Answers to editor

Dear Martina,

We have now revised our paper again.

After carefully studying the report, my decision is now to accept the manuscript with minor revisions (review by editor). In the revised version, I would ask you to take into account the recommendations of the referee, i.e. to tone down the interpretation of the results (see comments Category (A)) and to better discuss the inaccuracies of the used parameterizations (see comments Category (B)).

In particular we have toned down our interpretation in the abstract and summary (as requested by reviewer 1 in Category A). We say now clearly that the effects are often negligible and describe the atmospheric conditions in which they are not.

We have also now added an additional estimate of the probability of significant changes in contrail formation when using a different IWC threshold for selecting the cirrus that we include in the analysis. This should give the reader a better quantitative idea of how uncommon large changes are. A better quantitative measure for the frequency of large changes should make the exact wording less of an issue.

Changes made in connection with Category B were mainly made in section 2.2.4. Unfortunately, we still believe that the reviewer did not understand how we estimated the impact of cirrus ice crystals on the vortex phase loss since he still insists that we did not consider the competition between cirrus and contrail ice crystals in the descending vortices. In order to improve the description and to make it easier to understand we rewrote section 2.2.4 and iterated it with the LES modeling group at our institute that specializes in modeling the contrail ice crystal loss in the vortex phase. We hope very much that the section is now generally easier to understand.

Nevertheless, we have also improved our description of the assumptions and uncertainty of our parameterization in the conclusions adding 'Furthermore, we assume a constant plume subsaturation within the descending vortex in order to calculate the competition between cirrus and contrail ice crystals and calculate contrail ice crystal survival after adjusting the plume water vapor content consistent with the impact of the cirrus ice crystals on plume water vapor. '

Please provide a revised version of the paper together with a manuscript with changes tracked. Also, it would be good to briefly answer to the major points raised by the referee.

We have answered all of the reviewer's comments. But some of the comments seemed to relate more to our answers to his comments and less to the paper or seemed to agree with parts of our argument. Those points we partly did not answer.

Answers to comments of Reviewer 1

The authors have chosen not to follow most of the suggestions in my review of the prior revision of this manuscript (an exception is in the substantial pruning of section 3.3, which has improved the manuscript). The authors' attempts to rebut my concerns, both in additions to the manuscript and in replies to my review comments, I found unconvincing, and sometimes plainly incorrect. Accordingly, the bottom-line conclusion from my prior review remains unchanged: I think this paper needs revisions if it is to be published. While I consider these in the category of "major revisions" because they involve the primary conclusions, I am not requiring changes in the actual analysis or figures, just in making the text consistent with the results presented and the level of approximations represented in the modeling.

The paper's stated aim is to study the impact of pre-existing cirrus on contrail formation, in particular its effect on the number of contrail crystals that nucleate and their survival fraction in the vortex regime. As noted in detail in my prior reviews, my concerns about the manuscript can be grouped into two broad categories: (A) that the presentation, particularly in the abstract and conclusions, gives an inflated impression of the potential importance of these effects relative to what the results presented actually show; and (B) that the accuracy of the parameterizations employed for both nucleation and crystal loss are much rougher and more uncertain than the presentation implies. I will consider these categories in turn below, but confine my comments to changes made in the current manuscript version and authors' comments about them. I won't reiterate all the points in my last review -- not because I think they have been adequately addressed, but because I have no changes to make in them.

Category (A):

In the abstract the authors highlight the largest differences they find (e.g., "...which can be as large as 2K", "...contrail ice nucleation rates can be significantly increased...") without highlighting that these occur only in the rare tails of their simulation cases (less than a fraction of a percent). Nor is the frequency of occurrence accurately presented in the summary or conclusion sections. For example lines 663-664 ("We conclude that the sublimation of cirrus ice crystals in the engine and the impact of cirrus ice crystals mixed into the plume can have a significant impact on contrail formation.") are given with no indication of the rare occurrence. Or in lines 653-655, where the authors concede that the effects are not significant "in large parts of the cirrus cloud field", but their own figures indicate "large parts" is really "almost everywhere". I think finding these "cirrus correction effects" at a significant level (as measured by changes in crystal number) only rarely is strong evidence for the unlikeliness of these effects having a significant impact on the climatic effect of contrails. The authors in their comments and additions try to argue otherwise in a few ways, none of which I find convincing.

Text passages in the abstract and in former lines 663-664 and lines 653-655 were changed.

(A1) First, in their comments and the added lines 555-558, the authors try to argue that this is not the case because the rarity seen in their results is only an artifact of "the minimum IWC that we use as a cloud mask". This is a spurious argument. It is true that their choice for defining their probability distributions is arbitrary, but it is adequate for a rough picture. If they had normalized the probabilities with a more physically relevant choice such as fraction of all "conditions producing contrails" or "conditions producing contrails above some significance level" then the dependence on the choice of "minimum cirrus IWC" would drop out, but the fraction of cases with significant "cirrus correction effects" would still be very small (particularly since in assessing these effects for contrail-climate-impact purposes one should consider also all the contrails forming in clear skies). I suggested a possibly better way to present the relative importance of these effects in my last review by modifying the authors' then fig.11, but the authors chose simply to remove that figure instead.

When normalizing with the fraction of conditions producing (significant) contrails we do not eliminate the dependence on the choice of minimum cirrus IWC. At low cirrus IWC, ice supersaturations can be high and contrail formation leads to large numbers of ice crystals similar to contrail formation in cloud free ice supersaturated areas. We now analyze the dependency of our results on the cirrus IWC. We have recalculated the probability of larger changes when considering only cirrus with ice water content higher than 10^{-5} kgm^{-3} in order to quantify this dependency of our results on the cirrus IWC. We have added accordingly a few sentences in section 3.2.

(A2) Second, in their comments and added lines 703-714 they try to argue that this is not the case because "...the relative change [in crystal number] in itself is not an indicator for the climate impact of the cirrus induced changes. Instead the climate impact depends crucially on the background optical depth of the cirrus". It is certainly true that the radiative impact of a contrail depends on multiple factors beyond crystal number alone, including the optical depth of the contrail and of the surrounding cirrus. But the effects the authors are considering here act most directly through changes in contrail crystal number. If these changes are small then changes to the contrail optical depth from these effects may naturally be expected to be small; and if significant changes in crystal number from these effects occur only rarely among significant contrails then the expectation is that significant changes in contrail optical depth from these effects will occur only rarely. The two radiation papers the authors have added citations to do not contradict these expectations in any way that I can see. Further, these and other work that I am aware of are fully consistent with the expectation I noted in my review: that the magnitude of the radiative forcing from a contrail will in general be diminished with increasing optical depth of the surrounding cirrus. That the "cirrus effects" considered in the paper can, for some specially balanced cases, happen to flip the sign of the net radiative forcing, implies no extra significance to the potential for these effects to alter the radiative impact of contrails collectively. And the authors' comment beginning "Our disagreement is caused by the fact that the reviewer assumes that the radiative forcing due to contrail perturbations is necessarily positive...." is simply false, both in the nature of our disagreement and in my assumptions about radiative forcing. Having performed numerous LES that include computations of the radiative transfer (and the feedback of radiative heating/cooling back on the contrail dynamics itself) I am well aware of the variabilities and complexities involved (see e.g., Lewellen 2014, J. Atmos. Sci. 71, 4420–4438).

We discuss the physics relevant for estimating the climate impact in the conclusions. We say that our simulations do not allow the estimation of the climate impact as it is not the topic of our paper anyway. Therefore, we only ‘speculate’ about the possible climate impact in the conclusions.

Nevertheless, we appear to agree that the radiative impact of changes in cirrus properties due to contrail formation depends on the contrail induced cirrus changes and on the background cirrus properties. This means that the climate impact of the contrail induced cirrus changes depends directly on the change in the cirrus properties and on the undisturbed cirrus properties.

The remaining disagreement appears to be that the reviewer insists that from the low frequency of large changes in contrail formation caused by preexisting cirrus it can be concluded that they do not have an impact. For this argument please see our answer to A3.

(A3) Third, they argue in comments that this is not the case "...because the places in which the impact of cirrus on contrail formation is largest are the places in which contrails can be expected to have a large impact". Lines supporting this have been added to the paper, e.g., lines 703-705 ("The change in cirrus ice crystal numbers due to contrail formation may ... have a significant influence on cirrus optical depth, radiative fluxes and cirrus life times."). That contrails can significantly impact cirrus occurrence and properties is well known. But, while this is obviously a necessary condition for "cirrus effects on contrails" to be important, it is clearly not a sufficient one. It is entirely consistent with the possibility that in the bulk of cases where contrails significantly alter existing cirrus (or create new cirrus) that the "cirrus effects on contrails" are negligible. And it is that possibility which the authors' presented results seem to me to support. If the authors continue to doubt this, I suggest they consider the following exercise: (1) estimate a cirrus IWC level required for large "cirrus effects on contrails" from their results (it seems to me about ~0.1 gm^-3); (2) estimate what fraction of all significant contrails form in conditions with cirrus IWC above that threshold.

Thank you. We did indeed misformulate this sentence. It reads now 'The impact of cirrus ice crystals on contrail formation may, therefore, be expected to have a significant influence on cirrus optical depth, radiative fluxes and cirrus life times. (Lines 723 to 725)

In the text in the conclusions we draw on our experience of the atmospheric conditions that support contrails that have a large climate impact. Our argument is that contrails that form in cloud free air have a particularly large effect when the life times are long and that the life times are long in situations such as frontal passages and their associated conveyor belts. In exactly those situations the impact of cirrus properties on contrail formation is large which may point at those impacts having a much larger radiative effect than would be expected when considering only the frequency of occurrence.

(A4) In places the authors try to be non-committal about the question whether "the effects that we are studying do not have an impact on contrail radiative forcing" claiming "our results do not support this conclusion, nor do they support the opposite". If the authors believe this is so, it should be stated clearly in the abstract, since this is likely to be the question of most interest to potential readers. But in fact, I think it is not so: the authors results as presented actually do support the premise of negligible impact. That this support is not entirely conclusive is, I think, due mainly to the issues in category (B).

We do not believe that it is common practice to make statements about the topics that are not studied in the abstract.

Category (B):

In simulating contrail nucleation or crystal loss, the authors neither solve the underlying physical equations involved (though such treatments exist in the literature), nor provide any estimates of the error levels in their results that may arise from the highly simplified parameterizations they use instead. The works from which they obtain their parameterizations (before modification), do employ the underlying physical equations in their development, but not for conditions that prove most important here (e.g., in the presence of cirrus with extremely high IWC). Nor do I think the original developers of these parameterizations would claim that they are (even in the absence of cirrus) accurate at the levels of precision results are quoted to throughout this paper. Such use is arguably sufficient if the authors' goal is just to speculate qualitatively about the direction of some sensitivities, or to argue that an effect is likely negligible (so that even a factor of ten error, say, would not change the basic conclusion). But that is not how the authors are presenting their results.

In defense of their parameterizations they offer a mix of wishful but untested speculations (e.g., "...the sensitivity of contrail ice nucleation on cirrus ice crystals is valid even if the parameterization of Kärcher et al. (2015) should generally overestimate contrail ice nucleation."; "Most of the processes (single activation time and plume inhomogeneity) may change the estimate of the overall number of contrail ice crystals but may change little in our estimate of the sensitivity of contrail ice nucleation to cirrus ice crystal sublimation/deposition within the plume."; "In summary, plume inhomogeneities and consecutive nucleation may affect the number of contrail ice crystals nucleating but may not have a significant impact on the sensitivity of contrail ice nucleation to sublimation and deposition on cirrus ice crystals", etc.) and some misleading and/or incorrect statements. Concentrating on statements in the manuscript itself, the latter include:

As Lewellen (2020) shows the parameterization of Kärcher et al. (2015) may overestimate ice nucleation due to the lack of plume inhomogeneities and the approximation that all contrail ice crystals nucleate at the same time. In our study we calculate the increase in the plume water vapor content due to the sublimation of cirrus ice water and its impact on ice nucleation. The associated increase in relative humidity stays nearly always within the range of values found within aircraft plumes in cloud free air. This means that our application of the parameterization probes mostly the same parameter space for which the parameterization was set up. Only in very few of the situations that lead to large changes in ice nucleation would the plume experience larger relative humidity than found in plumes forming in clear air ice supersaturated areas (Those large plume supersaturations occur mainly on the level around 10.5km). This means that entrained ambient aerosols would on average have a similar impact when contrails form within cirrus than when they form in cloud free air due to the fact that fewer aerosols are entrained and relative humidity is mostly similar to the plumes developing in clear air.

On the nucleation treatment:

(B1) lines 229-230: The authors are misquoting the results of Lewellen (2020) (L20 hereafter). One of the conclusions of L20 was that box-model computations of contrail nucleation in some regimes can significantly over-estimate crystal production relative to results from LES (which include much more of the correct physics). But nowhere in L20 was it stated that that problem occurred only for aerosol emissions above 10^16 (as the authors have stated in trying to argue that their simulations here are free from such problems). No fixed threshold was cited in L20 because the threshold would vary with location in the multi-dimensional parameter space. Two general regimes were identified in L20 where

box model results seem to reliably match LES results reasonably well, but the cases of most interest to the authors here are not in either category. The first is where nucleation is predominantly on ambient aerosol (which is not what the authors are considering here). The second is where essentially all the relevant exhaust aerosol is nucleated (the "LND" regime of L20). But for the small fraction of cases the authors are highlighting, where the presence of cirrus significantly increases the number of exhaust aerosol nucleated, this will not be the case: at the least, the comparison simulation not including the "cirrus effects" must necessarily be nucleating a significantly reduced fraction of the exhaust aerosol in order for the nucleation rate to significantly increase in the simulation with the "cirrus effects" included. In short, the simulations the authors are highlighting are precisely ones where L20 finds the box-model approach suspect. Moreover, the Kärcher et al. (2015) parameterization uses approximations (including the single activation time) above and beyond the box-model approach itself. So even restricting to where the box model reaches a target level of accuracy does not ensure that level of accuracy for the parameterization the authors are employing.

We have modified the text saying that 'This impact is large for large aerosol emissions, e.g. for EIs = 10^{16} kg-fuel^{-1} and higher (when using parameters as in fig. 5 of Lewellen, 2020). Added lines 230 to 231.

Our result that the increase in plume water vapor content leads to an increase in ice crystal nucleation should not depend on the approach we are using. Nevertheless, we would welcome additional studies on this topic using different methods.

(B2) The argument alluded to in lines 230-234 and in comments (e.g., "Within existing cirrus, we can exclude entrainment of aerosols into the plume that preferentially form ice crystals.", etc.) and which apparently is used as the basis for the added lines 727-728 in the conclusions ("But, within cirrus, ambient aerosols can be expected to have a small impact on contrail formation within cirrus") does not hold. The argument seems to be that the relevant ambient aerosol would be either destroyed within the engines or already bound in cirrus ice crystals. But the elevated supersaturations encountered in the exhaust plume are such that they can easily nucleate ambient aerosol that in normal circumstances would not be nucleated in natural cirrus. And the vast majority of these aerosol are mixed into the aging plume without ever passing through the engines.

We write that 'within existing cirrus, aerosols are not entrained that form **preferentially** ice crystals. The plume supersaturations that we see when contrails form within cirrus are similar to those in plumes forming in clear air. Only in very few of the cases in which changes in ice nucleation are large (in particular higher up in the atmosphere) does the plume experience relative humidity that is larger than the relative humidity in plumes forming in cloud free air. With lower ambient aerosol concentrations that get mixed into the plume and comparable supersaturations the problem of ambient aerosols should not be bigger in our simulations than in simulations of contrail formation within clear air. We have added lines 231 to 238 and 748 to 750.

(B3) Regarding increased uncertainties "near-threshold": in their comments the authors try to dismiss these concerns by claiming that the example "near-threshold" LES cases included in L20 are within a "few tenths of a degree" of the threshold, while some of their cases in fig.6 with significant Delta_n/n are further away (even ~2-3 degrees below threshold and therefore not "near enough" to have elevated uncertainty). But the uncertainties in crystal production near the contrail threshold are closely related to the steep fall-off in what the authors refer to as AEI_i; as can be seen from their fig.1, this extends much further than a "few tenths of a degree" below threshold. The "near-

threshold" LES cases in L20 are actually 0.2 and 1.3 degrees K below threshold (depending on RH_i). The "near-threshold" uncertainty scatter illustrated in the plots there is indeed much larger for the 0.2 cases (exceeding an order of magnitude) but still large for the 1.3 K cases (tens of percent). Further, the uncertainty scatter illustrated in L20 is only that from variations in turbulence realizations. Additional sources of uncertainty, such as the deviations of the box-model results relative to LES, also grow as the threshold is approached.

Nevertheless, we find changes more than 2 or 3K below the formation threshold that will not be strongly affected by those uncertainties. We changed the text in the conclusions slightly and say now 'Furthermore, contrail formation close to the formation threshold (within about 1K) is connected with a large uncertainty since details in the plume development may have a large impact, leading to varying contrail ice crystal numbers resulting from slightly different plume evolutions (Lewellen, 2020)'

On the crystal loss parameterization:

(B4) The reviewers suggest in their comments that I have misunderstood their approach and so have expanded the discussion of it in section 2.2.4. On the contrary, I followed what they were doing the first time and the problem remains: they are not including what I would expect to be the largest potential effect of the ambient cirrus on the contrail crystal survival rate. The Kelvin-effect-dependent scavenging of moisture by large crystals from small ones is a primary component of the crystal loss in the vortex regime. It depends heavily on the size spread of crystals involved. Thine parameterization of Unterstrasser (2016) is empirically based on LES studies which include the Kelvin effect but not in the presence of ambient cirrus crystals (which are generally much larger than the contrail crystals at this stage and so can be more effective scavengers). This is true for all values of water emission in Unterstrasser's LES sets; no adjustment to the inputs into the parameterization will account for the omitted effects (including the authors' adjustment to airplane water emissions).

We have rewritten again parts of section 2.2.4 and made hopefully even clearer that the 'adjustment to the water vapor emission' is calculated by simulating/estimating (using the diffusional growth equation) the competition of deposition / sublimation of cirrus and contrail ice crystals in the time after nucleation including the time of vortex descend. That means that we do include the impact of ice crystal sizes on deposition/sublimation considering the different sizes of cirrus and contrail ice crystals before and during vortex descend. We roughly estimate the contrail ice crystal loss after adjusting the water vapor emissions by the water that is deposited on / sublimated from cirrus ice crystals before and after vortex descend. Added lines 351 to 359.

The approximation that we make for the estimate of the sublimation of cirrus and contrail ice crystals during vortex descend is to prescribe a constant ice subsaturation instead of simulating the temporal evolution. We choose a value for the fixed ice subsaturation motivated by the simulations of Naiman et al. (2011). We now mention this simplification in the list of uncertainties of our simulations in the conclusions. The sensitivity simulation, extending the phase before vortex descent, extends the time that cirrus ice crystals can grow at the cost of contrail ice crystals and, therefore, test the sensitivity to the constant ice subsaturation approximation.

(B5) The added clause in line 301 ("...while accounting for differences in ice crystal growth due to the Kelvin effect"), while technically true when referring to Unterstrasser (2016)'s original work, is misleading in implying that it extends to the authors' use of that parameterization here (it does not).

This sentence is in the section in which we describe the Unterstrasser (2016) scheme without any of our modifications. A misunderstanding should therefore not be possible.

(B6) The authors' estimate of crystal loss is not only "very rough" (line 356) but also one-sided. It includes the bulk of the cirrus effects that might aide in contrail crystal survival, but omits the one that could potentially produce the greatest additional crystal loss. Some discussion of this loss mechanism is present in the paper (added in the first revision), but the mechanism itself is not included in the authors' simulations. Thus while the authors can rightly conclude that their results support that the potential for ambient cirrus to increase contrail crystal survival in the vortex regime is negligible, more general conclusions (e.g., lines 28-29, 699-700) ruling out the potential for enhancing crystal losses are premature.

The mechanism, the competition of deposition/sublimation dependent on ice crystal sizes is included. See answer to B4 and the description presented in section 2.2.4.

(B7) lines 357-359, 601-604:

In my opinion the sensitivity study added is of negligible utility. It seems random to test a component of a parameterization of minimal importance while making no attempts to gauge uncertainties in much more important components of the parameterization.

Since the reviewer is concerned that we do not include the competition between cirrus and contrail ice crystals, we test here the sensitivity to increasing the time period during which relative humidity is slightly supersaturated and cirrus ice crystals can grow at the cost of contrail ice crystals. Prolonging this time period leads often to more water getting deposited on cirrus ice crystals while the contrail ice crystals either grow more slowly or even decrease in size. This leads to a larger loss in contrail ice crystals.