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Comment on acp-2021-497

Anonymous Referee #2

Referee comment on "Contrail formation within cirrus: ICON-LEM simulations of the impact of cirrus cloud properties on contrail formation" by Pooja Verma and Ulrike Burkhardt, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-497-RC2, 2021

General Comments

The paper addresses the question of contrail formation within cirrus clouds. Contrails constitute an important part of aviation impact on the atmosphere but the evaluation of their radiative forcing remains difficult to determine because of limited scientific understanding and complexity of the different processes to take into account. This paper is focused on the formation of "real life"-contrails, that do not only form in clear sky but also within existing clouds and we expect to know whether the effects are negligible or not. Therefore the work provides valuable effort in order to better understand the processes and evaluate the effects. It uses a model including a cloud microphysical model and study the influence of contrails/cirrus clouds on each one for two synoptic situations. The paper focusses on the potential effects but is not meant to evaluate since it would require proper integration in a climate model. As a matter of fact, it is difficult to draw your own conclusions after reading whether preexisting cirrus clouds are important for contrail impact since the effects depend on the synoptic situation, the properties of cirrus clouds, the soot EI, the way the processes are treated etc. But it is important to go forward with such a work. It is perfectly in the scope of ACP.

The paper is a bit long, some details are repeated several times, and the paper is not very easy to read through, especially because within a paragraph you may have to refer to a lot of figures at different locations (see for example section 3.3.1 where figures 4b, 7a, 2, 3 are called in the first 10 lines).

A list of acronyms could also be useful to the reader since a lot of them are used in the text, sometimes unnecessarily (RBF or CWP are only mentioned once for instance).

Specific comments/remarks

- The soot emission index that was used in the simulation, 2.5×10^{15} particles per kg fuel was fairly high. It is indeed mentioned that this is for the soot-rich regime. Recent works on the non-volatile particle emission certification process have also emphasized the importance of particles loss in the measurement system, and that could lead to underestimates of soot emissions, especially from previous studies and in flight. Surely the choice of this index has some consequences on the results and the sensitivity study of the survival fraction to the soot emission index that has been performed in section 3.3.1 should have been more emphasized (or advertised at the beginning of the paper as the reader may think that only one soot emission case has been treated).
- Regarding the effect of ice crystals sucked into the engine, Gierens (2012) indicates that the change in the water vapour emission index is very small, of the order of its variability regarding the fuel's composition. In this work, the effect depends on the air-to-fuel ratio and the cirrus cloud ice water content (eq. 2). It would be nice to have at this point some example values so that the reader can figure out how much water vapour can be added to the plume. Besides, eq. 2 give an upper limit to the added H2O since it may be affected while going through the engine, including the hot combustion chamber.
- Please add some justifications on the choice of a "rough(ly) estimate" in equation 5: no Kelvin effect (are particles large enough at this point to exclude it?), spherical particles (correct for young contrails but not for cirrus clouds).
- Following point (3) in the summary, from line 627, the role of the temperature change on the saturation vapour pressure and in turn on the relative humidity is described. The sublimation of cirrus ice crystals and contrail crystals releases water vapour so that the system tries to reach a vapour/ice equilibrium. The change in the survival rate due to cirrus ice crystal sublimation should be considered a maximum since (if) cirrus crystals are treated the same way as contrail ice crystals (spheres, no influence of size). The competition between contrail and cirrus cloud particles during sublimation seems to be the important part and should be treated with the maximum accuracy.
- In the conclusion section line 706, the authors conclude that "the pre-existing cirrus can lead to changes in the contrail formation criterion and, therefore, can lead to contrail formation when otherwise none would have formed". This is a strong statement for someone who would only read the conclusions, regarding to the text for instance line 607 "In large parts of the cirrus cloud field the presence of cirrus does not impact the contrail formation criterion and contrail ice nucleation significantly".
- Line 708-710 seems not so clear to me. "That means that the pre-existing cirrus ice crystals can lead to contrail formation in cases when otherwise the passage of an airplane would have dissolved the cirrus". Does this refer to taking into account or not the effect of pre-existing cirrus in the contrail formation process?
- Sometimes "forgotten" in plume microphysical model studies for convenience or for the sake of simplification, ambient aerosols and especially ice cirrus clouds could be taken into account for a more detailed sensitivity analysis. It could be one more recommendation of the conclusion.

Technical corrections

- Line 34 "since in IPCC style double CO2…"
- Line 146 "vertically consistently"
- Figure 2 (a), (b), (c), typo in the vertical axis legend "Tamperature"
- Figure 4, in the caption unit hPA instead of hPa in (~250 hPA)