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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Influence of the actual weather situation on non-CO_2 aviation climate effects: The REACT4C Climate Change Functions" *by* Christine Frömming et al.

Anonymous Referee #1

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Review of Frömming et al.: Influence of the actual weather situation on non-CO2 aviation climate effects: The REACT4C Climate Change Functions

SUMMARY:

The study presented in this manuscript investigates how typical weather situations in the North Atlantic region affects the most important non-CO2 climate change mechanisms from aviation emissions. The North Atlantic flight corridor, which connects Europe and North America, is one of the busiest regions for commercial air travel and the authors give very good reasons for their geographical choice. The authors have a track record of publications in this field and demonstrate awareness of the published lit-

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erature, even though they appear to focus more on publications by European research groups. The model chosen for this study has been used previously to assess the impact of non-CO2 aviation emissions in a climate context. In summary I feel this is a good manuscript about a worthwhile study which has been carried out with diligence. It demonstrates good scientific significance and quality and is presented in a focused manner. Therefore, I would like to congratulate the authors to this manuscript. Having said this, I have several concerns that I would like the authors to address before I recommend accepting the manuscript for publication. While there are many good aspects about this manuscript, the remaining part of my review will focus on the areas where I think improvements are necessary.

GENERAL CONCERNS:

This paper is one of several publications based on work from the REACT4C project, hence there are many references to companion papers which describe partly the model and methodologies. It would be helpful if the authors can describe very clearly the novelty and individual contribution presented in this manuscript. The authors state that apart from a Grewe et al. 2014 pilot study this is the first assessment of the impact of weather situations on aircraft climate impacts. I am making this point because, for instance, a subset of the authors of this manuscript have also authored the Rosanka et al. ACP companion paper where there seems to be partly an overlap with scientific aims. The Rosanka paper is however only mentioned late and rather cursory. Further, the Irvine et al. 2013 study also has been a precursor study for this work. It would be good to explain more clearly and early in the manuscript how this study distinguishes itself from the other studies and how the REACT4C publications relate to each other.

The authors correctly state that previous publications focused on aircraft emission climate impacts from an annual average/climatological perspective. An important reason for this is that global chemistry-climate models (at least those comprising a reasonably detailed treatment of tropospheric and stratospheric chemistry) are computationally so expensive that the higher grid resolutions which are typically employed to study syn-

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optic scale weather patterns are beyond the reach of these models. In the study presented here, the authors have employed a model with a quadratic Gaussian grid at T42 which results in grid point spacing greater than 300 km at the Equator. The spacing will become a bit smaller at mid-latitudes where the focus of their study lies, but it still is substantially coarser than grid resolutions used for synoptic scale considerations. Naturally, the authors are limited by the above-mentioned computational constraints. Do they consider the grid resolution as a constraint for the validity of their results at all, given that they are focusing on the impact of synoptic scale weather patterns rather than multi-annual averages? Would they expect their findings to be sensitive to grid resolution? This has not been mentioned at all and I would think this could be a considerable caveat. I would like these considerations included in the discussion, given that emissions from aircraft flight tracks, contrail formation, and many synoptic processes are sub-grid scale processes for the grid resolution that is used here.

The technical description how the experiments are carried out remains unclear and needs to be described better. In Section 2 the components of the modelling system are listed diligently by referring to large number of citations, however how the 280 experiments relate to the 4032 perturbation events, the particulars of the trajectories that are released at the perturbation points, and how all this leads to the results shown in the figures remains to some extent a mystery to me. There is much more clarity needed.

SPECIFIC COMMENTS:

l. 42: Lee et al 2010 remains an important paper but it is now more than 10 years old. Perhaps add Brasseur et al 2016 or other more recent review/assessment papers?

l.71: Correct typo ‘respect’. What is meant by “user defined” penalty factors – please explain this a bit clearer.

l. 89: In my opinion the companion paper Rosanka et al., ACP, 2020 should be mentioned already here and not only in l. 323, as their findings appear to result also from

actual weather patterns (related to the same project?)

l. 107: The entire Section 2 forms a dense amount of text which is difficult to digest as one block. I suggest structuring it more clearly by breaking into subsections. I would suggest at least a section describing the model components, and a further section describing the set up used for the experiments and how the model outputs are obtained (combination of Eulerian and Lagrangian approach?). A dedicated description of how the experiments were conducted is missing, instead it seems to be scattered over the remaining parts of the manuscript. Please draw this together in a clearly reproducible experiment description.

l. 131: Why is 00 UTC not part of the emission times? On what criteria have the authors chosen the magnitude of the NOx and H2O emission perturbations, are they in any way related to an emissions inventory? Particularly the H2O value looks oddly precise. Also, are these perturbations in addition to the existing aircraft emissions at the grid point location?

Table 1: Thinking about the background conditions at these grid point locations: How do they overlap with the present flight routing/aircraft emissions distribution? Are they all located inside the main North Atlantic flight corridor?

l. 207: In the interest of understanding, clarity and reproducibility, it is not clear how the perturbations were applied. Obviously, this study consisted of a substantial number of model experiments (as indicated by the total CPU hours), however the 280 experiments remain still an order of magnitude below the 4032 perturbation events. How were they grouped together? How many perturbations were applied during one experiment? How many experiments represent one weather situation? Further, after a detailed listing of the model's abilities and sub-components (with citation) in Section 2 of the manuscript, there is a lack of technical detail how the experiments were conducted. Were they all initialised from a common set of initial conditions, perhaps one for each weather situation? How long was the model spun up for prior to the 90 days of the output that

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was taken into consideration?

Figures 2 and 3: It is not clear how these findings relate to the experiments: Is this the additive result of all the perturbations combined? How do you obtain from perturbations in individual grid cells, which are propagated through trajectories, a regional map of potential contrail coverage in %? This should become clear through an improved experiment description in Section 2. Similarly, this will also help interpreting Figs 7, 10 and 11.

While the method of compiling the information needs better explaining, I would like to point out that I find the information conveyed by these maps of great value and I welcome this method of visualising the results in general (and I am glad that figure labelling has a reasonable, legible size!)

Section 4.2: My concern here stems again mainly from a lack of understanding how to interpret the figures. While contrails are formed almost instantly in the wake of the aircraft, impacts from NOx emissions have a longer lifetime and the map representation in Figs 7 and 10 only makes sense if it indicates the location of the emissions release (even though the climate change impact might be felt elsewhere). Can the authors confirm this?

l. 379: It would be interesting to know the author's view on why the jet stream region would yield generally more positive values, seeing that otherwise subtropical high-pressure belts exhibit positive values.

l. 382: Here the authors compare their findings with climatological studies that were partly carried out with even lower grid resolution (e.g. T21). Previously the authors have shown the importance of the pathway of NOx emissions which would be better resolved with higher model grids. To what extent do the authors believe a T42 representation of short-lived weather situations offers a fair comparison to climatological studies? While I can understand that it can instil confidence to have findings agree with previous studies, I would like to be reassured that the comparison is on fair terms?

How important do the authors consider grid resolution in their study? What is the minimum resolution required to resolve the weather situations sufficiently to represent the emissions transport on non-climatological time scales?

l. 413: explicitly (typo)

Conclusions: The first paragraph is fairly high level, only telling me that jet stream location, tropopause height, high pressure systems and polar night are all having an influence, but this is not further elaborated. It would be nice if the main findings or messages could be brought more clearly across. Is there a pattern that the authors recognise? If the results are too diverse to summarise then perhaps it should be stated here.

l. 469 or l. 479: As previously mentioned, the conclusions here need to acknowledge the limits in grid resolution and what impact the authors believe this might have on their findings.

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