Review of High-resolution modelling of early evolution from hydrogen-powered aircraft.

General comment:

The aim of this paper is to present a new parametrization for ice crystal lost during contrail vortex phase. More than just a parametrization, a lot of 3D simulation has been performed in order to have a sensitivity analysis to the apparent ice crystal number emission index and to water emission index in order to include what could be expected from a H2 combustion engines or fuel cells. This has been made by increasing or decreasing the number of ice crystals from the one of a usual kerosene jet engine by 1 and 2 order of magnitude and using the adapt water emission for an H2 engine. I think this approach is totally valid considering the lake of knowledge on H2 contrails.

First, the computational methodology is described in detailed. Then a physical analysis of the results is made with several sensitivity analysis bringing good understanding of the physics. Then the parametrization is presented and compared to their CFD results (from this study and from a past one). The comparison between the parametrization and the CFD results are convincing and the paper shows an improvement compared to the previous parametrization.

On the improvement of the parametrization by itself, I think the paper present good results which can justify by itself the publication. The paper is well written and gives a lot of information. However, I think it would gain in clarity for the reader with less knowledge if some information on the past parametrization are given. I have regroup what, in my opinion could be added in order to improve the paper into the minor comments section of the present document.

My main concerned are about the CFD by themselves that are may be in a too small domain, see mainly the major comment 1,2 and 5. In addition the considered initial condition may also introduced a modification of the survival ice crystals number (major comment 3). The major comment 4 is mainly here to increase clarity but, in my opinion, the information is important to be given. Therefor I will recommend 2 or 3 more computations. 1 with the same initial condition but with a bigger computational domain, made in such a way to leave more space above the contrail at the end or the vortex phase (comment 5) and more space at the left and right, in order to go under 1% of error on the vortex descent velocity (comment 1). Another one with your initial mesh with a gaussian plume surrounding the vortex core (almost no ice crystals in the vortex core). And one with a thinner discretization in order to the if there is no too much numerical dissipation in the vortices could be a good addtion. The comparison of the contrail evolution and the ice crystal survival fraction could be made in order to evaluate these influences. Since, I think, the paper is long enough, this can be added in an annex with a reference to it in the core text. In addition, I will say that the considered case for these simulations should be where the survival ice crystal fraction has an evolution so around a zd between 1 to 2.

On that regard I do not recommend publication on that stage, even if I think that this paper present great quality. I just want to make sure that no surprise arrive when using a better mesh, and with another choice of initialization, and since Comment 1 and 3 could bring opposite effect on the survival ice crystal number, it could be great to see if and at what degree.

Major comments:

1. For the A350 you have a domain width of 384m. Assuming periodic boundary condition, it means that at initial time, you have an infinity (let assume 6, the 2 computed and the images at the left and at the right) vortices lines up on the x axis. Using point like vortex theory, you can compute the vortex descent velocity in the 6 vortices case and compare it to the 2

vortices that you want to compute. If Γ is the circulation of the vortex (520 m²/s), b the separation length of your 2 vortices and L the size of your computational domain (384m), the theoretical descent velocity of the vortex pair is given by $V_t = \frac{\Gamma}{2\pi b}$ wherease, taking into account the 4 other vortices, the descent velocity in your computation should be given by $V_c = \frac{\Gamma}{2\pi b} - \frac{\Gamma}{2\pi\left(L-\frac{b}{2}\right)} + \frac{\Gamma}{2\pi\left(L+\frac{b}{2}\right)} - \frac{\Gamma}{2\pi L} + \frac{\Gamma}{2\pi(L+b)}$. (I don't think so but I may have make some mistakes so please validate or not this formula). Using your values I find Vc=1.7m/s and Vt=1.75m/s, this mean an error of 3%. In https://doi.org/10.5194/acp-15-7369-2015, the authors use a 1km domain and not only 384m (error of 0.5%). The span of their aircraft is kind of the same, however the circulation is a bit higher, still the relative error doesn't depend of the circulation. Since the descent of the vortices may affect the position of your ice crystals, it may also affect the ice crystal loss and therefore your parametrization. Your comparison with this mentioned paper show that your parametrization is always under their results and only two cases of Lewellen 2014 is under and he also has an error in descent velocity under 1%. Could you look at the influence of the domain size in transvers direction by doing a simulation with 1km in the reference stratification case and a configuration with a zd around one or 2 (it seems to be most challenging).

2. Your mesh resolution is 4 to 5 points in the core radius of the vortices (as in https://doi.org/10.5194/acp-15-7369-2015). In https://doi:10.1017/S002211209600849X it would be more 10 points and in https://doi.org/10.1063/1.4934350 it is about 8 meaning about twice as much points by radius core as yours. Have you done (for this paper or in the past) a sensitivity analysis to this parameter?
3. The discs present a constant ice crystal number, however since the ice crystals are created outside the vortex core it will take some time for them to penetrate the vortex core since diffusion is probably the main process which can allow them to penetrate the core due to the closed streamlines. For the same reason the ice crystals initially inside the vortex core are mainly trapped and will descent with the vortices. By this initialization choice, don't you increase artificially the number of ice crystals that remains in the primary wake and then the number of particles that sublimate? (As an example https://doi.org/10.1103/PhysRevFluids.8.114702 , shows (in 2D case) that the position of the initial plume can have an influence on the vertical spreading of the contrail, and putting them inside the vortex core force them to stay inside the vortex core.)
4. You gives different quantities such as $P_{amb}, T_{amb}, RH_i, amb$ and a Brundt Vaisala frequency. I guess the given value is at flight altitude whereas the pressure field follow the hydrostatic equation, and T in the atmosphere follow a constant Brundt Vaisala frequency? If so I guess it worth to be written for the non-specialist readers. Moreover, the choice of the relative humidity evolution with altitude is not trivial, since the mass fraction of water can fluctuate in order to impose a constant relative humidity with altitude, or one can keep the mass fraction of water vapor constant, or some other choice can be made… Please indicate what has been your choice in that matter.
5. In fig 2 the ice crystals are going above the flight altitude. In the picture, it seems to go dangerously close to the boundary of the domain. Are the ice crystals stopped in their movement due to the presence of the boundary condition?

Minor comments:

1. In lines 102-103 you say that in the LCM solver you use numerical particles which represent a certain number of physical particles. However I haven't found in the text how many numerical particles are used and how many physical particles are represented by them. Since you make a sensitivity analysis on the number of particles, is it the number of numerical particles which is reduced or their weigh (number of physical particles inside a numerical one). Moreover can you provide a reference on the number of numerical particles to have a good contrail representation? (may be this one: https://doi:10.5194/gmd-7-695-2014 ?)
2. Line 105 you explain that some routine of LCM has been switched of such as aggregation and radiation. In 450 you add that sedimentation was also off for vortex phase. I think it worth noticing it also in line 105. Can you also provide either some reference or some order of magnitude in order to justify that you neglect tem?
3. In the paragraph from line 114 up to 123, you gives detailed of your mesh and boundary condition. You say that you use rigid condition for the vertical direction. At first glanced I have not really understood if you meant the xmin xmax (vertical normal) boundary or the zmin, zmax boundary (vertical plans). Please could you rewrite the lines 121 to 123 such that it will be blatant.
4. You provide a lot of information about the initialization such as the plume radius which is represented by two discs. I have not find the position of the center of this discs. I imagine that it is center on the vortex center but please add the information in the text.

5. In the z direction, you have 600m which seems to go from -500m to 100m (on figure 2). Please put this information into the text.
6. In your analysis you decrease the Brundt Vaisala to 0.005s-1. In https://doi.org/10.5194/gmd-5-543-2012 gives a typical range between 0.01 to 0.03. Is your parametrization has been tested for higher stratification levels?
7. In your part 3.3 and in the annex you talk about the paper U2016 and tell this formula replace this formula from U2016 … I think the paper will gain in clarity if you write the formula from U2016
8. In formula 6 and 7 you have T_{CA}, e_s, s_i which seems undefined in the text (unless I miss them). I guess they are defined in U2016, but I think you should defined them here too.