Answers to reviewer I:

We appreciate the comments of the reviewer. In the following our answers to the reviewer's concerns are marked in blue while the reviewer comments are in black.

This revised manuscript is substantially improved in several respects: it is now clearer from the text what is being done; some of the parameterizations are improved somewhat; and some important shortcomings/uncertainties are now acknowledged. Even with these changes, however, the primary concern from my original review largely remains: the approximations and analysis being used are not at the level required to justify many of the findings that are being emphasized. I would strongly encourage the authors to focus on results that their analysis defensibly supports. The manuscript could also benefit from a tightening up of the prose throughout.

The aim of this research -- addressing the impact of pre-existing cirrus on contrail formation and properties -- is a useful one. Further, the approach employed and results presented are, I think, sufficient to draw a valuable conclusion toward that aim: that the presence of natural cirrus is unlikely to significantly increase the climate effects of contrails. To me this is the natural conclusion to be drawn from e.g., fig.3, which shows enhancement of water vapor of at most 20%,and at that level for only a tiny fraction of the cirrus samples (1 in 10000); or from figs 5,6 that show for large contrail ice numbers (i.e., where contrails have a potential for significant impacts) that Delta_n/n is quite small almost everywhere in the sampling of cirrus conditions.

Further, even the modest enhancements of contrails by cirrus that are seen in the results may be reasonably interpreted as upper bounds, since the shortcomings in the parameterizations and analysis employed (e.g., those listed in my prior review) generally tend toward overestimation of contrail ice crystal number.

This general result is admittedly one-sided: because the radiative effects of the cirrus have not been considered in this work, the results do not rule out the possibility (I would suggest likelihood) that in some regimes the presence of the cirrus might significantly reduce the potential climate impact of contrails.

Unfortunately, the authors seem reluctant to simply focus on what is essentially a reinforcement of the conclusions of Gierens (2012). They do make statements in support of these conclusions in several places, but elsewhere include contradictory statements stressing smaller effects in the tails of their distributions to try to argue for a significant strengthening effect of cirrus on contrails (e.g., in lines 22-23, 697-700, 702-704, 706-707, 713-714, 755-757). If the authors want to argue for the validity of results at the few percent level (or, in many cases in the paper, fractions of a percent), they need to demonstrate that their modeling uncertainties in the regimes in question are actually less than this level. Given the parameterizations used and the regimes involved (heavy cirrus and/or near-threshold conditions) their current model/methods would fail this test. The problem is exacerbated by trying to argue the general significance of results that are non-negligible in only a tiny fraction of the author's cirrus samples. And there are statements throughout the manuscript along the lines of ``the effect is larger in case X than in case Y'' where the effects in both cases are negligible relative to the uncertainty level in the model itself.

We disagree with the reviewer on a large number of points. In particular we disagree with the conclusion 1. 'the presence of natural cirrus is unlikely to significantly increase the climate effects of contrails', 2. that the low fraction of cloudy grid boxes that have a large impact is an indication that the processes have no significant impact on climate and 3. that the increase in plume water vapor of up to 20% means that the climate impact is negligible and 4. that Delta n/n is quite small almost everywhere is a sign for a low impact on radiation.

In this paper, we do not want to draw conclusions about the climate impact of contrail formation within cirrus, as the reviewer would like us to, because there are many other factors that would need to be considered within the calculation. Our goal is to clarify the processes that need to be included in such a study.

1. Radiative impact

One of the reviewer's main arguments is that the effects that we are studying do not have an impact on contrail radiative forcing. We disagree. As we said in the last review, our results do not support this conclusion, nor do they support the opposite. The effects may well have a climate impact. But the estimation of this climate impact goes far beyond the simple arguments of the reviewer and should be part of a subsequent study.

Our disagreement is caused by the fact that the reviewer assumes that the radiative forcing due to contrail perturbations is necessarily positive (if contrail ice crystal numbers are increased) and that contrail radiative forcing is on average proportional to contrail ice crystal numbers after the vortex phase. In the case of contrails forming in cloud-free ice-supersaturated air this is the case. But it is not true in the case of contrails forming within pre-existing cirrus. The radiative impact of contrails forming inside cirrus depends on the optical depth perturbation caused by contrail formation and on the optical depth of the undisturbed cloud. Among others, Meerkötter et al. (1999) and Markowicz and Witek (2011) discuss the dependence of radiative forcing on optical depth and show a decrease in contrail warming once a certain optical depth is exceeded. Therefore, a large number of contrail ice crystals after the vortex phase (and a large correction of contrail ice crystal numbers due to cirrus ice crystals) may lead in an optically thin cloud to an enhanced warming and in an optically thicker cloud to a cooling relative to the undisturbed cloud. The overall impact of contrail formation within cirrus likely depends on the balance of the two effects. A large optical depth perturbation in a cirrus that has an optical depth that causes the maximum radiative impact may well have a smaller radiative impact than a relatively small optical depth perturbation at a much lower or much higher cirrus optical depth. Furthermore, the change in radiative forcing due to the contrail perturbations depends on the ice crystal habit of the natural cloud and their change due to contrail formation.

Since corrections in ice nucleation are large in clouds with large ice water content which are likely connected with large optical depth, we may possibly underestimate the cooling of the contrail perturbations when disregarding the impact of cirrus on contrail formation. The reviewer says that 'the presence of natural cirrus is unlikely to significantly increase the climate effects of contrails.' As the reviewer suggests, there is actually a good possibility that the impact of the cirrus ice crystals on contrail formation is decreasing the climate effects of contrails, although it appears from the previous review that we disagree on the reasons for this cooling impact. Disregarding the impact of cirrus on contrail formation may then result in an overestimation of the contrail impact. Determining this climate impact should be the topic of future research.

We have added text in the conclusions discussing the difficulties assessing a potential climate impact of contrail induced cirrus perturbations even though this is not the topic of our paper.

2. Statistics

The reviewer states that the low fraction of cloudy grid boxes where the impact is large is an indication that the processes have no significant impact on climate. We do not believe that this is a valid argument because the places in which the impact of cirrus on contrail formation is larges are the places in which contrails can be expected to have a large impact. This means that there are reasons to expect that the impact of cirrus ice crystals on contrail formation can have a significant effect.

The formation of contrails in cloud free air is known to have a large impact in large scale ice supersaturated areas connected with frontal systems (Bier et al., 2017; Burkhardt et al., 2018) which is the same regime where we find an impact of cirrus ice crystals on contrail formation. In particular, large scale upward motion and the connected water transport into the upper troposphere, as found in the context of frontal activity and conveyor belts, facilitate a large radiative impact. This means that the regimes in which the corrections in contrail ice nucleation are relatively large are the same regimes/ times when contrails formed in cloud free air often have a large climate impact. This points at the possibility of cirrus ice crystals having a significant impact on contrail properties within cirrus and leading to a significant radiative impact.

Furthermore, it is well known that a small fraction of contrails forming in cloud free air explain a large share of the climate impact of contrail cirrus. This means we need to study contrail formation within those regimes where the climate impact is strong even if those situations are not common. Studying average contrail properties or the average impact of cirrus on contrail formation may be irrelevant (the same way as it is close to irrelevant to know the average life time of contrails). Our study agrees in so far with the Gierens (2012) study that on average there is hardly any impact of cirrus on contrail formation. But it is the whole point of the paper to probe a large number of regimes, atmospheric states and cloud properties in order to find out if in some cases it does matter. If those situations, in which the impact of cirrus ice crystals on contrail formation are relatively big, are situations in which the climate impact may be large, then this warrants the inclusion of this process within the model. We believe that this may well be the case but it is not within the scope of this paper to study the climate impact of the cirrus induced changes. This climate impact should be studied in a subsequent study (see above).

The argument of the reviewer is not really clear here 'If the authors want to argue for the validity of results at the few percent level (or, in many cases in the paper, fractions of a percent)'. In case this means that the reviewer is doubting that there is a signal at all given that the fraction of larger changes is so low:

If we were conducting a field significance test, then a significant result in a tiny fraction of grid boxes should be expected simply by chance and would be a sign that there is no physical connection. But we don't do that. In our case a small fraction of large changes simply means that in certain conditions (mainly large ice water content IWC) we can see a signal. The physics behind this connection is clear. The low fraction of large changes is simply caused by the fact that we use IWC 10^{-11} kgm^{-3} as a cloud mask and therefore include subvisible cirrus. As we commented in our last revision, the probability of large changes is a function of the minimum IWC that we use as a cloud mask. By setting the threshold to a higher value, the fraction of large changes can be easily increased.

We have modified the text at the end of section 3.2 (line no 556 to 559).

3. <u>Change in plume water vapor negligible</u>

The reviewer says that the presence of natural cirrus is unlikely to significantly increase the climate effects of contrails because plume water vapor is enhanced by at most 20%. As our analysis shows, this increase in plume water vapor can lead to changes in the contrail formation threshold of a few Kelvins (maximum 2K) and associated increases in ice nucleation can have the same order of magnitude as the ice nucleation when neglecting the impact of cirrus ice crystals even at temperatures of several Kelvins below the formation threshold. We judge this to be a significant change.

The reviewer says that we should accept that our study reinforces the conclusions of Gierens (2012). Gierens estimates the enhancement of the plume water vapor to be on the order of a few percent in case of a thick cirrus while we show that it can be as large as 10% and very seldomly reaches 20%. As we show in the paper, this difference is mainly due to the fact that Gierens (2012) does not consider the impact of cirrus ice crystals entrained into the plume while we do.

The discussion of this topic can be found in section 4 (line no 638-649).

4. Delta n/n

The reviewer argues 'for large contrail ice numbers (i.e., where contrails have a potential for significant impacts) Delta_n/n is quite small almost everywhere in the sampling of cirrus conditions'. As we point out under 'climate impact', the relative change 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.

As discussed under 'Statistics' it is true that in large areas of the clouds sampled the change is small. But as we show in the paper it is not true for cirrus with a large ice water content. In figure 5 and 6 we show the contrail ice numbers, n_i, when not considering the impact of cirrus ice crystals on contrail ice nucleation and its change Δ n_i when considering sublimation and deposition on cirrus ice crystals. Whereas on the main air traffic levels changes can reach maximally 10% of n_i, on the lower levels changes are larger and can be of similar size than n_i. Since the radiative impact is not a function of contrail ice crystal numbers alone (see above under 'Radiative Impact'), the analysis should not concentrate only on cases of very high contrail ice crystal numbers (~ 10^8 m^{-3}).

Concerning your comment 'Further, even the modest enhancements of contrails by cirrus that are seen in the results may be reasonably interpreted as upper bounds, since the shortcomings in the parameterizations and analysis employed (e.g., those listed in my prior review) generally tend toward overestimation of contrail ice crystal number.'

We assume that the reviewer refers here mainly to his results from Lewellen (2020) who shows that box model estimates overestimate contrail ice crystal numbers when compared to LES for high soot number emission indices. Since we assume a soot number emission index of $2.5*10^{15}$ kg-fuel^{-1} we are still in the regime where the reviewer judges the box model to agree with LES. Furthermore, the Kärcher et al. (2015) parameterization may generally overestimate ice nucleation relative to the box model estimates since it does not include subsequent nucleation. But, in particular, when contrails form within a cloud large differences in aerosol properties should be severely reduced, limiting this sensitivity (see below for more details).

Finally, 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. An increase in plume water vapor is bound to change the contrail formation threshold and ice nucleation independent of the fact that the nucleation parameterization that we use does not consider e.g. the impact of plume inhomogeneities on ice nucleation. It is common in climate science to evaluate a systematic change in a field that is subject to large uncertainty and variability, which is often caused by limited information on e.g. aerosol concentrations and properties or connected with the low resolution of a model.

You indicated the following lines (22-23, 697-700, 702-704, 706-707, 713-714, 755-757) which highlight differences to Gierens (2012). As we argued above we find significant differences to Gierens and believe that it is appropriate to discuss those differences and believe that they can have an impact (see above). Please see below for the discussion of modeling uncertainties.

Further comments in regards to the authors' responses to the specific numbered points in my original review (using the original numbering) follow:

(1) On approximations in the nucleation treatment: Some improvements have been made here (both in the approximations used and in the caveats included in the text). Including some estimate of the effects of cirrus crystals mixed into the exhaust plume is an improvement, though it is quite rough (as is clear from appendix A). Whether these changes are adequate depends on the conclusions one is trying to draw (see discussion above). The changes made a sizable percentage difference in some of the results the authors are highlighting (e.g., in figs 3-6). If the remaining shortcomings (e.g., a single activation time, not including a size spectrum so crystal losses can't be assessed, neglecting effects of plume inhomogeneity, omitting some feedbacks, etc.) were addressed one could expect to change the results at least as much (i.e., the uncertainty level is sizable).

The reviewer comments that our estimates presented in appendix A are quite rough but does not hint at shortcomings. We assume here that he refers to the temporal evolution of the dilution rate. We use the dilution rate given by Kärcher et al. (2015) which is again based on Kärcher (1999). Kärcher (1999) shows that the entrainment rate used in Kärcher et al. (2015) is a good fit to the entrainment from LES (Gerz et al., 1998) at times >~0.01s. Since we start calculating entrainment and sublimation of cirrus ice crystals at 0.01s we should not overestimate sublimation due to overestimating entrainment. Another reason for the 'rough estimate' may be the impact of neglecting the ice crystal size distribution - please see below for a discussion. The error of our estimate of the ice sublimation/deposition when estimating from a midway value instead of calculating the temporal evolution is a few percent which is certainly a good estimate.

Most of the appendix was rewritten.

In the following the reviewer gives a list of processes that may or may not impact our simulations significantly with no indication of which of those processes the reviewer regards to be most important and why. 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.

<u>Plume inhomogeneity:</u>

The overestimation of contrail ice nucleation due to not resolving plume inhomogeneities we expect to be small, since the reviewer concludes in Lewellen (2020) that plume inhomogeneities would lead to changes in ice nucleation particularly when choosing very large soot number emission indices such as 10^{16} kg-fuel^{-1} or higher. For soot number emission indices of 10^{15} kg-fuel^{-1}, which is close to what we chose, the reviewer concludes that box modelling produces a good estimate of ice nucleation.

Text had been added during the last revision in section 2.2.1 and was now changed (line no. 227 to 230)..

<u>Consecutive nucleation (single activation time):</u>

In the reviewer's paper (Lewellen, 2020) he concludes that incorporating differences in activation time due to different aerosol properties is of secondary importance, less important than plume inhomogeneities. Including different activation times requires information on e.g. concentration, size distribution and hygroscopicity of ambient and emitted aerosols that are not readily available. The reviewer chose in his paper very different values for hygroscopicity of ambient aerosols than Kärcher et al. (2015), maximizing the difference between emitted soot and ambient aerosols. Differences appear to be caused by different assumptions in the chemical composition of the aerosols. The assumption of equal concentration of large and small aerosols within Lewellen (2020) is additionally likely to overestimate the impact of different aerosol properties.

The impact of consecutive nucleation is dependent on the entrainment of ambient aerosols and their properties. Within existing cirrus, we can exclude entrainment of aerosols into the plume that preferentially form ice crystals. But aerosols will be added to the plume due to the sublimation of ice crystals within the engine. When ice crystals sublimate within the engine the sulfuric acid droplets, an aerosol on which very many ice crystals form, evaporates while the few IN, such as soot or dust, may be released with unknown properties increasing the already high soot number emissions slightly. We conclude that the problem of sequential activation due to differences in aerosol properties is much reduced when estimating ice nucleation within preexisting cirrus.

We added text accordingly in section 2.2.1

Not including a size spectrum so crystal losses can't be assessed

We have performed some offline calculations trying to estimate the size and number of ice crystals that may sublimate completely within the plume. We find that if an ice crystal is mixed into the aircraft plume at about half the time between emission and ice saturation then ice crystals up to a radius of around 1.5 µm can completely sublimate. Assuming a grid box average ice crystal radius of 6 µm (5 µm), which are the smallest values that we (only occasionally) find in our simulations, and assuming ice crystal sizes to be distributed according to a generalized gamma distribution (Seifert and Beheng, 2006), about 5% (10%) of ice crystals within the grid box have a size of below 1.5 µm. From this calculation we conclude that the impact of the change in cirrus ice crystal numbers within the plume due to complete sublimation before nucleation is rather limited. Additionally, the complete sublimation of cirrus ice crystals would likely have a smaller impact on the sublimation than on the subsequently happening deposition and, therefore, would be likely to lead to an underestimation of the increase in plume water vapor and of the impact of cirrus ice crystals on contrail ice crystal nucleation. This means that, when considering the loss of ice crystals during sublimation on our estimate of sublimation and deposition on cirrus ice crystals, our estimate is very likely conservative.

Large parts of the appendix were rewritten.

It is certainly true that 3D LES are much better suited to simulating all the processes relevant to estimating ice nucleation. But our ICON-LEM is better suited to sampling a large number of different atmospheric conditions connected with certain synoptic situations and estimating the impact of the atmospheric development on the development of contrails. In order to do this the model needs to capture as much of the physics of contrail formation as possible even if the resulting estimates will have a larger uncertainty than LES estimates given one particular atmospheric state.

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. Errors in estimating sublimation and deposition on cirrus ice crystals within the plume, such as those errors connected with neglecting cirrus ice crystal loss, would have a direct impact on this sensitivity but we estimate that the relative error in our estimate is likely small.

(2) On approximations in the crystal loss parameterization: The changes implemented are in the right direction but still miss most of the important effects. The revised treatment is implemented as if the Kelvin dependent effects were important only early on, before the ``vortex descent''. This is not so. Through much of the vortex descent, conditions arise in which the larger crystals continue to grow

while the smallest sublimate away. Further, there is significant detrainment from the descending plume leaving portions that never descend enough to achieve subsaturated conditions for all crystal sizes (but with losses for the smallest). And to emphasize again: the parameterization of Unterstrasser (2016) is based empirically on LES results that properly include the Kelvin effect, but not with ambient cirrus crystals included. The current treatment in the manuscript is probably best interpreted as estimating an upper bound on the crystal survival fraction during vortex descent, perhaps greatly underestimating the potential in some heavy cirrus regimes for additional Kelvin dependent losses. Whether this treatment is adequate again depends on what conclusions one is trying to draw (see comments above). I would argue that the only conclusion in this regard that the results in the manuscript will currently reliably support is that crystal loss rates are unlikely to be significantly reduced by the presence of existing cirrus; I would recommend heavily trimming the lengthy section 3.3 accordingly.

The reviewer misunderstands our approach. We do not assume that the Kelvin effect ends at vortex descent and we do not simply use the parameterization of Unterstrasser (2016) once the vortex is descending. Instead we use the parameterization after adjusting the water vapor emission according to the sum of the deposition on cirrus ice crystals before the vortex phase descent and the sublimation during vortex descent. 'In order to be able to use the parameterization of Unterstrasser (2016), that does not include the impact of cirrus ice crystals on the survival fraction of contrail ice crystals, we adjust the water vapor 'emissions' of the air plane, which is an input in the parameterization. While the water vapor emission is usually given by the EI_H2O coming from fuel combustion, in the context of contrail formation within cirrus we use the 'aviation induced increase in water vapor' that includes the sum of the sublimation of and deposition on cirrus ice crystals that changes the water vapor content of the plume. '

Therefore, we first calculate the ice crystal growth in the time between ice nucleation and vortex descent using the depositional growth equation to estimate the temporal evolution of the size of the contrail and cirrus ice crystals considering the Kelvin effect. We then estimate, using the full diffusional growth equation, how much of the cirrus ice water can sublimate in the time it takes the contrail ice crystals to sublimate in the descending vortex and limit the cirrus ice water sublimation using the time scale of vortex descent. Both those steps include the Kelvin effect. Our final step is applying the parameterization of Unterstrasser (2016) while adjusting the water vapor 'emission' according to the sum of the deposited and sublimated water vapor on the cirrus ice crystals. This means that we do consider the Kelvin effect before and during vortex descent.

This approach should give us a very rough estimate of the impact of cirrus ice crystals on the survival of contrail ice crystals within the vortex phase. Since, on the one hand, the time until vortex descent is not well defined and, on the other hand, during the first few seconds of vortex descent relative humidity may be such that cirrus ice crystals could grow at the cost of contrail ice crystals, we vary the growth time period before vortex descent (19s instead of 9s) in a sensitivity simulation. We hardly find a difference in the resulting impact of cirrus ice crystals on contrail ice survival. Deposition and sublimation rates close to ice saturation are very small and differences in cirrus and contrail ice crystal radii and cirrus ice crystal concentrations are not large enough to let cirrus ice crystals grow significantly at the cost of contrail ice crystals.

We have rewritten large parts of section 2.2.4 and hope that it is now easier to understand.

Finally, Yes, we agree with the reviewer that the presence of cirrus ice crystals does not significantly change the contrail ice crystal survival. We have cut section 3.3 significantly.

(3) On uncertainties in the near-threshold regime: The caveat added to the revised text is useful and important. But it does not alter the fact that the enhanced uncertainty in the near-threshold regime likely swamps most of the results that the authors are calling attention to there.

In the paper we call attention to the changes in ice nucleation that are connected with large cirrus ice water content independent on if a contrail forms close to the formation threshold or not. Besides, assuming that the reviewer uses the same terminology here as in his 2020 paper, a 'near threshold case' refers to contrail formation within a few tenth of a degree below the formation threshold. As can be seen in our figures 5 or 6 the impact of cirrus ice crystals on ice nucleation can be significant at temperatures several Kelvins below the formation threshold. Therefore, our main results do not come from near threshold cases (using the above definition of 'near threshold').

Furthermore, a systematic change in the contrail formation threshold, as we are finding due to the sublimation of cirrus ice crystals in some regimes, is certain to increase ice crystal numbers within the cirrus even if the resulting ice crystal numbers are highly sensitive to ambient temperature and the contrail formation threshold temperature, humidity, contrail inhomogeneities and many other variables. Therefore, the uncertainty does not 'swamp' the signal. Instead our signal is a systematic change of the highly variable ice nucleation.

Please note that it is common in climate science to evaluate a systematic change in a field that is subject to large uncertainty and variability.

(4) On not including the aircraft induced cirrus losses, etc: I find the added section 4 and fig. 11 (as well as related statements in various places such as lines 16-17, 530-534, 743-745, 767-768, 785-786) rather beside the point. The point in this work is not to compare ``contrail'' with ``no contrail'', but to compare ``contrail in presence of cirrus'' with ``contrail in absence of cirrus''. It is well known that in contrail-favoring conditions, contrails typically produce much greater crystal number densities than natural cirrus; the current work gives no new insights in this regard that I am aware of. Nor is fig.11 convincing evidence that losses of cirrus crystals due to the aircraft (which the authors are not computing) are necessarily negligible compared to the contrail ice numbers in the regimes where the authors are highlighting cirrus effects on contrails. In typical conditions within fig 11 it is indeed the case that the number concentrations from the contrail greatly exceed those from the cirrus. But in the small fraction of the cases that the authors tend to emphasize in the current presentation this needn't be so, either because the cirrus number concentrations are very high, or the contrail numbers are much lower (near-threshold). And I think the authors are missing an opportunity here in fig. 11. What they should compare for their purposes is this figure with the analogous one where the contrail numbers are those produced by their model when not including the existing cirrus. Judging from the results presented elsewhere in this paper, I suspect that the two figures would look nearly identical almost everywhere, providing good visual support for what I am arguing should be the main conclusion of this work.

It is true that the new section added a slightly different direction to the paper. Originally, we added this section due to the reviewer's comment 'For assessing the impact of aircraft on climate (presumably the ultimate motivation here) what matters is the net effect on the total system of contrail plus natural cirrus.' Apparently, we misinterpreted this comment.

Anyway, we cut this section since a proper discussion and evaluation of this would make this section too lengthy. We did not cut statements referring to the impact of contrail formation for cirrus cloudiness as long as those statements are supported by the results of the other sections. We believe that those statements are of benefit for a general reader. After all, we do not want our paper to be only read by the 'contrail community' but we would like it to be also informative for people generally interested in cloud processes who may not find those statement all that trivial.

As to the description of the methodology for comparing ``contrail in presence of cirrus'' with ``contrail in absence of cirrus'' in this work: the added statement in lines 341-343 remains ambiguous. What comprises ``exactly the same situation except for the absence of cirrus ice crystals'' depends on what variables are being used to describe that situation (e.g., water vapor vs total water), and these have not been specified here. It is not possible to set the cirrus ice number (or IWC) to zero while leaving all other variables exactly the same.

The text reads now ' In section 3.3 we will discuss the impact of cirrus ice crystals on the loss of contrail ice crystals during the vortex phase comparing it to the ice crystal loss that the contrail would have experienced in the same situation except for the absence of cirrus ice crystals and with a corresponding reduction in total water.'

(5) On considering where contrail radiative impacts are likely to be high (and where not): My concern here remains as before (and essentially unacknowledged in the manuscript). The regimes where the authors are claiming ``significant'' enhancements to contrail ice numbers due to the presence of existing cirrus are precisely regimes where the potential climate impacts of contrails are likely to be naturally reduced: near-threshold conditions (where contrails will have lower optical depths and shorter lifetimes) or high-IWC, high-ice-number cirrus (where the surrounding cirrus with its large optical depth can largely mask the radiative effects of the embedded contrail, as well as reduce the contrail's crystal numbers over time through Kelvin-dependent crystal losses).

The answers to this point are already given above in our discussion of the 'radiative impact' and in connection with your comment 3.

Newly added literatures:

Markowicz, K. M., & Witek, M. L.: Simulations of Contrail Optical Properties and Radiative Forcing for Various Crystal Shapes. Journal of Applied Meteorology and Climatology, *50*(8), 1740–1755, https://doi.org/10.1175/2011jamc2618.1, 2011.

Meerkötter, R., Schumann, U., Doelling, D. R., Minnis, P., Nakajima, T., & Tsushima, Y.: Radiative forcing by contrails. Annales Geophysicae, 17(8), 1080–1094, https://doi.org/10.1007/s00585-999-1080-7, 1999.