## Response of Report #2 (Anonymous Referee #1)

The authors have sufficiently addressed most of my questions and comments. However, I still have some points for minor revisions before the manuscript should be accepted.

1. In Table 1, the ice crystal size in the jet phase is noted as “large and concentrated”, which conflicts with Figure 6 and Table 2.
   **Response:** Thanks to the reviewer for the valuable suggestion. Table 1 has been removed from the revised manuscript.
2. With the presence of Table 2, Table 1 is not necessary. Or the authors may think to implement the qualitative assessment using an arrow marking the increasing or decreasing trend, or with color gradients.
   **Response:** We do agree that Table 1 is really not necessary now. It has been removed from the revised manuscript.

3. I understand the authors want to give a general understanding of the contrail ice status. It would still be relevant to make notations in the table to tell readers from where the estimated numbers are derived, and the same for the techniques and models.
   **Response:** Thanks to the reviewer for thoughtful comment on Table 2. We appreciate your understanding of our intention to provide a general overview of contrail ice properties and measurement techniques/model considerations. We acknowledge the limitations of presenting estimated values without specific references. While exact values may vary across studies, we compiled Table 2 to offer a broad range representative of contrail properties and measurement techniques/model considerations at different stages. We drew upon our understanding of various research articles to populate the table with ranges. To address your concern and enhance the value of the table for readers, we have provided references, 12 in total, related to the values presented in the table and the same for the techniques and models.

4. Line 681: "0:5 h" changes to 0.5 h
   Response: Thanks to the reviewer for pointing this out – revision made.

5. Table 4: Since the models are evaluated with in situ, satellite, or reanalysis data, can the authors also add a column listing the uncertainties of the models based on their validation method?
   Response: Thank you for your thoughtful comment on Table 4. We appreciate your suggestion to include model uncertainties based on their validation methods.
   We have addressed this in the revised manuscript (attached in updated Table 3).

6. Table 9: I suggest the authors add to the caption the models that are included for discussing the agreement and disagreement, or at least indicate the table number or section number if the models are listed elsewhere.
   **Response:** Thank you for your suggestion regarding Table 9. We've addressed this by including the list of compared models directly in the caption for improved clarity.

Additionally, we've incorporated a brief explanation of the key agreements and disagreements between the models directly below Table 9 (Table 8 in the revised manuscript) to enhance the reader's understanding (highlighted in blue font).

7. Line 1613: Newer paper by Gierens et al (2020) has shown the underestimation of ice supersaturation in reanalysis data.
   Response: Thanks to the reviewer for the suggestion, the Gierens et al (2020) reference has been added ((highlighted in blue font in the revised manuscript)

8. Line 1667: Remove the old paragraph.
   Response: Thanks to the reviewer for the suggestion. We have removed that paragraph.

9. Line 1839-1861: Too long. The authors can shorten this paragraphs to keep a similar length with the other bullet points.
   Response: Now, the paragraphs are shortened to one short paragraph (highlighted in blue font in the revised manuscript)

10. Reference: Mahnke et al (2022) is in the reference list but was removed from the main body. It is outdated, new entry https://doi.org/10.1021/acs.est.3c09728
    **Response:** Thanks to the reviewer for pointing this out. Now we have cited the update published by Mahnke et al. (2024) in ES&T.

## Response of Report #1 (Anonymous Referee #2)

I am sorry that I was less clear when commenting on the paper the first time during the discussion phase. I saw chances for an overview of the wide set of achievements of recent years and I saw chances for improvements. But now I see that the authors are driven by the aim to emphasize uncertainties above knowledge and progress.
The authors criticize the whole community; e.g.:
“Many global climate chemistry models used to study the physics and chemistry affecting the Earth’s climate system typically do not incorporate the impacts. Models that do account for contrails estimate contrail cirrus coverage based on simplified treatments of contrail aging and spreading mechanisms in ice-supersaturated regions (ISSRs) (Burkhardt et al., 2010; Burkhardt 86 and Kärcher, 2011; Bock and Burkhardt, 2016; Bier and Burkhardt, 2022; Chen and Gettelman 2013; Bickel et al., 2020; Schumann et al., 2015).
What scientific basis do the authors have? Do they ever have done work on contrail modeling? How can they be so negative without constructive suggestions? This is not what we need.
The paper is not scientifically open-minded and neutral but clearly biased to highlight all kinds of “uncertainties”. New findings are hardly acknowledged but cited with the comment that something might be missing
**Response:** Thank you for your clarification and for taking the time to re-evaluate the paper. We understand your initial vision for a broader overview of recent achievements, and we appreciate

your suggestion. However, our primary aim is to provide a balanced perspective by acknowledging both the progress made and the remaining uncertainties in contrail modeling. Our intention was to highlight areas where further research could lead to significant advancements. As a result, we had many other scientists, including most of the modelers that have done contrail studies, review the manuscript. They gave us positive comments and did not disagree with our discussion.
Regarding the critique of our approach to contrail modeling, we want to clarify that our goal was not to undermine existing models but to identify gaps and suggest directions for improvement. The references to previous studies were intended to contextualize our arguments within the broader scientific discourse, not to dismiss the work of others.
Once again, thank you for your guidance and support in improving our manuscript.

**Minor points:**

1. Line 155:" Soot emissions influence the meteorological factors…" that sounds wrong.

**Response:** Thanks to the reviewer for the suggestion, now we omitted the sentence because it added very little to the overall discussion.

2. Chapter 2.1 is very badly organized. Many aspects are repeated at various places,. E.g., text in lines 298 to 310 (Contrails are created …"), at least to a large extent, repeat statements made before. Lines 13-15.

**Response:** Thanks to the reviewer for pointing this out, now we have removed the redundancies.

Abstract.

3. The third sentence „While contrails …life cycle" is redundant, and should be deleted. The present fourth sentence uses the word "uncertainties" without any specification. It would be more reasonable to replace it by: "Despite extensive research, the relative importance of the climate effects of contrails compared to other aviation effects remain under debate."

**Response:** The abstract has been revised and the sentence revised.