Authors comment (AC2) to Referees comment (RC2)

Dear Referee,

on behalf of all authors I would like to thank you very much for taking your time to comment during the open discussion of the preprint. We addressed all of your comments and used the propositions to improve the manuscript for the reader, wherever possible. Please find our answers below in italic blue just to your comments (black).

Best regards,

Johannes Pletzer

This paper presents an assessment of the impacts of H2, H2O and NOx emissions from high-flying hypersonic aircraft. This is based on a series of calculations with a detailed 3D chemistry-climate model which are analysed for the changes in composition and radiative forcing.

This is an interesting topic and well within the scope for ACP. The tools and methodology are entirely appropriate for the problem and there are a number of useful results in this paper. However, in my opinion the paper is not suitable for publication in its current form. The paper is very long and I find the presentation very chaotic. I think the main points could be communicated in much less text and far fewer figures. There are also a number of typos and mistakes in the text which also detract from the overall impression.

Therefore, I think the paper needs major revisions. My main comments are below. Given that my advice is a significant shortening of the paper I have not provided comments and all parts of the text.

We appreciate your support of our methodology and results and would like to thank you for pointing at typos in the text.

The most stressed comment refers to the length of the paper.

We would like to ask the Editor, Prof. John Plane, to give his thoughts on this matter. From our perspective, we received the feedback to publish as is from RC1 especially pointing out the broad coverage of results and in turn, we were asked by RC2 to apply major revisions and basically shortening. In this respect, the two reviews contradict each other. We are happy to bundle certain parts of the main text as a supplement if it helps the reader. In our opinion, parts of subsection 2.3 (SWOOSH comparison and parts of section 5 (Radiative Forcing) could potentially be moved to the appendix or bundled as a supplement. While subsection 2.4 (speed-up technique) could also be moved to the appendix or a supplement, we want to refer to RC1, which found the approach very interesting and relevant for other studies and we would very much like to keep it in the main text. The appendix as it is now could also be bundled as a supplement. There are multiple options and we would require guidance from the editor.

Abstract.

I think that the abstract should be more quantitative. It should state that the results are based on 3D CCM simulations. I don't understand the message in the final sentence.

We included a remark, which kind of model we used for obtaining the results. Thank you for pointing that out.

Note that the final paragraph of the abstract was revised as a result of the first referee's comments. We think that the final sentence has become clearer.

Introduction.

I cannot see where you define/explain what a hypersonic aircraft is. You need to justify your use of 30 and 38 km for the emissions.

We added a technical explanation of subsonic, supersonic and hypersonic aircraft, as suggested by both, you and the first referee:

> *"Technically there are three categories of aircraft: subsonic aircraft that fly slower than the speed of sound, supersonic aircraft, whose speed exceeds the speed of sound, whereas the speed of hypersonic aircraft is at least five times the speed of sound."*

The section numbering in the final paragraph is wrong.

The section numbering has been corrected. Thank you for pointing that out.

Model Experiments

The model experiments should be given labels, including the new runs for this paper. Sometimes they are referred to as 'ours' which is not clear. The text says that all runs use nudged dynamics? Is that the case even for the future composition? The simulation years are also always in the range 2007-2017, or so. This is the case for the future scenarios, yes? Overall there are a number of things to do to make the model simulations clear for the reader.

Appearances of "ours" in the context of the model setup were replaced with the label "HS-sens", which is now introduced in subsection 2.1.

We can confirm that all runs use specified dynamics (nudged towards ECMWF data). In contrast, the chemical lower boundary conditions are based on 2050 (see new Table 1). We partly removed time ranges (e.g. "2013-2016") throughout the preprint, e.g. from figure captions, to avoid misunderstandings. The calculation of results is further clarified in section 4, first paragraph, with the following sentence:

> *"Note that all presented data in this section is based on multi-annual mean model results with both, specified meteorology (2013-2016) and 2050 source gas emissions."*

The subsection 2.1 was revised and shortened. Subsection 2.2 was further divided in subsubsections to structure the method section more clearly. A brief extension was added the the subsection "2.2 EMAC model setup":

> *"The setup combines boundary conditions of 2050 surface emissions and nudging to present day meteorology."*

The quick-look table (Table 1), which was added as a result to RC1, was extended by two lines to include information on meteorology and surface emissions.

Satellite Validation

I don't see how comparing present-day observations with model runs which use future composition adds anything beyond the comparison of the same model with realistic present-day composition. If the only difference in the model is the source gas loadings then one has to say that the present-day model has been evaluated and you have just changed the boundary conditions. If you want to show the impact of the source gas changes in the model then that is a different issue.

The approach you suggest is certainly another viable option.

SWOOSH may be a long climatology but only 4 years are used (2013-2016). What are the main datasets in that make up SWOOSH in this period? A 40% or so difference in stratospheric water vapour seems very large to me. How does CH4 compare? What about total hydrogen (2CH4 + H2O (+H2?)) Is there an issue with the age of air (too much CH4, too little H2O) at a certain location. I don't think a 40% error in H2O can be ignored.

Information on SWOOSH data is presented in subsubsection 2.3.1. It is introduced that for the years 2013-2016 SWOOSH dataset consists of Aura MLS data. Additionally, a comparison to other instruments is included. In our opinion the information on SWOOSH is sufficient and does not require any extention. However, we are happy to add further information

While we find all the questions very interesting, we focused on the two most important climate drivers, O_3 and H_2O. In our opinion, everything beyond would require specific publication focused only on model validation. Please note that EMAC results show a systematic cold bias, and hydrogen or methane oxidation should not be the issue (Jöckel et al, 2016; Pletzer et al, 2022).

The referee is correct. 40 % sounds like a lot. However, previous publications show that model results agree well on water vapour perturbations in both, perturbation patterns and total magnitude (Pletzer et al, 2022; Kinnison et al, 2020). Following the comparison to satellite data, our viewpoint is that background water vapour is less important for the magnitude of middle atmospheric water vapour perturbations than compared to e.g. transport time to the troposphere. The latter was quantified in Fig. 8. It would be interesting to see similar results from other models.

Figures

There are too many figures (35!). I know that some are in an Appendix but they are referred to throughout the text as though they are main figures and not supplementary ones. In effect, the reader is reading a paper of 35 figures which is way too long. Some panels in the figures are small with small font size.

The panels in the figures were increased in size. This includes zonal mean figures (Figs. 3-7, 17, 18, 20-26) and figures on radiative forcing (Figs. 9, 28-30).

Tables

The tables need checking and tidying up. Table 2 is just explaining a legend which appears in later tables without any reference back to Table 2. It would be simpler just to put this code in the heading of e.g. Table 3. The second 'Magnitude' column of Table 1 must also be 'per year'? Why is there the need for the final three columns of Table 1 – they are all the same.

We extended the 'Magnitude' column of Table 2 (formerly Table 1) with '/year'. Thank you for pointing that out. We removed the final three columns.

The information in Table 3 (formerly Table 2) is also used in multiple figures and is therefore needed not only for Tables 4 and 5 (formerly Table 3 and 4). We added a reference to Table 3 in Table 4 and 5 (formerly Table 3 and 4).

Results

Please shorten and rationalise the text and figures that are used to communicate the main results. The description of the the modelled changes for different latitudes/altitudes can be covered quite concisely but the relevant mechanisms at work should also be discussed (e.g. HOx, NOx chemistry etc).

We very much agree with your viewpoint that the publication covers many aspects.

We would prefer to not focus the relevant mechanisms of HOx, NOx chemistry, since another publication already focuses very much on the chemistry (Kinnison et al, 2020).

Please see also discussion a the top of the reply concerning the length of the paper.

Literature references

[1]

P. Jöckel *et al.*, 'Earth System Chemistry integrated Modelling (ESCiMo) with the Modular Earth Submodel System (MESSy) version 2.51', *Geosci. Model Dev.*, vol. 9, no. 3, pp. 1153–1200, Mar. 2016, doi: 10.5194/gmd-9-1153-2016.

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D. Kinnison, G. P. Brasseur, S. L. Baughcum, J. Zhang, and D. Wuebbles, 'The Impact on the Ozone Layer of a Potential Fleet of Civil Hypersonic Aircraft', *Earth's Future*, vol. 8, no. 10, Oct. 2020, doi: 10.1029/2020EF001626.

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J. Pletzer, D. Hauglustaine, Y. Cohen, P. Jöckel, and V. Grewe, 'The climate impact of hydrogen-powered hypersonic transport', *Atmospheric Chemistry and Physics*, vol. 22, no. 21, pp. 14323–14354, Nov. 2022, doi: 10.5194/acp-22-14323-2022.