ACP review by David Mitchell

Manuscript title: Does prognostic seeding along flight tracks produce the desired effects of cirrus cloud thinning?
Author(s): Colin Tully et al.
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General Comments:

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."

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:

$$\frac{dS_i}{dt} = a_1 S_i W - (a_2 + a_3 S_i)\left(\frac{dq_{i,nuc}}{dt} + \frac{dq_{i,pre}}{dt}\right)$$

where $q_{i,nuc}$ is the ice mass mixing ratio due to nucleation and $q_{i,pre}$ is the ice mass mixing ratio of pre-existing ice, parameters a_1, a_2, and a_3 depend only on the ambient temperature and pressure, S_i 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 $q_{i,pre}$ is, the smaller the increase in S_i is. This study by Dekoutsidis et al. implies that $q_{i,pre}$ may be overestimated in GCMs since $q_{i,pre}$ is based on layer mean IWC or q values, whereas the actual $q_{i,pre}$ should correspond to a thin layer near cloud top (where $q_{i,pre} < q_{i,mean}$) 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 $q_{i,pre}$ 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.

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).

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.

Major comments:

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.

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?

Lines 452-3: This appears true for the mid-seeding case but not the low-seeding case.

Line 454: Should “Fig. 7d” in this sentence be changed to Fig. 7b?

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.

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/articles/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.

Technical Comments:

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.

Figure 7 caption: The y-axis in Fig. 7b appears to indicate microns (change in ice radius) and not temperature as stated in caption.

Line 470: Novemver => November