waves. Please discuss the treatment of vertical motions in this model and inform the readers whether orographic gravity wave effects were included. These can have a strong impact on cirrus cloud properties (Joos et al., 2008, JGR; Joos et al., 2014, ACP).
    a. **Response:** This study is based on our previous study (Tully et al., 2022) that showed that using the orographic gravity wave parameterization by Joos et al. (2008, 2014) unrealistically increases ICNC in cirrus when using the P3 ice microphysics scheme (Morrison and Milbrandt, 2015; Dietlicher et al., 2018, 2019). We added a note to this in the text.
    b. **Changes in the text:**

"Vertical ascent in our model is represented by the updraft, which is calculated as the sum of the grid mean value and a turbulent component represented by the turbulent kinetic energy (Brinkop and Roeckner, 1995). Note that we do not consider orographic effects on the vertical velocity in our model when using the P3 ice microphysics scheme as discussed in Tully et al., (2022a)."

3. **Comment:** The treatment of pre-existing ice appears to assure the dominance of het which would assure that no cooling from seeding occurs, and that CRE changes must be positive. Therefore, any seeding effect will be a warming effect, as shown in Fig. 3. Nonetheless, this study has value in demonstrating the sensitivity of cirrus properties to seeding, regardless of whether CRE is positive or negative. And it demonstrates the limitations of aircraft seeding. However, in regard to aircraft seeding, it could be mentioned that commercial cloud seeding programs produce AgI seeding aerosol mean diameters on the order of 0.01 µm. Mentioning this would make the r0.01 seeding scenarios appear more realistic.

a. **Response:** Please see our response to your **Comment 1** regarding pre-existing ice. We agree that this issue is still open, so we added an outlook in the discussion section. We are unaware of any commercial applications of AgI seeding with small particles, but we found research on this and included it in the revised text under the Experimental Setup section to justify our small seeding particles.
b. **Changes in the text:**

"Including such small seeding particles in our model (0.01 μm) is justified based on previous work on the ice nucleation ability of silve-iodide (AgI). Xue et al. (2013) formulated a parameterization for glaciogenic cloud seeding with AgI in the Weather Research and Forecasting (WRF) model, using a mean particle diameter of 0.04 μm They reported that the model could reasonably produce the physical processes of cloud seeding. Geresdi et al. (2020) also investigated cloud seeding in the WRF model with slightly larger AgI with a mean diameter of 0.05 μm and reported that the model also reasonably reproduced the microphysical properties of real clouds. Marcolli et al. (2016) reviewed lab-based experiments of ice nucleation and showed that AgI particles of 20 nm in diameter had an increasing ice nucleation efficiency towards cirrus temperatures (238 K). Finally, Kanji et al. (2017) presented new evidence of the ice nucleation ability of small particles such as pollen and fungal spores, which challenges arguments that only large particles are suitable INPs."

Major Comments

1. **Comment:** Line 275: Please explain the difference between "global mean net top-of-atmosphere (TOA) and net cloud radiative effect (CRE) anomalies". The former accounts for everything, including RH changes, while the latter pertains to clouds only. Many readers may not know this.
    a. **Response:** Good point. We amended the text to make this distinction clear.
    b. **Changes in the text:**

"Fig. 3 and Tab. 3 present the five-year annual global mean net top-of-atmosphere (TOA) and net cloud radiative effect (CRE) anomalies for each seeding emissions radius and mass scaling factor that we tested. The TOA anomaly refers to the total "all-sky" (Ramanathan, 1987; Wild et al., 2019) radiative effect (i.e., from clouds, aerosols, surface albedo, and changes in atmospheric gases like water vapor), whereas the CRE anomaly refers to the radiative effect of clouds only. The TOA and CRE anomalies scale with the number concentration of seeding particles (Fig 3 and Tab. 3)."

2. **Comment:** Lines 280-282: The CCT modeling experiment of Gruber et al. (2019, JGR) shows the impact of CCT on lower mixed phase clouds. Do their results support this speculation?
    a. **Response:** Gruber et al. (2019) found seeding led to enhanced riming of cloud droplets, reducing mixed phase cloud cover. We added a brief reference to this in the text.
    b. **Changes in the text:**

"This latter point is the opposite of what Gruber et al. (2019) found for mixed-phase clouds, which was a reduction in cloud fraction through enhanced riming of cloud droplets onto the ice crystals that formed on injected seeding particles."

3. **Comment:** Lines 452-3: This appears true for the mid-seeding case but not the low-seeding case.
    a. **Response:** It is true for both cases, but it is insignificant for our low-seeding case, which we allude to further down in the text. However, as that is unclear, we revised the text for greater clarity.
    b. **Changes in the text:**

"While this signal is somewhat clear for the mid-seeding case, it is unclear for the low-seeding case due to the wide range of the 95% confidence level."

4. **Comment:** Line 454: Should "Fig. 7d" in this sentence be changed to Fig. 7b?
    a. **Response:** Yes, this should read as 7b. Thank you for pointing this out.

5. **Comment:** Lines 472-474: This explanation makes sense based on other studies, but this study shows ice particle size decreases (and presumably fall speeds as well) with decreasing emission scaling (i.e., decreasing INP concentration). This explanation thus appears to contradict the preceding discussion.
    a. **Response:** We agree. This discussion was reformulated in the revised text.
    b. **Changes in the text:**

"As shown above, this is the result of new ice crystal formation onto the injected seeding particles, especially for the high-seeding case, which showed ICNC anomalies that exceeded much of the ICNC in the unseeded cirrus. The smaller ice crystals have reduced sedimentation velocities. This is most pronounced in the mid and high-seeding cases, where we find negative IWC anomalies in the lower mixed-phase regime. However, the ice crystal radius anomalies for these two cases are smaller than the anomaly for the low-seeding case due to an increase in IWC because of less efficient sedimentation."

6. **Comment:** Lines 529-531: Could the use of drones make CCT more viable in this respect, as suggested in Mitchell et al. (2011, Cirrus clouds and climate engineering: New findings on ice nucleation and theoretical basis. In: Planet Earth 2011 - Global Warming Challenges and Opportunities for Policy and Practice, Prof. Elias Carayannis (Ed.), ISBN 978-953-307-733-8, InTech, Available from HYPERLINK "http://www.intechopen.com/artic les/show/title/cirrus-clouds-and-climate-engineering-new-findings-on-ice-nucleation-and- theoretical-basis"). For example, Storelvmo and Herger

(2014) describe a high-latitude seeding approach that would require less flight coverage, and even restricting flights to the Polar Regions would likely result in significant cooling based on their methodology. It seems plausible to increase the density of drone flights in the Polar Regions to address the concerns of this paper. Please comment on this.

   a. **Response:** We had not considered this, but it is a good point to add for future work. We added some discussion on this in the revised text.
   b. **Changes in the text:**

"Mitchell and Finnegan (2009) proposed that if CCT were implemented in the real-world, a potential delivery mechanism could be to use commercial aircraft, which would have a much less homogeneous spatial extent. Later, Mitchell et al. (2011) also proposed using uncrewed drones for seeding particle delivery, which could significantly enhance public safety but could be much more expensive to operate."

"Second, emitting seeding particles from commercial aircraft or from uncrewed drones were proposed as potential delivery mechanisms in the real-world by Mitchell and Finnegan (2009) and Mitchell et al. (2011), respectively. However, aircraft emissions of soot contribute an uncertain effect on cirrus, mostly from uncertainty surrounding the ability of soot to act as an INP (Mahrt et al., 2018, 2020; Lee et al., 2021). In addition, seeding with uncrewed drones could increase the efficiency of potential seeding campaigns by offering dedicated flight paths, but could also be very expensive and associated with legal as well as ethical issues."

<u>Technical Comments</u>

1. **Comment:** Figure 4 caption: There is no mention of the solid and dashed curves shown in these plots; these curves should be defined. They appear to represent the tropopause and the 0°C isotherm.
   a. **Response:** Yes, this was also pointed out by Referee #1. This is fixed in the revised manuscript, including all figures where this is applicable.

2. **Comment:** Figure 7 caption: The y-axis in Fig. 7b appears to indicate microns (change in ice radius) and not temperature as stated in caption.
   a. **Response:** This was also pointed out by Referee #1 and was fixed in the revised manuscript.

3. **Comment:** Line 470: Novemver => November
   a. **Response:** Thank you for pointing out this typo. This was fixed in the revised manuscript.