Review of contrail study by Jafarimoghaddam & Soler subm. to JECATS

Synthesis

The manuscript describes a new modelling framework for simulating the long-term evolution of contrails. Here, long-term does not refer to climatological time scales, but to simulations of individual contrails over their full life cycle.

Although the authors claim that advanced numerical schemes are employed, several *a priori* modelling assumptions impose strong conceptual limitations.

The introduction claims that the new model uses more sophisticated representations of ice crystal habit and sedimentation.

With regard to ice crystal habit, I have serious doubts about the way habit is included in the model and whether the approach is reasonable.

With regard to sedimentation, the use of a monodisperse size distribution does not allow sedimentation to be represented realistically. This may obscure the significance of the improved physical description of bulk settling claimed by the authors.

The study would also benefit from comparisons with contrail-cirrus models other than CoCiP. CoCiP itself is a heavily parameterised 0D model and cannot reproduce the typical contrail-cirrus lifecycle seen in higher-dimensional contrail-cirrus models.

An earlier version of this manuscript was submitted to ACP and was not accepted. I was one of the reviewers. The revised manuscript is better structured and easier to follow than the earlier versions. However, the main scientific concerns raised during the ACP review process have not been adequately addressed. These concerns concern the model concept and the selection of physical processes represented.

Therefore, I again recommend rejection for the reasons outlined below. Parts of this review are based on my earlier ACP reviews (see, e.g., https://doi.org/10.5194/egusphere-2025-4155-RC2).

Major comments

Comments 1–9 were already raised in two rounds of the ACP review process and have not been satisfactorily addressed. Comment 10 concerns the newly written Section 6.

1. Unbalanced inclusion of processes

The manuscript argues that the model is intentionally simplified so that hundreds of contrails can be simulated at regional scale. This rationale is used to justify important approximations, such as the assumption of a monodisperse size distribution. However, these simplifications are difficult to reconcile with the ambition to represent habit effects or to claim improvements due to a more detailed fall-speed parameterisation. Comments 2 and 3 identify fundamental

limitations of the model concept, while Comments 4 and 5 question the claimed advantages associated with two specific model components. Comments 6 to 9 raise additional issues that are not adequately resolved.

2. Monodisperse assumption in microphysical modelling

A monodisperse size distribution is a very strong assumption. Earlier LES modelling studies (Unterstrasser und Gierens 2010, Lewellen 2014, Lewellen, Meza und Huebsch 2014) have shown that only the largest ice crystals fall out from the contrail to form a fall streak, while the smaller ice crystals remain at the original altitude or fall only very slowly. I doubt that the superiority of the numerical implementation (as claimed by using an advanced description of the bulk settling velocity) matters as long as a monodisperse size distribution is assumed.

Allowing for a size distribution with pre-defined shapes would be one step forward. This can, however, lead to spurious sedimentation effects when ice mass and ice number vertical fluxes become decoupled. The bin approach (i.e. spectral microphysics) used by Lewellen (see citations above) or the Lagrangian approaches (Naiman, Lele und Jacobson 2011, Paoli, Hélie und Poinsot 2004, Unterstrasser, Gierens und Sölch, et al. 2017, Nybelen und Paoli 2009) are conceptually far more advanced than the present scheme. Already in 2006, (Huebsch und Lewellen 2006) demonstrated already that a monodisperse assumption is inadequate.

Recently, (Akhtar Martínez, Eastham und Jarrett 2025) showed that accounting for polydispersity is essential for simulating contrail fall streaks realistically. Therefore, this model improvement should not be postponed to future work. The statement at the end of Section 1 that an "extension to polydispersity is straightforward and reserved for future studies" is not appropriate, especially because the authors claim that a better fall-speed parameterisation provides a more realistic representation of fall streaks. Such a claim cannot be substantiated with a model based on monodispersity.

I strongly recommend to include polydispersity in your model. It remains to show that this advancement is really straightforward to implement (as mentioned in the manuscrupt). This would require introducing a suitable number of size classes, implementing particle transitions between classes during growth, and performing convergence tests.

3. Neglect of wind shear

Wind shear is ubiquitous in the upper troposphere and has been shown to strongly affect contrail spreading and properties. Its representation should be an integral part of the model. Vertical wind shear displaces the upper part of the contrail relative to the fall streak. As a consequence, falling ice crystals may enter contrail-free regions rather than the fall streak.

The manuscript states that the separation ansatz can be applied only for zero wind shear. As I understand it, the full equation set without the separation ansatz has also been implemented. I therefore strongly recommend including non-zero wind shear in model testing and evaluation.

4. Sedimentation

Around line 150 the authors write: *"Long-term diffusion and habit development (tens of minutes to hours): Once the jet-induced turbulence has dissipated, only the ambient wind and residual turbulence remain. In this regime, two competing inertial-turbulence mechanisms,*

loitering (enhanced drag from small-eddy sampling slows settling) and preferential sweeping (partial decoupling drives larger crystals into downward-moving regions), govern the net particle descent."

This is one example of a text snippet, where I cannot judge the appropriateness. I never came across any contrail studies that mentioned those effects and the authors miss to explain it properly nor do they show evidence of the relevance. Given the fact that polydispersity and vertical wind shear and their relevance on vertical ice transport have been neglected, I am not convinced that these effects are crucial.

How does the proposed formulation of bulk settling velocity compare with established empirical fall-speed parameterisations (Mitchell 1996, Böhm 1989, McCorquodale und Westbrook 2021, Heymsfield und Westbrook 2010)?
I appreciate that the authors question the universal applicability of terminal velocity and extend the model beyond this assumption. However, I have strong doubts that sedimentation is adequately represented in a model that assumes a monodisperse size distribution in each grid cell. Does the contrail layering in your simulations resemble that shown in Fig. 4 in (Lewellen, Meza und Huebsch 2014)? Would this improve if polydispersity were included?

5. Ice crystals habits

Given that several other components of the model are highly simplified, I am concerned that the inclusion of habit evolution may be over-engineered relative to the overall level of detail of the model. It is questionable whether predicting precise habit fractions within a single contrail is meaningful.

In the paragraph on habits (lines 85 to 105) the sentence "*Specifically, once contrail ice crystals exceed a critical size (typically around 5 - 10μm diameter), they experience microphysical behaviors analogous to those in natural cirrus clouds.*" draws a direct connection between habit evolution in contrail cirrus and natural cirrus. While I agree that the general (micro)physical laws are the same in both cloud types, you still have to consider that the two cloud types have completely different formation mechanisms and form in different environments (global statistics regarding encountered ambient temperature, ambient supersaturation). Moreover, contrails feature much higher ice crystal number concentrations than natural cirrus and have a different appearance. Hence, the relevance of the various microphysical processes and also the susceptibility to variations of ambient parameters differs. The paper by Yang et al 2010 that you rely on support my opinion by stating "*The aircraft-generated ice crystal habits (i.e., shapes) may be similar to those in natural cirrus, but their distributions (i.e., the percentage of various habits) may be different from those in natural ice clouds*.".

You write "*Empirical evidence from in situ sampling at approximately -61◦C shows that contrail ice crystals with effective radii larger than about 10 μm predominantly exhibit habits characteristic of natural cirrus: roughly 75% hexagonal plates, 20% columns, and a smaller fraction of triangular plates*."

This empirical evidence is based on 2 minutes of in-situ measurements in a single aircraft plume with an age of around 1 minute (Goodman et al, 1998). Given the data scarcity and

measurements in a very young contrail and for one particular ambient state, I doubt that any general patterns for contrail-cirrus habits should be inferred from it.

I do not doubt that habit assumptions can affect the contrail optical depth as shown in Fig.10. I am not arguing that treating all ice crystals as spheres is reasonable. Several contrail LES models described in the literature incorporate ice crystal habits with increasing aspect ratios, but do not include any additional habit aspects that are extrapolated from very limited observational data.

Nevertheless, given the uncertainties in the present approach and the scarcity of data, I strongly doubt that including time-dependent habit mixtures on a single-contrail basis meaningfully improves the model. This representation implies a level of detail that is not supported by the available evidence.

6. Treatment of water vapour (appendix G)

Relative humidity rises and water vapor becomes available when air masses ascend and cool adiabatically or some other diabatic cooling mechanisms. This is the usual driver of continued cloud growth. I am not aware of studies in which background water vapour is replenished by diffusion; in stratified flows, vertical exchange is generally weak.

The authors define an entrainment time scale $\tau_{entrain}$, which is expressed in terms of the inverse of a logarithmic time derivative of some mass M. However, it is not shown how $\tau_{entrain}$ or the latter quantity can be evaluated from available model fields, nor is it demonstrated that this process is important. To my knowledge, other single-contrail models do not include replenishment of background water vapour by diffusion. The authors should provide evidence justifying this choice.

Moreover, the strength of the water-vapour replenishment appears to be spatially uniform. This discards information about whether a grid cell lies near the contrail boundary or deep inside the contrail.

7. Derivation of mean mass (appendix A4)

The derivation of the equation for the mean mass is lengthy and, in my opinion, not needed. The authors state that the mean mass m is given by C_M / C_N in the case of monodispersity. Irrespective of the shape of the size distribution, the mean mass is always given by the above formula. As already explained in the ACP review process, I do not understand, why it is not sufficient to have prognostic equations for C_M and C_N and diagnose m by C_M / C_N. What is the advantage of solving an additional, more expensive prognostic equation for m rather than simply computing C_M / C_N?

My concern is the following: How can the authors ensure that the PDE solutions for C_M, C_N and m are compatible with each other? Given the complexity of the m-PDE and numerical considerations, I doubt $m = C_M/C_N$ will be fulfilled. If this condition is violated, your solution is inconsistent. If it is satisfied, there is no need to solve the complex PDE for m; you could just use $m = C_M/C_N$?

8. Unusual behaviour in earlier simulations

During the ACP review, several simulation results were reported that I found unusual. Since the underlying physics and implementation have not been substantially modified for the present submission, I would like to understand why these phenomena no longer appear in the presented simulations.

- The earlier version showed wavy contrail structures, which were attributed to the prescribed wind field. Does this behaviour still occur? This is not clear from the current manuscript, possibly because time-evolving (y,z) cross-sections are not shown.
- The earlier version also showed cases in which the contrail top rose by several hundreds of metres, which is difficult to reconcile with the absence of a prescribed large-scale updraught.

Can you explain why the simulations presented here do not show this behaviour?

1. Fig. 12 of the current manuscript shows that the centre of mass of the contrail changes very little over several hours: less than 30 m in 5 h for CoCiP and less than 20 m over 15 h for MultiCon. It seems unrealistic that sedimentation is so weak that the centre of mass changes by only few tens of metres over many hours. There are three interpretations:
    - The setup of the test case is unrealistic.
    - The numerical implementation of sedimentation has flaws.
    - Let's assume, the test case is well-chosen, the implementation is correct and the results are correct: Based on your result, sedimentation appears negligible, which is in stark contrast to your efforts of improving the details of the implementation. This contradiction is not reconciled in the manuscript.

9. <u>Comparison with other contrail models</u>

The comparison with other contrail models is very limited. The CoCiP model is heavily parameterised and zero-dimensional. A comparison with CoCiP is useful because it is widely used, but it is not comprehensive.

From the way the model is presented, I understand that the proposed model lies between CoCiP and LES approaches in terms of complexity. The question remains whether the inclusion of habit evolution and an improved fall-speed parameterisation are second- or third-order effects compared with the apparent limitations of the model, such as monodispersity [Not to mention that I am concerned with how habits are considered]. The model should be compared with higher-dimensional models.

CoCiP and APCEMM produce qualitatively different results. (Akhtar Martínez, Eastham und Jarrett 2025) showed that CoCiP may be too simple because of its monodispersity assumption—which is also used in the present model—and its 0D plume assumption. The present model is one-dimensional, or effectively two-dimensional if the separation ansatz is dropped and contrail tilting due to vertical wind shear is allowed. I therefore strongly recommend comparisons with APCEMM, using the test cases from from (Akhtar Martínez, Eastham und Jarrett 2025) and/or with LES results (e.g. with Lewellen, 2014).

10. <u>Section on test cases and sensitivity studies</u>

The sensitivity simulations provide a useful starting point. However, some parameter choices appear questionable, and the test cases should be harmonised. In addition, important sensitivities identified in previous contrail studies are missing.

- The cross-section of the initialized contrail is tiny. The initial contrail half width values (σ_z = 20m and σ_y = 10m) are not representative. Because of wake-vortex descent, contrails are usually much thicker and broader by the time of initialisation in the present model.
- Consistent settings should be used across the different test cases. For example, why is T = 222 K used in Case 1 (variation of initial ice crystal number) but T = 230 K in Case 2 (variation of background supersaturation)? Case 3 returns to 222 K. In the following paragraph, line 470, the degree of subsaturation changes from −0.15 to -0.05 without clear justification. The statement that the temperature "profile follows the standard atmospheric lapse rate" is also unclear; the stratification used should be specified.
- Figs. 4 to 9 show ice mass concentrations (in mass per volume). It would be useful to show the corresponding ice crystal number concentration, i.e. number per volume, rather than the auxiliary quantity g(z).
- Units are frequently omitted in this section.
- I strongly recommend analysing the effects of polydispersity and non-zero wind shear, as both are major drivers of contrail spreading.
- The first part of Section 6 addresses numerical convergence. Unlike the later test cases, the physical setup of this simulation series is insufficiently described. Information on s_{env}, thickness of supersaturated layer, temperature etc is missing. Fig. 3 shows the dependence on Δz, but an analogous plot for variations of Δt is missing.

Minor comments

11. Table H1 on CoCip setup

Table H1 summarizes the parameters used to initialise a CoCiP simulation. This information is of limited value unless readers already know how CoCiP processes these parameters and how they affect the simulation. It would be more useful to summarize the final properties of the initial contrail. Without context, it is, e.g., totally unclear why the longitude/latitude centre (0.1° and 50°) is specified and how it affects the results.

12. Introduction

- Line 22, you miss to say that CO_2 emissions of aviation only are regarded.
- Lines 81 and 82: the wording should be "the MISSING coupling" and "the MISSING interactions".

13. Acknowledgment

It is not necessary to thank the anonymous reviewers prior to the peer review process 😊

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