Authors Response

Dear Reviewer,

we would like to thank you very much for your analysis of our manuscript and broad understanding of the topic. Our answers are given in *blue and italic*.

Yours sincerely,

Johannes Pletzer

Review of "The Climate Impact of Hypersonic Transport" by Johannes Pletzer et al.

General remarks

This paper analyses the Climate Impact of Hypersonic Transport. Very few studies in this direction are available. Therefore such studies are needed and such results certainly constitute an important contribution to ACP. It is also good that two independent models are employed (and the results compared) as specific model features could impact the results of such model studies.
However, as the paper stands, I do not think it is very useful. My most important criticism is that the message (result) of the paper is not clear (see also below). Some issues (e.g. contrail formation by subsonic planes) seem to be overlooked and it is not clear to the reader how the "impact" is quantified and measured. I am also not convinced about the "first time that a recombination to H2O" is found (see equation 1), but such issues should be clearly worked out, not just mentioned in passing. (I note that the key reaction, equation 1, if I understood correctly) is not even explicitly mentioned in the manuscript.

We are thankful that the reviewer acknowledges the need of such studies and highlights the importance of the use of two independent models.
Message of the paper: This is an important point. It was not clear to us that our message was not conveyed correctly. The detailed changes to address this topic can be found below.

Contrail formation: We added another paragraph in the discussion subsection 7.1. The background is the following. A study by Stenke et al (2008) estimates the change in contrail formation and the change of contrail radiative forcing for subsonic and supersonic aircraft. According to the authors the change in contrail radiative forcing and total contrail cover is very small. They report a shift of contrail cover from mid latitudes subsonic cruise levels to low latitudes supersonic cruise levels (see enclosed Figure from that paper). For hypersonic aircraft this relation might be changed since those aircraft are flying above the tropical tropopause and hence do not form contrails in the tropics. Therefore, the replacement would probably lead to a reduction of contrail radiative forcing. See Figure 3 from Stenke et al (2008) showing the total contrail cover in percent for supersonic aircraft, i.e. S5 (right):

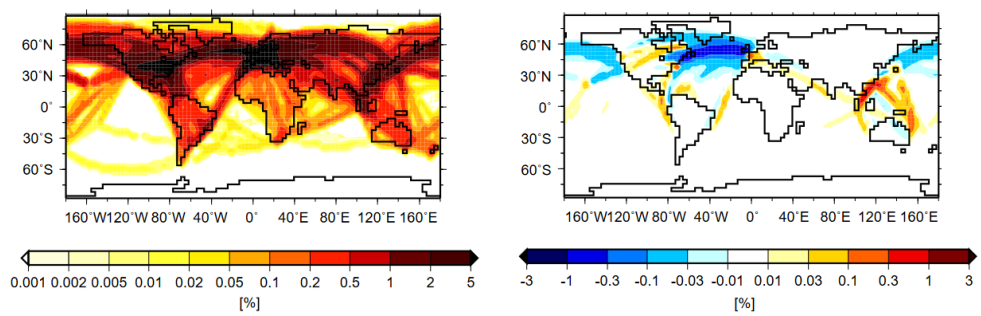

Fig. 3. Annually averaged total contrail cover [%] for the subsonic fleet S4 as simulated by ECHAM4 (left), and differences in total contrail cover [%] between S5 and S4 (right). The displayed differences are significant at the 95% level (t-test).

Very importantly, it seems that a misunderstanding exists: We certainly did not intend to claim to have found a new reaction for the recombination to H_2O. Instead we want to convey the overcompensation mechanism, counteracting to the expected photochemical H_2O loss as indicated by Brasseur and Solomon (production-loss). Hence the discussion of the rate of those reactions is the new finding that determines the perturbation loss time and not the reaction itself.

I am sorry for sounding so negative; I think a lot of work on the manuscript is required, but the topic is of great importance.

Comments

Message of this paper

It is not clear to me what the main message of the paper is. To me it seems that the question is the radiative forcing induced by a fleet of hypersonic planes because of the emissions of H2O they cause in the stratosphere. A lot of the discussion is along this argument. But then the abstract talks about depletion of the ozone layer (l. 14, but is this depletion in column ozone?) without addressing the processes (is the depletion caused by NOx or by HOx or both)? Is the depletion relevant because of UV issues (the ozone reductions seem small) or because of radiative forcing?

Thank you very much for this helpful comment. We revised the abstract accordingly to focus explicitly on the radiative forcing change.

"We find a 18.2 $\pm$ 2.8 mW m^{-2} and 36.9 $\pm$ 3.41 mW m^{-2} increase in stratosphere adjusted radiative forcing due to the two hypersonic fleets flying at 26 km and 35 km respectively. Ozone changes contribute 8.0-21.8 % and water vapour changes contribute 78.3-92.0 % to the warming on average."

The impact of water vapour and NOx emission on ozone has been extensively described by and focused on by Kinnison et al considering also their impact independently from each other. They showed that the NOx emission has a considerably larger impact on ozone than water vapour emission at altitudes from 30-40 km. In our publication, we focus on the radiative forcing changes, while addressing the similarities and differences between our model simulations and Kinnison et al. (e.g. subsection 7.1 "Atmospheric Composition Changes" and Fig. 6 "Photochemical water vapour lifetime"). The relevance of NOx or HOx in the depletion of ozone is indeed important and hence we dedicate another publication to this topic, focusing on altitude and latitude sensitivities.

According to the results from McKenzie et al (1991) one percent of ozone depletion cause approximately

1.26-1.40 % increase in erythemal irradiance (EI). For the average total ozone depletion this is equal to an increase of 0.10-0.12 % in EI for the ZEHST aircraft and to 0.20-0.22 % EI for LAPCAT.

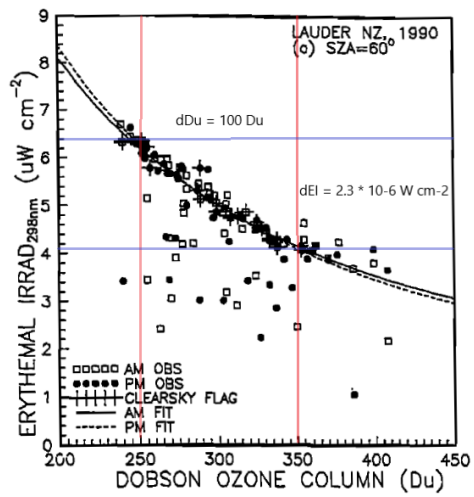

Fig. 4. From McKenzie et al (1991)

The climate impact is measured relative to subsonic aircraft (l. 16/17), but what quantity is used to calculate the relative impact? Is it radiative forcing? This should be clear from the abstract. Assuming it is radiative forcing does this forcing only consider the impact of CO_2 emissions by conventional subsonic aircraft? Such aircraft cause contrails (ice particles) which have a potential impact on radiative forcing (e.g., K˙archer, 1996) – has this effect been considered?

Thank you for pointing that out. We added the pieces of information "stratosphere adjusted radiative forcing" and "mean-surface temperature change" to the abstract and the introduction.

The relative impact is calculated using AirClim. The climate impact of subsonic aircraft includes contrail formation, CO2, H2O and NOx effects (short-lived ozone, primary mode ozone, methane). Further information is available from Grewe & Stenke (2008) and Dahlmann et al (2016). Note that also the efficacies of the individual species are taken into account as described by Dahlmann et al (2016) that are close to those of Lee et al (2021).

Further, there could be ways to manufacture carbon neutral kerosene like fuels (for subsonic aircraft as well as for supersonic and hypersonic aircraft) and of course a subsonic aircraft in the future which is fuelled by (green) liquid hydrogen is not unthinkable. I understand that this study cannot discuss all of these possibilities, but by making a particular choice, it rans into the danger of giving a biased comparison. And further (see below and the discussion in the manuscript) the lifetimes of radiative forcing of emissions of H_2O and CO_2 to the atmosphere are different (and depend very much on altitude in case of H_2O).

Clearly, your comment to use carbon neutral kerosene like fuels for high-flying aircraft is valid. We decided to focus on hydrogen powered aircraft for two reasons. First, current hypersonic transport projects focus, to our knowledge, on cryogenic propulsion. Second, kerosene fuel would reduce the emission of water vapour by a factor of three and would require a new design of propulsion technology and new aircraft design (larger take-off weight). These topics are being addressed in the EU-project MORE&LESS. Water vapour would most probably remain the main climate driver or one of the main climate drivers besides ozone and can be estimated with our radiative forcing sensitivity RF/Tg(H2O Emission).

A comparison of subsonic aircraft fueled with sustainable aviation fuel and hypersonic aircraft would result in a significantly larger ratio. First, because they would be both carbon neutral and, second, because the contrail radiative forcing is likely being reduced since sustainable aviation fuel (SAF) leads to lower particle number densities relating directly to fewer ice crystal number densities in the contrail and implying shorter lifetimes and smaller radiative effects.
Still it is a valid point and we changed the title of the publication to "The Climate Impact of Hydrogen Powered Hypersonic Transport" to avoid misunderstandings.

Carbon cycle

It is stated in the paper that "the CO_2 perturbation originating from fossil fuel is subject to a large variety of sinks with different lifetimes. In general, the range is approximated with 2-20 centuries, where most of the CO_2 climate impact is taken up by ocean and biosphere sinks and 20-35 % remain in the atmosphere for longer time . . . "

First, it should be noted that the CO_2 that remains in the atmosphere can only be really taken out of the system by sedimentation of carbon containing material to the ocean sediments on timescales much longer than centuries (100 000 years) and, second, the ocean uptake depends on the ocean circulation and ocean water chemistry (which is in the order of perhaps 5000 years) (Archer and Brovkin, 2008).

Your comment is very much appreciated. We already wanted to oppose the often-transported message that CO_2 has a lifetime of approximately 80-120 years, which is not true. That is why we addressed the long tail of CO_2 lifetime. Our description was vague with "for longer time". We happily extended the description with another sentence ("Hence, released CO_2 will affect climate for tens of thousands to hundreds of thousands of years").

Water vapour as a greenhouse gas

I think we all agree that water vapour is the most important greenhouse gas; it accounts for about half of the present day greenhouse effect and is the most important gaseous source of infrared opacity in the atmosphere. Tropospheric water vapour warms the climate and so does lower stratospheric water vapour (Solomon et al., 2010; Riese et al., 2012). However, it seems to be that an underlying assumption throughout the paper that stratospheric H_2O always warms. However, at higher stratospheric altitudes and especially in the tropics a unit mass increase in H_2O cools the climate (Riese et al., 2012). The authors might not agree with the results by Riese et al. (2012) but this aspect of heating and cooling should be discussed in the paper.

Thank you very much for the reference by Riese et al. Since the main mass perturbation of H_2O emission is accumulating in the lower stratosphere and only small mass perturbations appear in tropical or higher stratospheric regions (Fig. A2, Fig. A3), we, as a matter of fact, focus on the warming. We gladly agree to include the information stated by Riese et al to convey a complete picture to the readers. In our opinion the main picture remains the same. We additionally referenced the Figures on the mass change in the main text, which was missing.
We extended the text in the section "Radiation and Climate" with an introduction, which relates atmospheric composition to radiative forcing.

"The radiative forcing caused by atmospheric composition changes depends very much on location (Lacis et al, 1990; Riese et al, 2012). In our results, the largest water vapour concentration change appears at lower stratospheric altitudes and increases poleward (Fig. A2, Fig. A3). The differences between the ZEHST and LAPCAT scenario, except for magnitude, are small. Additionally, we describe the ozone increase in the lower tropical stratosphere, where the RF sensitivity and air density are larger (concentration changes not shown). Hence, according to (Lacis et al, 1990; Riese et al, 2012) we expect a warming for both ozone and water vapour changes."

Overall, I suggest that the background balance of water vapour through-out the stratosphere is considered (e.g., LeTexier et al., 1988; Brasseur and Solomon, 2005; Poshyvailo et al., 2018, and references therein), on top of which the impact of perturbations by the proposed fleet of hypersonic planes can be assessed.

The water vapour budget according to LeTexier et al (1988) has been re-evaluated with three different approaches using EMAC by Frank et al (2018). This includes simulations with full chemistry very similar to ours, which confirm the findings by LeTexier et al. The methane oxidation submodel that we applied in our setup (CH4), was developed by Winterstein & Jöckel (former Frank).

For completeness, we address the background balance of water vapour in the revised introduction (paragraph line 50).

"The middle atmospheric balance of water vapour is determined by methane oxidation, photochemical lifetimes of HOx compounds and tropical upward transport, which is limited by the coldpoint temperature (LeTexier et al, 1998; Brasseur, 2005; Frank et al, 2018). Polar dehydration by polar stratospheric clouds and the sedimentation of the particles contribute to the balance."

In our opinion the UTLS region is a special case, since large uncertainties remain that projects like TPCHANGE are trying to address. We used the model evaluation with IAGOS to assess our models performance in the extratropical UTLS. Further we addressed the variability of upper tropospheric water vapour and included an upper estimate of radiative forcing based on the variability (Tab. 7).

Water vapour production in the stratosphere

The authors state in the abstract that "H2O depletion at high altitudes is overcompensated by a recombination of hydroxyl radicals to H2O . . . " and later in the paper state: "Opposite to the expected removal of H2O emissions, we found a before unknown net-recombination of H2O" (l. 391). I think that they are referring to the reaction

$$HO2 + OH \longrightarrow H2O + O2 \quad (1)$$

First, given that this seems to be a major issue for the paper I suggest explicitly stating the reaction. Second, the water vapour production in eq. 1 is known (see, e.g., eq. 5.105 in Brasseur and Solomon, 2005). So I am not sure what "before unknown" means here. Finally, I agree with the authors that the lifetime of water vapour decreases with altitude, but of course water vapour concentrations are a balance of loss and production terms (like it is the case for other species), so I am also not sure about "expected removal of H2O emissions".

- *We added the reaction right before Fig. 7.*
- *The water vapour production is known in literature and has of course been implemented in both models, EMAC and LMDZ-INCA for a long time. Thank you for pointing out that we did not state the reaction explicitly. We want to convey that the overall net-recombination as a follow up to hypersonic emission was not known before. We highlight three processes, increased methane oxidation, increased nitric acid oxidation, and HOx recombination to explain the overcompensation. Expectations for the climate impact in regard to the photochemical destruction of water vapour were published before (Steelant et al, 2015). Here, we want to address researchers with aircraft design background in addition to researchers on atmosphere with a broader knowledge of atmospheric processes.*

Former: „ Opposite to the expected removal of H2O emissions, we found a before unknown net-recombination of H2O. Both models show an increase in H2O perturbation lifetime and H2O perturbation at the higher altitude and this analysis indicates that a net-recombination and enhanced methane depletion is overcompensating the H2O destruction. Our finding is robust with good agreement between the two models."

New: "The photochemical depletion of H_2O and shift to H_2 concentrations (e.g. Fig. 5.23, p. 312, Brasseur, 2005) clearly has no large effect at these emission altitudes. So instead to the expected removal of emitted H_2O by photochemical depletion, we found a before unknown importance of the reaction rates of the net-recombination of H_2O based on HOx recombination and an increased methane and nitric acid oxidation. Both models show an increase in H_2O perturbation lifetime and H_2O perturbation at the higher altitude, which is further increased by the net-recombination, i.e. overcompensation of photochemical depletion. Our finding is robust with good agreement between the two models."

The debate on supersonic transport

I realise that this is not a historical paper and I see that some mention has been made of earlier projects (e.g. COMESA). However, I suggest looking back a bit to the issue of supersonic transport (which is indeed discussed again today, see some of the citations in this paper). But in the seventies a controversy about supersonic transport had started in the United States. At that time, large fleets of stratospheric supersonic aircraft were planned (US: Boeing, Britain/France: Concorde, Soviet Union: Tupolev) and a fleet of 500 supersonic planes seemed a reasonable estimate. It is interesting to note that the concern was an enhanced catalytic ozone destruction; originally ozone destruction by OH and HO2 radicals (resulting from the release of water vapour in the engine exhausts, like discussed in this manuscript for hypersonic transport) was considered, but it was soon realised that the catalytic destruction of ozone by NOx posed a much greater threat to the ozone layer (Johnston, 1971; Crutzen, 1972). Indeed this issue was part of the motivation of Crutzen (1970) to investigate the impact of NOx on the ozone layer. Perhaps some effort to touch upon this history might be helpful to the paper.

We thank you very much for the additional information on the effect of NOx on the ozone layer. We included these for the readers next to the list of overview-publications in the introduction (line 43).
We do not differentiate between emission like Kinnison et al or others. We rather look at the combined effect of H_2O, H_2 and NOx emission in this publication. Clearly, it is of interest to quantify the different sensitivities. However, the individual effects of emission are presented in a second publication, where impact of NOx emission, H_2O emission and H_2 emission are looked at independently (Pletzer et al, in prep.).

Some details

- l. 3: it would be helpful to give approximate numbers for these emis-
  sion.
  Since we introduce only the concept of hypersonic aircraft, we would prefer to refrain from giving pieces of information here that depend on aircraft design and are based on multiple aircraft fleets with different ranges of emissions.

- l. 6: if 15 km is in the tropics, months seems rater long, on the other
  hand months is short for emissions at (say) 30 km in the stratosphere.
  Perhaps one could be a bit more specific here.
  Former: "While H_2O that is emitted near the surface has a very short residence time (hours) and thereby no considerable climate impact, super- and hypersonic aviation emit at very high altitudes (15 km to 35 km), with residence times of months to several years, and therefore the emitted H_2O has a substantial impact on climate via high altitude H_2O changes."
  Now: "H_2O that is emitted near the surface has a very short residence time (hours) and thereby no

considerable climate impact. Super- and hypersonic aviation emit at very high altitudes (15 km to 35 km) and water vapour residence times increase with altitude from months to several years with large latitudinal variations. Therefore, emitted H_2O has a substantial impact on climate via high altitude H_2O changes."

- l. 8: I would not include (potential) speculations in the abstract – concentrate on the new findings of the paper.
  Thank you for sharing your impression. We changed the sentence accordingly.

- l. 350: what about the loss of H2O in the Antarctic stratosphere in winter (e.g., Kelly et al., 1989; Poshyvailo et al., 2018) could this loss process be of relevance for the considerations here? Is it implemented in the models?

  Since water vapour perturbations are low in the Antarctic, we do not expect a significant influence there. Polar stratospheric cloud chemistry is calculated in both models and contributes to not only dehydration, but denitrification as well. For EMAC, temperatures in the Antarctic polar stratospheric cloud environment are well represented. The simulated Antarctic polar vortex was relatively weak with a warm bias in former versions, but has been improved by changing the parameter that manages momentum deposition in the stratosphere and mesosphere by non-orographic gravity waves (Jöckel et al, 2016).

  We added the sentence to line 350: "Additionally, polar stratospheric clouds cause dehydration through sedimentation."

- l. 356: Here you say that higher altitudes have a negligible effect on the mass perturbation, but in l 361 you say that the "H2O mass perturbation is approximately twice as large for the higher flying aircraft compared to the lower flying aircraft. . . " – isn't this a contradiction? I think this could be better explained.
  Thank you for pointing that out. We slightly altered the sentences (changes underlined) in the first (line 356) and second paragraph (line 361) to avoid a mix-up with the previous paragraph.

  L 356: "The latter <u>altitude range</u> <u>contains only a small amount</u> of the mass perturbation in the models, since the largest mass perturbation <u>accumulates</u> in the middle and lower stratosphere, where air density is larger"

  L 361: "Absolute values of the mass perturbation and the respective <u>perturbation</u> lifetime of water vapour are listed in Table 3. Values were calculated for the perturbation above the tropopause (WMO, 1957). The <u>total</u> H2O mass perturbation is approximately twice as large for the higher flying aircraft compared to the lower flying aircraft for each model <u>due to the longer transport to the troposphere and due to the larger emission.</u> The perturbation lifetime clearly increases with <u>cruise</u> altitude from 2.8-3.5 years to 4.2-4.6 years."

- l. 370, Fig 6: here and elsewhere: H2O should not be in italics in the figures.
  H2O was changed accordingly in Fig. 6, Fig. 3, Fig. 8, Fig A2.

- l. 424, Table 4: ozone in percent; do you mean total ozone here? Total ozone change
  We are referring to the relative total mass change of ozone in %. We changed the caption accordingly.

"Relative total ozone mass change for each model and each aircraft fleet."

- l. 522, Fig. 12: The figure shows an enhancement factor, which is not explained in the caption. No unit is given. However, a little below (l. 532) a unit is given in the text, and the values are compared to Fig. 12: I find this hard to follow.
  The enhancement factor is unitless, since it is a ratio of radiative forcings from supersonic to subsonic aircraft. Clearly, the term used in l. 532 is not correct, since it is the normalized near-surface temperature change. The enhancement factor is the 61 in the next sentence. We added the information, that the enhancement factor has no unit in line 510-511 and corrected the sentences in line 532.

- l. 575: This is not the most recent edition of this book; see Brasseur and Solomon (2005).
  We updated the reference, the Fig. reference and the page number.

- l. 617: How is this reference available?
  The reference is available via ls@vki.ac.be or secretariat@vki.ac.be. The bibtex data was updated accordingly.

- l. 634/637: Journal missing?
  Journal was listed in the bib file. It is present now in the pdf as well.

References

Frank, F., Jöckel, P., Gromov, S., & Dameris, M.: Investigating the yield of H_2O and H_2 from methane oxidation in the stratosphere, Atmospheric Chemistry and Physics, 18, 9955–9973, doi: 10.5194/acp-18-9955-2018, URL https://www.atmos-chem-phys.net/18/9955/2018/ (2018)

Winterstein, F. & Jöckel, P.: Methane chemistry in a nutshell – the new submodels CH4 (v1.0) and TRSYNC (v1.0) in MESSy (v2.54.0), Geoscientific Model Development, 14, 661–674, doi: 10.5194/gmd-14-661-2021, URL https://gmd.copernicus.org/articles/14/661/2021/ (2021)

McKenzie, R. L., Matthews, W. A., & Johnston, P. V. The relationship between erythemal UV and ozone, derived from spectral irradiance measurements. Geophysical Research Letters, 18(12), 2269-2272, https://agupubs.onlinelibrary.wiley.com/doi/abs/10.1029/91GL02786 (1991).