Review of Dedekind et al, subm. to ACP

Synopsis

This manuscript presents a study that is motivated by better forecasting contrail occurrence and deals with improving the prediction of ice-supersaturated regions (ISSR) in an NWP framework. It is suggested to adjust the deposition coefficient, which modifies the life cycle of natural cirrus and hence ice supersaturation in the GEM-P3 model. A variation of this parameter to better forecast ISSRs is novel and interesting.

The study also improves several aspects of existing studies dealing with the same topic. For example, contrail formation is not implemented as a simple binary decision based on the SAC but includes an estimate of the contrail ice crystal number considering ice crystal loss during the vortex phase.

I can recommend publication after the following comments are suitably addressed.

Major comments

1. From my perspective, the manuscript has two separate topics:
    1. Better forecasting relative humidity and ice supersaturation by adjusting the deposition coefficient. As a consequence, contrail-forming areas are better predicted, which is a valuable goal.
    2. A case study comparing contrail occurrence in model and multi-faceted observations.

    Both topics are relevant, but it should be made clearer that both topics are somewhat independent of each other. For the first topic, a less detailed description of contrails would suffice to identify ice-supersaturated regions. What's your opinion about my perspective?

2. It is not clear how you define contrail persistence. It seems that contrails that have enough ice crystals at the end of vortex phase are called persistent? In GCMs or in contrail plume models, contrails are typically initialized after the vortex phase. Hence, 5-min old contrails may be considered being part of an extended formation stage. Then, persistence relates to contrails with larger lifetimes (let's say e.g. at least 60min, but choosing a precise threshold is certainly subjective). Similarly, around line 80 you state that contrail formation covers the vortex phase. This would contradict your persistence definition (t_{age} being larger than around 5min). Please define formation and persistence and use it consistently. Check also line 399, it may be beneficial to describe in the summary section again precisely what is meant with formation and persistence. Typically, SAC only refers to temperature-controlled formation criterion and not really to persistence in ice-supersaturated regions. Please, be verbose that every reader knows exactly what is meant.
3. A)
    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?
    B)
    Please also state explicitly that your contrail initialization does not depend on the deposition coefficient, i.e. your CoAT model is independent of α_d. Both, ice crystal formation and ice crystal loss during the vortex phase may, however, depend on α_d

but this is neglected in the parametrisation of Unterstrasser (2016). Lewellen (2014) looked at a variation of the deposition coefficient.

4. The deposition coefficient is varied only implicitly, see description Appendix A1. I wonder whether it wasn't technically possible to directly vary the parameter within P3. This would simplify the presentation of the results and remove the uncertainty by the non-ideal relation between deposition coefficient and mass growth rate for differently sized ice crystals (e.g. the appendix would be superfluous).
5. I am not sure whether all relevant information is given to understand how contrails are initialized in your model. You mention Unterstrasser (2016), which provides a parametrization of ice crystal survival fraction. What's your choice of the initial ice crystal number (prior to loss)? Does this number depend on ambient conditions? (for ambient temperature close to the SAC temperature, not all soot particles are activated (Lewellen, 2020, Bier et al, 2022)). How do you determine number concentrations n, which requires the specification of a contrail cross-section? How do you determine the ice mass concentration m? Do you specify a mean mass to obtain m from n? How are contrails initialized in subsaturated air, for which Unterstrasser (2016) makes no prediction? Moreover, Unterstrasser (2016) assumes an ice-supersaturated layer with a constant RHi value. As a consequence, the parametrization was not made for shallow layers where the wake vortices move out of the moist region and for which it seems you apply it.
   At the end of section 2.2.3 you write that the surviving ice crystals grow through deposition. Due to the very short paragraph, I am not sure whether you really simulate contrails beyond the vortex phase? Then I would assume that you had to feed the contrail properties into P3, which is however not explicitly stated. You also write that GEM advects the number and mass concentrations? Are the contrail ice crystals treated as passive tracers? Or do you really include microphysical processes? Could you please clarify this.

Minor comments

1. The title of the study could be refined to „Improved forecast of persistent contrail <u>occurrence</u> …", because you do not simulate persistent contrails
2. line 52: turbulence is typically not a crucial process. Shear and sedimentation are much more important for contrail spreading, see Lewellen, 2014 or Unterstrasser & Gierens, 2010.
3. I do not understand why precise time periods and drift distances are provided for 300hPa.
4. The definition of G is introduced in Schumann 1996, not in Schumann 2012.
5. Line 211: you may cite Lewellen 2012 (instead of Jensen 2024)
6. Fig 2 says 5km, but the text states 30km. What is true?
7. Line 317: what are the numbers in the square brackets?
8. Is correct that α_d is implicitly defined by Eqs. A3 and A4?
9. Section A1 uses radius, Figure A1 uses diameter. Moreover, the axis titles are not "nice" (long, different styles etc.).
10. In general, I believe the font sizes in the figures are too small.
11. Line 55: Unterstrasser (2016) provides a parametrization of early contrail properties. Contrail-cirrus simulations are presented in Unterstrasser et al, 2017a,b or Lewellen, 2014a,b
12. Line 421 and 427: It depends which models you talk about. In typical LES, no phase relaxation (saturation adjustment) is used.

13. The parametrization of Unterstrasser (2016) has been applied in regional and global-scale models. You may find similar findings on ice crystal loss in those studies (Gruber et al, 2017 and Bier & Burkhardt, 2022)

Technical corrections

14. A temperature cannot be warm, cooler or colder. This is a feature of the air mass. Temperature is low/high. Similarly, a rate is not fast, only the process that is described.
15. I would not refer to wake vortices as wake turbulence. Once the vortices break up and no coherent dynamical structures exist, then the elevated turbulence intensity might be called wake turbulence.
16. I believe you forgot to delete the sentence in lines 191 & 192.
17. Fig.7 should zoom into the 200 to 400hPa layer. In the present form, most areas in the panels are just white and wasted space.
18. Make sure to use the same subscript: you use α_d and α_D.
19. I prefer a mathematical correct notation in eq. A4. One can use units in equations (s_crit/% = … ΔT/K).
20. Fig. A1 should extend the y-axis down to 0.0 in all panels.
21. Please check your reference section thoroughly; e.g. CO2 in line 562
22. Line 150: Why is a refinement of the tropical troposphere relevant in your study?
23. Line 390: from STUDY XX?
24. Line 405, for the summary section, it might be useful to explain again what Dep_0.8 refers to.
25. Line 424: Should this be 0.2 mm = 200µm?

References

Only those papers are listed that do not appear in your manuscript.

Bier, A., & Burkhardt, U. (2022). Impact of parametrizing microphysical processes in the jet and vortex phase on contrail cirrus properties and radiative forcing. *J. Geophys. Res.*

Gruber, S., Unterstrasser, S., Bechtold, J., Vogel, H., Jung, M., Pak, H., & Vogel, B. (2018). Contrails and their impact on shortwave radiation and photovoltaic power production – a regional model study. *Atmos. Chem. Phys.*, *18*(9), 6393–6411. https://doi.org/10.5194/acp-18-6393-2018

Lewellen, D. C. (2012). Analytic solutions for evolving size distributions of spherical crystals or droplets undergoing diffusional growth in different regimes. *J. Atmos. Sci.*, *69*, 417–434.

Lewellen, D. C. (2014). Persistent contrails and contrail cirrus. Part 2: Full Lifetime Behavior. *J. Atmos. Sci.*, 4420–4438. https://doi.org/10.1175/JAS-D-13-0317.1

Lewellen, David C. (2020). A Large-Eddy Simulation Study of Contrail Ice Number Formation. *J. Atmos. Sci.*, *77*(7), 2585–2604. https://doi.org/10.1175/JAS-D-19-0322.1

Unterstrasser, S., & Gierens, K. (2010a). Numerical simulations of contrail-to-cirrus transition - Part 1: An extensive parametric study. *Atmos. Chem. Phys.*, *10*(4), 2017–2036. https://doi.org/10.5194/acp-10-2017-2010

Unterstrasser, Simon, Gierens, K., Sölch, I., & Lainer, M. (2017). Numerical simulations of homogeneously nucleated natural cirrus and contrail-cirrus. Part 1: How different are they? *Meteorol. Z.*, *26*(6), 621–642. https://doi.org/10.1127/metz/2016/0777

Unterstrasser, Simon, Gierens, K., Sölch, I., & Wirth, M. (2017). Numerical simulations of homogeneously nucleated natural cirrus and contrail-cirrus. Part 2: Interaction on local scale. *Meteorol. Z.*, *26*(6), 643–661. https://doi.org/10.1127/metz/2016/0780