**Reviewer #2 Comments (italicized), and responses (blue):**

*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 wo rk 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 El, 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).*

→Thank you for the review of our paper. We changed our paper according to your suggestions.

In the process of revising the paper according to both reviewer's comments we included a number of additional processes that have implications for the contrail ice nucleation and for the contrail ice crystal survival in the vortex phase. In particular, we included the impact of cirrus ice crystals that get mixed into the aircraft plume in the first second of the plume's lifetime before contrail ice nucleation section 2.2.2. Furthermore, we include now the diffusional growth phase of the plume ice water content after contrail ice nucleation and before vortex descent in our estimate of the impact of cirrus ice crystals on the contrail ice crystal loss within the vortex phase (section 2.2.4). For this estimate we use now the full diffusional growth equation (Appendix A). Finally, we found an error in our implementation of the parameterization of ice crystal loss in the vortex phase that we corrected. All together we find now that the impact of cirrus ice crystals on contrail ice nucleation is larger than estimated before and that the impact of cirrus ice on the vortex phase loss is most of the time negligible except for the parts of the natural cirrus that include many small ice crystals and those where contrail ice nucleation is very low and the cirrus ice crystal radius relatively small (section 3.3).

Accordingly, large parts of the paper were changed. In the context of the revision the text was shortened in many places in order to avoid repetition and we improved our use of abbreviations.

*Specific comments/remarks*

*The soot emission index that was used in the simulation, $2.5xl0^{15}$ 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).*

→ The soot number emission index of $2*10^{15}$ kg-fuel$^{-1}$ was chosen according to Bräuer et al. (2021) but we agree that this value is very uncertain and highly variable. We mention this uncertainty now in the conclusions. When choosing a lower soot number emission index, changes in ice nucleation due to the impact of cirrus ice crystals would remain the same for the lower soot number emission index except that the new $AEI_i$ would be limited to the lower soot number emission index. In the analysis of the impact of cirrus ice crystals on the survival of contrail ice crystals the soot number emission index and the number of nucleated contrail ice crystals is very important so that we have analyzed the sensitivity of our results to a range of soot number emission indices. We mention this sensitivity analysis now earlier in the paper e.g. in the last sentence of the introduction and in the last sentence of section 2.3.

*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 airto-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 H20 since it may be affected while going through the engine, including the hot combustion chamber.*

→In Gierens (2012), as well as in our paper, the change in the water vapor 'emission index' is dependent on the air to fuel ratio and the cirrus ice water content. We choose the same air to fuel ratio and the same water vapor emission index (1.25 kg per kg-fuel) as Gierens (2012). The cirrus cloud properties that we use are displayed in our figure 2. Our results are consistent with Gierens (2012) regarding the impact of the sublimation of cirrus ice crystals in the engine. Our figure 3 (dashed lines) shows that the sublimation of cirrus ice crystals within the engine amounts to maximally a few percent of the 'aviation induced change in the plume water vapor' that is the sum of the water emission due to fuel combustion and the sublimation of and deposition on cirrus ice crystals. For our thin cirrus we get a maximum change in water vapor 'emission' of about 0.2% (blue lines) and for our frontal cirrus day a change of about 4% (red lines) at a probability of about $10^{-2}$, which agrees with the 'order of percent' result of Gierens. The sublimation and deposition on cirrus ice crystals that were mixed into the plume after emission can occasionally be larger than the sublimation of cirrus ice crystals within the engine but it can be also negative. Gierens did not consider the impact of cirrus ice crystals that are mixed into the plume on the water vapor 'emission index'. Therefore, the maximum increase in the water vapor 'emission' due to the sublimation /deposition of cirrus ice crystals (figure 3, solid lines) is larger than the estimate of Gierens and, when assuming an IWC that would be representative of a relatively thick in-situ cirrus, can also be larger than the variation in the water vapor emission index given by IPCC. these results are described in section 3.2.

We have added a sentence in 2.2.2 shortly after equation 2 saying that the impact of cirrus ice crystals on the aviation induced water vapor content of the plume is discussed in section 3.2. We have also added a sentence in section 3.2 saying that we use the cloud properties as displayed in figure 2 for our calculations. Furthermore, we mention in the figure caption of figure 3 that the water vapor emission index is 1.25 and that the cloud properties are displayed in figure 2. In the summary and comparison to literature we compare our results to Gierens as explained above.

To our knowledge the $H_2O$ content of the air is not changed when going through the engine. Water does not accumulate in the engine.

*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).*

→We use now the full diffusional growth equation (including the Kelvin effect) for calculating the diffusional growth of mixed-in cirrus ice crystals before nucleation and the growth of contrail and cirrus ice crystals after nucleation and when estimating the cirrus and contrail ice sublimation during vortex descent. Please see our new Appendix A for details.

We assume spherical particles as we have no information on the particle shape or the mix of particle shapes of the cirrus ice crystals from our model. When estimating the impact of cirrus ice crystals that are mixed into the plume on ice nucleation and on the survival of contrail ice crystals we find that it is mainly cirrus containing very many small ice crystals that have the largest impact. This cirrus is connected with the frontal system crossing Germany on that day and has properties representative of a medium thick in-situ formed cirrus and partly a liquid origin cirrus (Krämer et al., 2020). Those ice crystals are likely close to spherical so that the assumption is likely a good one. Nevertheless, when cirrus ice crystals are larger our assumption of spherical particles may often underestimate the real sublimation and deposition on the mixed in cirrus ice crystals. Nevertheless, the changes in contrail ice nucleation and survival in the vortex phase are so much lower when cirrus ice crystals are large that we do not expect our results to change significantly even if we would have more information on the shape of cirrus ice crystals.

We have added a few sentences in the new appendix just after the introduction of the diffusional growth equation Line 819 to 824. We have also mentioned the dependency of our results on the shape as one of the uncertainties in our analysis (conclusion, line 769).

*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.*

→As mentioned in the above points we have changed our parameterization and use now the full diffusional growth equation. Please see our new Appendix A for details.

As argued above, the impact of cirrus ice crystals on contrail ice nucleation and on the survival of contrail ice crystals during vortex descent is largest when the cirrus consists of many relatively small ice crystals. In this situation the assumption of spheres may not be that bad. If cirrus ice crystals deviate significantly from spheres then the deposition on the ice crystals would be larger as well as the sublimation would be faster during vortex descent. It is not clear to us if this would have mostly a positive or negative impact on contrail ice crystal survival. Since it is mainly large ice crystals that have shapes that are far from spherical and since large ice crystals usually lead to a very slight change in the survival fraction, we believe that the impact of assuming different shapes for large ice crystals would not be very large. But we agree that there is an uncertainty connected with our assumption that we can treat ice crystals as spheres. As mentioned in the answer to the previous point, we have added in the appendix a few sentences about the problem treating the impact of particle habit. We also added in the conclusions that the shape of ice crystals and its influence on the diffusional growth is one of the uncertainties that should be in future tested with LES modelling.

*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 statement 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".*

→We have rewritten the conclusions and the statements should sound now less contradictory. In large parts of the cirrus clouds, the cirrus ice crystals have little impact on contrail formation except when IWC and ice crystal number concentration is high or if contrail formation is happening very close to the contrail formation threshold.

*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?*

→Yes, when we take them into account during contrail formation. We have reformulated the sentence and hope it is now less ambiguous. 'The presence of pre-existing cirrus ice crystals leads often to an increase in the contrail formation threshold temperature. Therefore, contrails may form and cause locally relatively high ice crystal number concentrations when, in the absence of the pre-existing ice crystals, no contrails would form so that only the sublimation of cirrus ice crystal in the engine would lead to a change in the ice crystal concentrations.'

*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.*

→We do analyse the impact of cirrus clouds on the contrail ice nucleation and the survival of ice crystals in the vortex phase. We do not consider the full impact of ambient aerosols on contrail ice nucleation. While prescribing soot-rich emissions the ambient aerosols do not play an important role in determining ice nucleation unless their properties are significantly different from the soot properties which could lead to larger errors when using the contrail ice nucleation parameterization of Kärcher et al (2015). We added in the conclusions that variability in aerosol properties together with inhomogeneities have an impact on ice nucleation and constitute an uncertainty of the parameterization of contrail ice nucleation and therefore of our approach.

Technical corrections

*Line 34 "since in IPCC style double C02..."*

It is not clear to us what this comment refers to.

*Line 146 "vertically consistently"*

done

*Figure 2 (a), (b), (c), typo in the vertical axis legend "Tamperature"*

done

*Figure 4, in the caption unit hPA instead of hPa in (~250 hPA)*

done