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 H_2O” 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.

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 H_2O 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 NO_x or by HO_x or both)? Is the depletion relevant because of UV issues (the ozone reductions seem small) or because of radiative forcing?

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., Kärcher, 1996) – has this effect been considered?

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).

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).

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.

Overall, I suggest that the background balance of water vapour throughout 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.

Water vapour production in the stratosphere

The authors state in the abstract that "H_2O depletion at high altitudes is overcompensated by a recombination of hydroxyl radicals to H_2O ..." and later in the paper state: "Opposite to the expected removal of H_2O emissions, we found a before unknown net-recombination of H_2O" (l. 391).

I think that they are referring to the reaction

$$HO_2 + OH \longrightarrow H_2O + O_2 \tag{1}$$

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 H_2O emissions".

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 HO_2 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 NO_x 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 NO_x on the ozone layer. Perhaps some effort to touch upon this history might be helpful to the paper.

Some details

- l. 3: it would be helpful to give approximate numbers for these emission.
- 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.
- l. 8: I would not include (potential) speculations in the abstract – concentrate on the new findings of the paper.
- l. 350: what about the loss of H_2O 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?
- l. 356: Here you say that higher altitudes have a negligible effect on the mass perturbation, but in l 361 you say that the "H_2O 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.

- l. 370, Fig 6: here and elsewhere: H_2O should not be in italics in the figures.
- l. 424, Table 4: ozone in percent; do you mean total ozone here?
- 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.
- l. 575: This is not the most recent edition of this book; see Brasseur and Solomon (2005).
- l. 617: How is this reference available?
- l. 634/637: Journal missing?

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