Review of : A milti-physics Eulerian framework for a long term contrail evolution : single plume analysis.

General comment:

This paper propose a new framework for simplified contrail simulation in its diffusion phase. This model is able to solve habit evolution of ice crystals and bulk settling velocity. This model is describe in detail and I congratulate the authors on the clarity of their explanations. It is then applied in 2D (z,t) and a sensitivity study has been made to numerical parameters (time step, discretization in z) and physical parameters. Finally a 3D (x,z,t) simulation has been made to compared the new model to CoCiP, the most used model for this kind of simulation by the community.

I find the idea used by the authors to developed a complete multiphysic eulerian-eulerian framework and then simplified the system with several assumption leading to very low computational time very interesting. I think most of the approximations are good, however I regret in the writing that little order of magnitude are given to justify them. It also lack of comparison with other model, there is one with CoCiP in one case, but there is results from APCEMM and CoCiP available in the literature as well as 2D LES results. It should have been great to have more comparison with other model.

In conclusion, I think this paper is interesting and deserve to be published. However, I think some part deserve more detail. In the following there is more detailed remarks.

Main Comment:

1. Line 178-180 you mentioned that you assume a monodisperse particle distribution. This is a big assumption that should not been made as suggest by https://doi.org/10.5194/acp-25-12875-2025 . You tell in the abstract that it is a first step. It should either been mentioned here that it a first step, or to be proven that monodispersity by cell is enough for contrail modeling.
2. In appendix A you write that the PBE is f(x,t,m). Since you consider the habit of the particle, and since two ice crystals with the same mass may have two different shape as shown in https://doi.org/10.1175/2010JAS3562.1 , phi should be consider in the phase space. Then I think at first you PBE should be written as f(x,t,m,phi) and then either derive the complete equations of justify the simplification.
3. The equation A2 show an approximation for the slip velocity that should be justified in the text.
4. In Line 250 you tell that wz is negligible but if I look at : https://doi.org/10.5194/acp-10-2037-2010 i see a vertical updraught velocity of 0.1m/s and a maximum shear of 0.006/s which mean a velocity of 6m/s 1km away from the reference position (let say 0m/s at flight altitude then 6m/s 1km below or above flight altitude). Does it means that you don't consider updraught velocity? Please give more detail on this approximation.
5. Between line 355 and 360 you tell that since T(z,t) and si(z,t) then fV(z,t) and m(z,t). My question would be about m(z,t). In the equation for m there is a term in $\nabla m . \nabla c_N$

suggesting that if initially cN(x,z,0) and m(x,z,0) the right hand side will also be a function of (x,z,t) in the mass equation. Therefore I don't understand why m(z,t) only and therefore I don't understand the justification for cN separable ansatz. I agree with you that it is probably a good separation ansatz without shear but I think it is less simple with shear.

6. I may have miss something for the following point. I don't understand the appearance of a wiener random process in the equation system 12, especially dX and dWt seems undefined.
7. In part 6 you give the time step and the grid discretization to perform sensitivity to these numerical parameter. However, numerical convergence also depends of the discretization scheme and time stepping strategy. Can you give more detail in the discretization scheme?
8. For the comparison with CoCiP, the way you solve the 2D flow with your 1D model with characteristic is unclear to me. Do you use the F function? Of do you follow different line in the initial condition?
9. You compare your results with only CoCiP, however in https://doi.org/10.5194/acp-25-12875-2025 there is CoCiP and APCEMM wich show also very different evolution. It would have been interesting to superpose your multicon results to their curve even without the polydispersity.
10. The equation derived in this paper can be implemented in a CFD software. It would have been interesting to have the 2D CFD to verify certain of your assumption.

Minor comment:

1. In line 43 you give the Bouhafid 2024 reference, this paper does not really simulate the contrail they derive a framework to link the jet phase to the vortex phase but they simulate only the aerodynamic field. They have applied this methodology to contrail simulation in https://doi.org/10.5194/acp-26-1053-2026 .
2. In line 83-85 you mentioned that CoCiP doesn't take into account habit development wich is more or less true since they don't consider the habit in deposition/sublimation equation. However, a habit parametrization as a function of spherical radius is use to determine the terminal velocity and the radiative forcing. I think it has to be mentioned.
3. Line 172: I think it worth to recall that the second hypothesis is justified by the fact that you initialized the contrail after the vortex break up as you have mentioned in introduction.
4. Line 277-278 you give order of magnitude of St an Ga number. Please give order of magnitude for a classical contrail simulation of the different variables to compute them.