# Review of revised version (1st revision) of
# Dedekind et al, subm. to ACP

## Synopsis

The revised manuscript has improved and several components of the study are now described more clearly. In addition, a variation of $EI_{soot}$ has been added, following the suggestion of the other reviewer (although I am not certain that the other reviewer explicitly asked for this).

I had not examined the other review of the original manuscript before submitting my previous report. It was interesting to see that the other reviewer criticized different aspects of the study. One comment we shared was that the descriptions and explanations are insufficient for a reader to understand what has been done.

After reading the authors' reply to my previous review, I can confirm that several important components of the study design were omitted from the original draft. Unfortunately, the revised manuscript is still not in a state that allows me to understand exactly what was done.

In my first review I noted that the manuscript covers two loosely related topics; I still think this is the case.

*Regarding the evaluation of NWP performance for ISSR prediction*: the authors do not demonstrate that their advanced contrail treatment provides any additional insight into how to assess ISSR-prediction skill. The NWP ISSR skill could have been evaluated over a larger spatial domain and a longer time period by comparing it directly to measurements of RHi. Is it really necessary to simulate contrails in order to obtain a better impression of the NWP prediction skill of RHi?

In its present form the manuscript discusses a single idea to increase predictive skill—adjusting the deposition coefficient $\alpha_d$. However, the effectiveness of this adjustment is demonstrated for only one synoptic scenario. It therefore remains unclear whether an adaptation of $\alpha_d$ would be beneficial for ISSR prediction in other synoptic regimes. Moreover, I would have liked a broader discussion of other possible NWP-model adaptations that could improve ISSR prediction, and a comment on whether adjusting $\alpha_d$ would be a viable solution in other NWP systems.
Recently, other approaches to improve supersaturation forecasting in NWP models have been described (e.g., Hanst *et al.*, 2025).

The additional topics addressed in the study are also insufficiently elaborated because, in my opinion, too many different subjects are covered in a single manuscript. The current findings are rather heterogeneous:

- **Adjustment of $\alpha_d$ in the NWP model** to enhance ISSR prediction in one case study. The general implications are unclear, as noted above.
- **Development of the CoAT model**, which combines the well-known Schmidt-Appleman criterion with an existing ice-crystal-loss parametrisation of the vortex phase. Including the parametrisation of Kärcher *et al.* (2015) could have provided an estimate of the initial number of ice crystals before vortex-phase losses. The comment from the other reviewer *"This includes, e.g., the number of ice particles (ice nucleation) and assumptions on sub-grid-scale variability"* may refer to this point, i.e. that ice crystal formation in contrails also depends on the meteorological environment.
  For me, it seems CoAT is the algorithm for contrail initialization in GEM-P3, isn't it?

- **Contrail-cirrus in GEM-P3:** Very little is reported about the subsequent contrail-cirrus evolution. How are contrails treated within GEM-P3?
- **Variation of aircraft type and EIsoot**, which demonstrates that the definition of contrail persistence may depend on the values of these parameters.

I recognize that this heterogeneity cannot be easily remedied. I therefore recommend a clearer focus in future studies.

Nevertheless, I have several **major comments** that must be addressed before I can recommend publication. The manuscript still lacks a proper description of what has been done; many formulations are ambiguous or vague. At one point I stopped drafting my review because I had to speculate too often about the authors' procedures. Clearer, more precise descriptions are essential for me to assess the work.

## Major comments

1. **Treatment of contrails within GEM-P3** – Although the description of the CoAT model has improved, the manuscript still does not explain how contrails are handled in GEM-P3. I could not find any information, even though Figures 5–8 display GEM contrail results.
   - Do you use separate cloud categories for natural cirrus and contrail-cirrus? If not, how can the two cloud types be distinguished?
   - Are natural and contrail cirrus governed by the same physical processes?
   - How is the competition between the two cloud classes implemented?
   - The caption of Fig. 7 states that *CINC* is advected, which would imply that contrail ice crystals are treated as a passive tracer. The heading of Sect. 2.2.4 ("tracking …") also suggests that contrails are only advected. This remains unclear.
   - Because I do not understand the contrail-modelling approach of GEM-P3, I still do not grasp how you define *persistence*.
2. **Figures 5–8**
   - **Fig. 6** – Does the CINC field represent the contrail from the red flight track, or from previous/other flights? If it is the former, why does CINC remain larger than zero well above the 300 hPa flight altitude? The relationship between the plotted flight track and the CINC field is therefore ambiguous.
   - **Fig. 8** – This figure shows changes in contrail IWP, which suggests that contrail ice particles are not treated as passive tracers. Please clarify the governing processes.
3. **Generality of the optimal deposition coefficient** –
   In my previous review I asked:
   *"You determined an optimal deposition coefficient for one synoptic scenario: How universally valid is this value? Using GEM-P3 for other synoptic scenarios, would you obtain a similar value? Is your optimal value also relevant for other microphysical models, or do you consider it to be only a tuning parameter of your P3 model?"*
   The authors replied:
   *"In response to the reviewer's concern in point 4, we have implemented a direct calculation of $\alpha_D$ within P3 rather than heuristically modulating $\alpha_D$. By diagnosing $\alpha_D$ as a function of temperature, pressure, ice particle radius, and humidity, the formulation becomes physically based and broadly applicable to other synoptic scenarios and microphysical models, rather than serving as a tuning parameter specific to P3. The description of the modifications made to the P3 cloud microphysics scheme is explained in Section 2.2.2 and Appendix A."*
   I appreciate this clarification. I understand that the formulas for $\alpha_d$ are also applicable in

other synoptic scenarios. Yet, my original question has not been answered: Whether a similar adjustment of $\alpha_d$ would be required in other synoptic situations, and whether the resulting value can be transferred to other models? In other words, how universal are your findings?

## Minor comments

The line numbers refer to the manuscript without track changes.

4. **Line 7** – "high-resolution simulations of what?" – please specify what has been simulated.
5. **Lines 50-55** – Volatile particles (UFP) are always formed; the ambient temperature controls only how much they contribute to ice crystal formation.
6. **Line 58** – You write "contrail formation". Contrail formation is completed within the first few seconds; the wake vortices therefore affect the evolution of the contrail over minutes, not the initial formation stage.
7. **Sentence on condensation and freezing** – The wording "*water vapor condenses and freezes onto soot and ambient aerosol particles*" could be misunderstood" could be misinterpreted. It should read: "Water vapour first condenses onto soot and ambient aerosol particles; the resulting liquid droplets subsequently freeze."
8. **Citation of Li *et al.* (2023)** – The statement "*Although Li et al. (2023) demonstrated that contrails may persist under ice-subsaturated conditions*" is misleading. Contrails can only *grow* in supersaturated environments; they may *persist* for a while after the environment becomes subsaturated, but they cannot develop under subsaturation. Re-phrase or relocate the citation.
9. **Line 287** – "initially detected?" – the meaning is unclear; please elaborate.
10. **Line 289** – It would be clearer to write "no contrails are initialized".
11. **Caption of Fig. 7** – I do not think concentrations are advected. Ice crystals are advected. Adjust the wording accordingly.
12. **Eq. 8** – The notation should indicate that *P* denotes a percentile of TICD.
13. **Use of "formation" and "persistence"** – These terms are not used consistently throughout the manuscript; please adopt a single definition for each and apply it uniformly.

## Technical corrections

14. "Albany" and "Albony" appear in the text.
15. **Equation 1** – Write the function as dgr(RHi, T,..) to make clear that it is not a constant.
16. **Table 1** – Round the drift distances to integer values; this better reflects the uncertainties associated with their evaluation.
17. **Text vs. figure** – In the text you refer to 240 hPa, but the red line in the figure appears to be at 250 hPa. Correct the discrepancy.
18. **Definition of TICD** – TICD is never defined explicitly. Please state that it is the column-integrated ice-crystal number concentration (or the intended definition).

## References

Hanst, M., Köhler, C. G., Seifert, A., & Schlemmer, L. (2025). Predicting ice supersaturation for contrail avoidance: ensemble forecasting using ICON with two-moment ice microphysics. *Atmospheric Chemistry and Physics*, 25, 17253–17274.