Contrail models lacking post-fallstreak behavior could underpredict lifetime optical depth

Report of Reviewer #3

Summary

The study presents a timely analysis comparing two widely applied simplified contrail models that are typically used to evaluate contrail mitigation options. The benefit of mitigation options in terms of reducing some contrail climate metric can in most cases only be based on model-based estimates. Rerouting strategies aim to avoid 'big-hit' contrails. Yet, the prediction of those high-impact contrails with simplified models is challenging. The skill of the models is unknown as the validation with observations is limited in the sense that such extreme contrails have not been sampled and is unlikely to be possible due to their long lifetime. Hence, comparing two models for a set of meteorological parameter combinations is very interesting and the study is insightful.

General comments

1. The writing style is in my opinion too informal and formulations are too often too vague and not precise enough. Moreover, it is not explained sufficiently how you compute specific values.
   Some examples are listed in the specific and technical comments, but my list will not be exhaustive. Hence, I strongly recommend that the authors go over the whole manuscript and work on the text. Sloppy formulations make the life of the reviewers and future readers harder than it should be.
2. I am not sure if "fallstreak" and "post-fallstreak" are good expressions for what you want to describe. The first phase (that you refer to as "fallstreak") is dominated by the creation of the fallstreak that fills the moist layer underneath the flight altitude over time. Once the fallstreak covers the whole layer, you speak of "post-fallstreak". In my understanding, the contrail at that stage still consists of a contrail core and a fallstreak. The fallstreak continues to exist and is fed by ice crystals falling out of the contrail core. Hence, I would not call it "post-fallstreak".
   [In the following review, I will stick to your terminology and will not make any further comments whether I think the terminology is appropriate.]
   Moreover, you speak of a Cocip fallstreak. As a single Gaussian plume is used in Cocip, this model cannot represent the bimodality of the contrail (i.e. contrail core and fall streak as e.g. described in the high-resolution modelling study by Lewellen 2014). Hence, referring to the Cocip plume as Cocip fallstreak is misleading as the Cocip plume falls only slowly in the beginning and accelerates only very late in its lifecycle. Why not use the more neutral term 'Cocip (Gaussian) plume or contrail' throughout the text?
3. The prescribed meteorological scenarios are highly idealized and it is likely that subsidence causes the contrail to sublimate before it reaches an age of 15 hours. Hence, the comparison should emphasize the early differences more than the discrepancies beyond 10 hours. I doubt the "fading sub-regime" will be encountered as such very often.
   The change in the slope of the ice crystal number reduction might be a particular

result of APCEMM and the idealized scenario used. In reality, vertical motions in the atmosphere will perturb the contrail evolution.

Specific comments

4. Line 39: Can you substantiate the statement about thin contrails having the largest cloud radiative effect (what you call local RF)? Unterstrasser & Gierens (2010) and Lewellen (2014) show at least the dependence on wind shear.
5. Line 40: Typically, the introduction of scientific publications does not summarize the results of the present study.
6. Line 44: In my opinion, contrails with cross-sections of 100km^2 represent extreme cases. Or did you want to say, the grid boxes of the gridded models are 100km^2? In this case, please reformulate.
7. Line 47: This sentence is very general and contains little information. Which models were compared to each other? In which paper was the comparison done? What are the main findings?
8. Line 77: I thought Cocip only tracks a Gaussian plume for the ice crystals and the humidity is taken from NWP data. Why is it necessary to have a plume of water vapour concentrations?
9. Line 96: are the bins fixed in radius space or dynamic as in Lewellen 2014?
10. Line 82: 'evaporate' or is it 'vanish/disappear'?
11. Line 114: I am not sure whether the title is appropriate. Could you reformulate it? "Meteorological background scenarios/data"??
12. The quantities you define in Eqs. 1 and 2 have been used in previous studies, yet with other names. It would be good to make the connection to those studies. Unterstrasser & Gierens 2010 introduced the total extinction, which is equal to your definition of γ. Since then, total extinction has also been evaluated in the context of GCM contrail simulations (Bier et al, 2017). Moreover, total surface area S in Lewellen 2014 is basically the same as total extinction (except for a constant scaling factor of 2).
    Your definition of "lifetime optical depth" was introduced as '(life)time-integrated total extinction' in Unterstrasser (2020).
    I would recommend to stick to one of the names that have been previously introduced to make clearer that all these studies analyse basically the same quantity.
13. The crystal loss rate is not well-introduced and I stumble across the units. Is the logarithmic derivative of N(t) used?
14. Around line 190: You analyse dI/dt and d^2I/dt^2 which serve as conditions in a contrail phase classification. It makes the impression that those conditions can used as classification criterion across different scenarios. I doubt that the signs of these two quantities are universally interpretable as they may depend on many parameters (such as the thickness of the moist layer, vertical air motions and so forth).
15. Section 3.2.1: The study by Bier et al (2017) also analysed what factors limit the contrail lifetime.
    Matching maximum ISSR lifetimes alone are not a sufficient criterion ensuring that your meteorological background state is representative. The characterization of the sub-regimes is more complicated in scenarios where the background humidity changes over time due vertical air motions and ice mass evolution changes by

those 'external' drivers. Hence, your claim of widespread applicability is probably a bit overselling.

16. If I understand Fig. 6a correctly, the x-coordinate for the blue and the according orange data point are the same. Correct?
    I understand the information given in the text about which fraction of the contrail lifecycle is unobservable (based on $\tau<0.1$). Basically, Cocip contrails are nearly always observable and 100% of their lifecycles belong to the fallstreak regime. Due to these rather peculiar values, the panel b is difficult to understand. First of all, the legends in the two panels say 'fallstreak only' and 'fall streak'. Is this the same criterion?
    In the text you mention that on an aggregate level, 92% of Γ_{APCEMM} comes from post-fall streak regime. It is not explained how you derive this number. Is this the ratio of the orange and blue slope in Fig. 6a? Are all data points equally weighted in the averaging? Do you take the average over the ratios $\Gamma_{APCEMM,\ fallstreak}$ / $\Gamma_{APCEMM\ all}$? Or do you sum up over $\Gamma_{APCEMM,\ fallstreak}$ and $\Gamma_{APCEMM\ all}$ separately and then compute the ratio of the two sums?
    Similarly, I miss information about how the values 35% and 15% (in lines 248 and 249) are computed. Are these the mean values of the orange data points in x and y direction in Fig. 6.2?
17. section 4.2.2: You compare APCEMM and Cocip sensitivities with those found in Lewellen 2014. Unterstrasser & Gierens 210a,b also studied contrails in scenarios with constant RHi and analysed the sensitivities to most of the parameters listed in your table 2. Hence, it would help including the findings from these studies in your discussion.

Technical comments

18. Abstract, first line: what does 'optimized' imply?
19. Line 56: sublimation is a specific physical process, whereas formation and persistence are more general terms. Moreover, contrails can disappear through other physical processes. Replace 'sublimation' by 'demise'?
20. Fig.2: Would it be possible to use white as colour for the zero IWC bin, which would help to better identify the borders of the contrail?
21. In Eq.3, should the index be 3 (and not 'n')?
22. Line 199: "During the fall streak"?? and "fastest center of mass fall rate" (a rate is not fast, it is large)
23. Line 214: I am not sure what **local** optical depth means.
24. Line 215: 'is produced at **times** where …'. Better use time instead of point to make clear it is about time and not space. Moreover, I would prefer to use plural to make clear you consider a time span over which the contrail is not detectable.
25. Caption Fig.6: 'unobservable'
26. Line 262: 'at the end of the APCEMM fallstreak **regime(?)'; '**shear does not increase the contrail width'. It is true that shear increases the contrail width. But here you want to say that a larger shear value leads to a larger contrail width.
27. Line 337: 'once the fallstreak ends': in time or space?
28. Line 345 I believe it should be 'Contrail avoidance strategies **that**' because the following clause is restrictive. Same in line 385: 'which' -> 'that'.
29. Line 348: and also lifetime-integrated radiative effects?

References

[Only those are listed that are not listed in the manuscript under review.]

Bier, A., Burkhardt, U., and Bock, L.: Synoptic Control of Contrail Cirrus Life Cycles and Their Modification Due to Reduced Soot Number Emissions, J. Geophys. Res., pp. 11 584–11 603, https://doi.org/10.1002/2017JD027011, 2017JD027011, 2017.

Unterstrasser, S.: The Contrail Mitigation Potential of Aircraft Formation Flight Derived from High-Resolution Simulations, Aerospace, 7, 170, https://doi.org/10.3390/aerospace7120170, 2020.