Response to the reviewers' comments on "Radiative forcing of anthropogenic aerosols on cirrus clouds using a hybrid ice nucleation scheme"

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We thank editor and two reviewers very much for dedicating their time to read our manuscript and present important comments. We carefully studied these comments and revised the manuscript widely. Replies to these comments are listed point by point as below.

Reviewer #1

The revised version addresses most of the comments raised by the reviewers, however there are still a few issues that should be considered to further improve the manuscript.

In particular:

- The statistical tests are not shown on every figure. In Figure 3, 4 and 5 these are depicted only for panel (a). In Figure 7, 8 and 9 they could also be shown in panel (f). The same would apply to Figure 12h and 13. In Figure 11, tests are also arguably shown only for panel (a), although this is not specified in the caption.

Re: We have added statistical significance in all panels in Figures 3, 4, 5, 7, 8, 9, 11, 12, 13.

- Statistical significance should be mentioned for the globally averaged changes discussed in the text (for example at lines 792 and 806, but there are other occurrences in the text). The same applies to the values in Table 3 (here I would suggest to mark the statistically significant changes somehow).

Re: We checked all differences in global average Ni, IWP, LWP, SRF, LRF and NRF list in Table 3 and marked the statistically significant results. All the global average NRF are not statistically significant.

- Thank you for adding Table 1. Could you also specify which fraction of Aircraft OM/BC, Dust and SOA are assumed to be effective INPs? I guess this is 100% for aircraft and SOA, but what about dust?

Re: As stated in Table 1, we assumed pre-activated aircraft soot within contrails with less than 3 monolayers of sulfate, dust with fewer than 3 monolayers of sulfate coating and glassy SOA in the accumulation mode are effective INPs in our model. We did not set specific fraction for aircraft soot, dust and SOA.

Technical corrections:

L195-198: this sentence does not look correct ("We assume…, is assumed")

Re: We have changed this sentence to "The soot that has already been included in contrail ice is pre-activated. We assume the pre-activated aircraft soot coated with less than 3 monolayers of sulfate to be an INP similar to the treatment in Zhou and Penner (2014)".

L586: "changes in LWP are all less than 0.5%". Is this a global average?

Re: Yes, we have changed to "changes in global average LWP".

Reviewer #2

I thank the authors for nicely addressing a lot of my concerns, and including a new sensitivity test in the study.

In my opinion there are still a few issues in the manuscript that I would like to point out.

General comments:

1.) By including a form of a significance in their global map plots, the authors often confirmed my worries about the statistical significance of regional changes to several plotted quantities. The authors would ideally follow the work by Chen and Gettleman, 2013 strategy, which decreases the natural variability (and increases the signal to noise ratio) by running a longer simulation that is nudged always to the meteorology of one specific year only.
I do understand that may not be possible in the current study. However, it may still be beneficial to either extend the simulations for several years or add additional ensemble runs to increase the significance and confidence in a lot of the results, particularly those related to radiative flux anomalies.

Re: Thanks so much for your suggestion. We will try to run a longer simulation nudged to the meteorology of one specific year. We expect to increase the significance in our future work.

2.) Visualization

I give some more suggestions to the authors regarding their visualization. and would appreciate if they would take those points into consideration both now and in the future.

(i) An article should ideally contain the minimum number of figures necessary to deliver the scientific message. I suggest therefore to:

- move to the appendix panels b,d,f of figure 6
  Re: We have moved Figure 6b, d, f in the previous version to Figure S5.

- think whether they really need to show both all-sky net radiative forcing and cloud radiative forcing in figures 7,8,9,12.
  Re: Regarding to the previous comments from the other reviewer, we added the plots about cloud radiative forcing.

(ii) All zonally averaged plots should also include some form of statistical significance (e.g. standard deviation)

Re: We have added statistical significance in all zonally averaged plots (Figures 7, 8, 9, 12, 13)

(iii) The authors should think about stippling the significant gridboxes in a way that does not prevent the reader to read the value below the dots. (e.g. you could try using smaller dots or hatching)

Re: We have changed some dots to smaller ones to improve the visualization. Thank you for the comments.

(iv) A bar chart showing global and maybe other zonally averaged (e.g. tropics, northern hemispheric mid latitudes) quantities, particularly radiative fluxes, may be easy to digest and a nice complement to figures 6,7,8,9. Such a bar plot should indeed also include a form of uncertainty (standard deviation?)

Re: We think the zonally average changes in Ni, IWP, LWP and radiation have been shown in Figure 13 for all latitudes, so that it is easy to compare the changes among tropics, norther hemisphere and southern hemisphere. However, we still thanks your suggestion and will consider to try bar charts for these comparisons.

3.) Comment on the simulated ice crystal burden

The ice crystal number concentration burden shown in Figure 2a) seems to be missing the observed increases in ice number over orography, particularly over the Andes, Rockies, the Antarctic mountains, and Greenland.
I would suggest that your follow up studies compare also regional ice crystal number

patterns in your model to those observed by CALIPSO-CloudSat (e.g. Sourdeval et al., 2017, Gryspeerdt et al., 2017). I therefore assume your model does not include an orographic wave drag parameterization, or something similar that enhances updrafts over orography?

A comment about the missing ice crystal burden peak over such regions would be appropriate at some point in the manuscript (maybe in section 3.1 when describing results from Figure 2).
Re: Thanks so much for your suggestion. We will try to include an orographic wave drag parameterization in our future work and compare to CALIPSO-CloudSat observations. We have added statement as "Due to the lack of the effect of orographic wave on ice nucleation, the observed increases in ice number over orography, particularly over the Andes, Rockies, the Antarctic mountains, and Greenland are not shown in Figure 2." starting at Line 468 (Section 3.1).

Specific comments:

Line 136:
I think Kuebbeler et al., 2014 was not the first to add the effect of orographic waves into the ECHAM model. A more appropriate citation there may be Joos et al., 2008, while Kuebbeler et al. 2014 could still be cited as another study showing the dominant role of homogeneous ice nucleation, maybe in line 64.
Re: We have changed the citation to Joos et al. (2008) for the contribution of orographic waves and moved the citation to Kuebbeler et al. (2014) to Line 64.

Lines 317 – 320:
Does this mean that part of the large Ni peak in the warm pool originates from anvil detrained ice water content and detrained vapour (if vapour is detrained). Or the opposite, the detrained ice is suppressing ice nucleation by decreasing the RH_{ice} by vapour deposition?
Re: As we stated in Lines 314-317, in our model, anvil clouds and in situ cirrus compete for the available water vapor within a grid box. When anvil clouds are formed due to convective detrainment, it reduces the saturation ratio in the clear-sky portion of a grid, and can potentially reduce the frequency of in situ large-scale cirrus formation.

Lines 474-476 and Figure 3e,f:
I do not see any significance in Figure 3e and 3f.
Do panels 3b-f, 4b-f, 5b-f include the significance like panels 3a,4a,5a? If not, please add significance stippling/hatching to those panels!
Re: We have added statistical significance in all panels in Figures 3, 4, 5.

Additional comment:
I would find it useful if your answer below would find the way to the manuscript text.

Why is the effect not larger in the midlatitudes, where soot emissions are the largest?

Re: The number concentration of ice nuclei from homogeneous nucleation is *largest in the tropics as shown in Figure 3(c), so the effect of inhibiting homogeneous nucleation as a result of adding the heterogeneous nucleation of soot is larger in the tropics although soot emission is larger in midlatitudes.*

Re: We have stated "Although the emissions of aircraft soot are large in midlatitude, the effect of inhibiting homogeneous nucleation as a result of adding the heterogeneous nucleation is large in the tropics (Figure 3c). That is because of the largest number concentration of ice nuclei from homogeneous nucleation in the tropics as shown in Figure 2c." starting at Line 483.

References

Chen and Gettelman, 2013, Simulated radiative forcing from contrails and contrail cirrus

Gryspeerdt et al., 2018: Ice crystal number concentration estimates from lidar–radar satellite remote sensing – Part 2: Controls on the ice crystal number concentration

Joos et al., 2008: Orographic cirrus in the global climate model ECHAM5

Kuebbeler et al., 2014: Dust ice nuclei effects on cirrus

Sourdeval et al., 2018: Ice crystal number concentration estimates from lidar–radar satellite remote sensing – Part 1: Method and evaluation