Review of “Response of cirrus clouds to idealised perturbations from aviation” by Ella Gilbert et al., submitted to Atmospheric Chemistry and Physics (ACP)

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This study uses a large eddy simulation model to quantify the impact of ice crystal number concentration (ICNC) perturbations on the water budget and microphysics of pre-existing cirrus clouds. It examines two specific types of cirrus—gravity wave cirrus and warm conveyor belt outflow cirrus—and their responses to simulated perturbation in ICNC, which are intended to represent the effects of aerosol emissions from aircraft. The research finds that higher ICNC extend the cloud's lifetime by reducing the size of ice particles, which then take longer to grow and sediment out of the cloud. This effect is more pronounced in the gravity wave cirrus case. Additionally, the study assesses the sensitivity of the ice water path (IWP) to these perturbations, noting that the degree of impact varies based on the type of cirrus cloud and its initial conditions of ICNC.

I appreciate the authors' effort in utilizing the LES model for this study and their examination of how cloud microphysical processes evolve during perturbations over time. The paper is engaging, and its language makes it accessible and straightforward to follow. The descriptions of the simulations are both clear and concise. However, I feel that the paper does not fully deliver on what the title promises. The methodology and assumptions used to assess the impact of aviation on ICNC changes in cirrus clouds require substantial revision. Therefore, it seems inappropriate to link the study's conclusions to the effects of aviation, even if idealized. Consequently, I recommend that this paper undergo a major revision before it can be considered for publication in ACP.

General Comments

1. Environmental Suitability for Contrail Formation

Before studying the effects of contrails on cirrus clouds, we must first verify whether contrails will form under the given environmental conditions or not. The Schmidt–Appleman criterion provides the threshold temperature, representing the warmest possible conditions conducive to contrail formation. This criterion depends on ambient air pressure, humidity, and specific aircraft characteristics. Additionally, contrails will only persist if the ambient humidity is at least saturated relative to ice.

I highly recommend that the author check and demonstrate whether the cloud cases are appropriate for contrail formation before analyzing their main results. I question, especially, the suitability of the GW case described in lines 150-154 for studying the impact of contrails, as it is characterized as "a relatively warm and low-altitude cirrus cloud, with a mean modeled in-cloud temperature of 229 K and formation occurring at 8.5 – 9.5 km altitude, with a supersaturation of ~10% with respect to ice." However, this 110% is at the start of the simulation, and for realistic calculations, the author should use the supersaturation at the time they applied the perturbation, which is at the end of the spin-up phase at t=1500 s in the GW case. Perhaps the information in Figure 5c is more relevant, which clearly shows that in-cloud relative humidity over ice is around 100%-100.3% (also mentioned in lines 375-377).

Given that the top of the atmosphere is at 22.5 km, it can be inferred that the case is assumed to be in a mid-latitude region. Comparing this with two relevant studies, Verma & Burkhardt (2022) indicate that typical cruise levels in Germany range between 10.3 km and 10.8 km, where the ambient temperature often falls well below the contrail formation threshold. Additionally, Li et al. (2023) indicates that the most frequent aircraft cruising altitudes correspond to a pressure range of 200–245 hPa (207–218 K in temperature). Based on the provided discussion, I strongly believe that the GW case does not meet the Schmidt-Appleman criterion for contrail formation and is not a suitable case for the purpose of this study.

2. Contrail-Cirrus Cloud Interactions and Methodological Approaches

In studying contrail-cloud interaction, it's crucial to consider several key factors. First, the ice crystals from contrails are notably smaller than those in natural cirrus clouds, due to the combustion of fuel which releases limited water vapor and high number of nucleated ice crystals. Consequently, contrails tend to introduce smaller ice crystals into cirrus clouds, creating perturbations that are not accurately represented by merely multiplying the ice crystal number concentration of natural cirrus clouds, which typically feature a different mass mean diameter (Voigt et al., 2017; Unterstrasser, 2017; Verma & Burkhardt, 2022).

Furthermore, the number of ice crystals formed during contrail formation varies based on aircraft and fuel characteristics, influenced by factors like the release of aerosol particles and the atmospheric conditions (Kärcher et al., 2015). When an aircraft passes through a cloud, it introduces (if the persisent contrail forms at all) additional ice crystals into the atmosphere based on these parameters, suggesting an increase in ICNC by “addition” rather than by “multiplication” of the initial concentration which is used in this study.

Additionally, the method introduced in the paper, which involves multiplying the initial concentration of the cirrus cloud by various scale factors, sets another issue. By employing this approach, we essentially assume that when the ice crystal concentration of a non-perturbed cloud is higher, then the aviation perturbation is also stronger. This assumption may affect the interpretation and comparisons of the cases in the results.

Another important consideration is the interaction between the high concentration of small ice crystals in contrails and the relatively lower concentration of larger ice crystals in natural cirrus clouds. The dynamic interplay between these two groups of ice crystals is crucial and yet is overlooked in this study.

3. The Need for Reassessing ICNC Perturbations

After reviewing the findings from Marjani et al. (2022), I've noticed several critical arguments in their paper that seem to be overlooked in the current manuscript regarding the selection of ICNC perturbation values based on their result. The ICNC data in Marjani et al. was derived from DARDAR-Nice retrievals, which accounts only for ice particles larger than 5 micrometers. Considering that contrail-generated ice crystals are typically much smaller than those in natural cirrus clouds, the dataset from Marjani et al. (2022) predominantly represents those ice crystals larger than 5 micrometers. Consequently, adopting ICNC perturbation values based on these results without discussing or accounting for the 5 micrometer size threshold in your simulation is not a valid scientific assumption to proceed with.

From Marjani et al. (2022) paper: the mean diameter of ice crystals in young contrails (up to 1 hour) is typically smaller than 10 μm (Bock & Burkhardt, 2016b; Schröder et al., 2000). It is even less than in young cirrus clouds, which were found to be 10–20 μm (Schröder et al., 2000). **Therefore, it is possible that we have lost information about a certain fraction of contrail's ice crystals, those which are smaller than the retrieved threshold of 5 μm in the DARDAR-Nice product.**

4. Demand for Statistical Accuracy in Median Change Analysis

A Considerable part of the analysis in the manuscript is based on observing the median changes in various atmospheric variables like temperature, updraft speed, relative humidity, IWC, and ICNC between the control and perturbation simulations, labeling these differences as "large" or "small." However, the analysis doesn't really dig into the statistics to support these claims. To make the findings more solid, it would be essential to include tests for statistical significance and to share the uncertainty ranges for these median values. This approach will ensure that the observed changes are both statistically significant and relevant to the study's objectives.

5. Magnitude of the effect vs. Sensitivity

The paper mostly talks about the magnitude of the effect, especially highlighting how much more the GW case perturbations affect the Ice Water Path (IWP) compared to the WCB case. Understanding both how sensitive and how big these effects are is important to really get what's going on when things change in a system, but the paper doesn't really focus much on the sensitivity part (last section). It kind of sticks to talking about how big the effects are. Toward the end, though, it suddenly says that the WCB outflow might actually react more to changes in ICNC, but it doesn't do a good job of explaining or connecting this idea back to the earlier parts. This flip-flop between focusing on how big the effects are and then jumping to sensitivity without much explanation might leave readers a bit confused.

Additionally, I think it would make things clearer and more comparable if, especially in the sensitivity analysis, we compare the cases based on equal changes (the addition of constant amount of perturbation in both cases instead of multiplication as discussed earlier in comment 2). This way, we can observe how each case responds to the same level of perturbation, making it easier to understand and compare their responses.

Specific Comment

1. Abstract

To enhance clarity and coherence, it would be beneficial for the authors to include a short explanation in the abstract of how this methodology is representative of aviation impact.

2. Lines 25-26

The sentence suggests that the more pronounced effect in gravity wave cirrus compared to warm conveyor belt outflow cirrus is due to the latter having lower initial ICNC and ice water content. However, this reasoning may need clarification. It's not necessarily the lower ICNC and IWC that directly cause the difference in effect between the two cases. **Instead, it could be attributed to the strength of the perturbation, which is stronger in the gravity wave case due to initial higher ICNC.** Therefore, it might be more accurate to specify that **the stronger perturbation in the gravity wave case, resulting from higher initial ICNC, leads to the more pronounced effect**. Clarifying this point would enhance the interpretation of the sentence.

3. Lines 114-121

In the final paragraph of the introduction, there's a lack of explicit explanation regarding the study's connection to aviation, similar to the abstract. Instead, the focus is primarily on the aerosol-cloud interaction, outlining the two steps involved in this process. While the authors mention examining the response of cloud microphysical processes, properties, and water budget to ICNC perturbations, which aim to mimic the addition of ice-nucleating particles (INPs) that freeze heterogeneously, the direct link to aviation is not clearly articulated. If the study aims to investigate the impact of aviation, it should be explicitly addressed in this section.

4. Section 2.2 Case Description

It would be beneficial to include a brief explanation in the case description section regarding the differences in model top height between the two cases (22.5 km and 15.9 km), the rationale behind applying water vapor forcing in one case but not the other, and the decision to apply wind forcing in one direction in one case and in both directions in the other. Providing this explanation will help readers better understand the model setup and the reasoning behind the choices made.

5. Lines 182-186

The explanation effectively outlines how the perturbation was incorporated into the model. However, it would be crucial to further elaborate on how these perturbations are representative of contrail perturbations within the cloud environment. Contrails contribute to the presence of high concentrations of small ice crystals. Therefore, it's inappropriate to represent contrail signatures simply by multiplying the ice crystal number concentration (ICNC) in natural cirrus clouds, which usually have different mean mass size. (explained more in general comment 2)

6. Lines 191-192

Why were the IWC×2 and ICE×2 experiments only done for the GW case? The manuscript mentions around lines 299-301 that "Considering that WCB cirrus are common in mid-latitudes, a region that has a high density of air traffic, the impacts of increasing ICNC on WCB outflow cirrus may be considered more directly relevant for examining potential aviation-aerosol-cirrus interactions". Then again in lines 579-580 the manuscript mentions that "the GW cirrus occurs infrequently in the atmosphere". Then wouldn't it make more sense to dive deeper into the WCB case?

7. Lines 229-230

It's somewhat unusual to find higher rates of homogeneous nucleation at the cloud base and higher rates of heterogeneous nucleation at cloud top. Providing an explanation to this unusual vertical distribution of nucleation processes would enhance clarity.

8. Lines 394-403

The text mentions a median updraft speed increase from 3.34 to 4.63 cm/s, which is about a 38.6% rise, yet labels it as a minimal effect. Meanwhile, it describes changes in ice crystal mass mean radius — from 12.39 to 11.43 micrometers in the GW case and from 14.72 to 14.2 micrometers in the WCB case — as large differences. This inconsistent interpretation of changes could be confusing. The labeling of changes as large and small needs further clarification to enhance the analysis's consistency and credibility.

9. I suggest discussing the differences in r_{ice} values with at least one decimal point to ensure precision. While rounding may not significantly impact the r_{ice} differences in the GW case (e.g., from 12.39 to 11.43 vs. 12 to 11, which results in nearly 1 micrometer difference), it does affect the WCB case. Here, rounding 14.72 and 14.2 to 15 and 14 alters the difference from 0.52 to 1 micrometer. **This is especially relevant since the author describes these as large differences in line 400**.

10. section 3.4

I found the discussion on IWC perturbations in the GW case within Section 3.4 particularly interesting. However, it remains unclear how this section contributes to the main claims made in the paper's title regarding perturbations from aviation. Could the authors clarify the connection and explicitly detail how these IWC perturbations are representative of those caused by aviation activities?

11. Figure 6 and Figure 3

It appears that Figure 6 is identical to Figure 3. I request the author to check if the same figure was mistakenly uploaded twice.

12. Lines 455-457

The concern mentioned here is crucial, yet its placement feels somewhat out of context. I was surprised it wasn't emphasized earlier, especially during the discussion of Figure 5 in Section 3.3. It is important to frequently remind readers that **the quantity of doubling in the GW case represents a significantly**

stronger perturbation compared to the doubling in the WCB case. This should also be taken into account when analyzing the results.

13. Lines 477-479

The text mentions that in-cloud RH_{ice} remains higher in ICNC x2 simulations compared to control, yet it is unclear where these values are obtained from. I could not find them in Table 2 or Figure 5. If the mentioned medians are from Table 2, then GW case shows a **decrease** of 0.06%, and WCB case shows an **increase** of 0.8%. Could the author please clarify the source of the values in these lines?

Minor Comment

1. Lines 21-22

In the abstract, it says the study focuses on **'the second half of the chain'** but doesn't really explain what this 'chain' is all about. Later, around lines 114-116, the paper does a good job explaining this chain as a two-step process related to how aerosols interact with clouds. It might help if the abstract gives a clearer explanation of this chain to make things easier to grasp for the reader.

2. Line 45

It would be helpful to cite specific examples or studies that illustrate these discrepancies.

3. Lines 139-140

The author could enhance the flow of the sentence by considering a slight refinement. For instance, 'In the model, at simulated cirrus temperatures,' could be replaced with 'At the cirrus temperatures simulated'.

4. Reference

The reference for Yang et al. (2012) seems to be missing from the reference list. It is recommended that all references cited in the text be included in the reference section.

5. Line 159

The given unit includes an extra "kg^{-1}"

6. Figure 1

It is suggested that the time in the figure be presented in seconds rather than hours, as mentioned in the accompanying text. This adjustment would facilitate comprehension for the reader and consistency within the paper.

7. Please ensure consistency in the notation used for variables like "r_{ice}" and "R_{ice}" . Additionally, maintain uniform terminology across the manuscript, tables, and figures for clarity.

8. In figures 3 and 6, it is recommended to include the cloud type in the figure title, as done in figures 1, 2, 5, and 7.

9. In Figure 5, please ensure consistency in the notation used for 'mmr-weighted' and 'MMR-weighted' between the y-axis title and the caption.

10. Please ensure consistency in the terminology used for 'IWC-weighted RH_{ice}' and 'Ice MMR-weighted RH_{ice}' throughout the document.

11. Line 386

The phrase 'larger ICNC perturbations' could be misunderstood as referring to larger ice crystals. It is suggested to revise to 'larger perturbations in ICNC' for clarity.

12. Reference

The reference for 'Lee et al., 2021,' cited multiple times, is missing from the bibliography. Please add it.

13. Lines 450 and 452

The referenced information is actually found in Table 2, not Figures 3a and 6a. Please correct this.

14. Line 468

Should "larger" be "smaller"? Please check.

15. Figure 8

Correct the unit on the colorbar to "mg" instead of "m g"?

16. Lines 518-519

The punctuation at the end of each sentence could confuse readers about the sequence and relationship of these variables, potentially being mistaken for a dot product. Please consider revising for clarity.

17. Lines 535-536

It seems the described relationship might be reversed. Could the author please verify this?

References:

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Voigt, C., Schumann, U., Minikin, A., Abdelmonem, A., Afchine, A., Borrmann, S., Boettcher, M., Buchholz, B., Bugliaro, L., Costa, A. and Curtius, J., 2017. ML-CIRRUS: The airborne experiment on natural cirrus and contrail cirrus with the high-altitude long-range research aircraft HALO. *Bulletin of the American Meteorological Society*, *98*(2), pp.271-288.