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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The response of stratospheric water vapor to climate change driven by different forcing agents" *by* Xun Wang and Andrew Emory Dessler

Anonymous Referee #2

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This is a useful and mostly clearly written analysis of the behavior of stratospheric water vapor (SWV) in a set of idealised multi-model (PDRMIP) climate simulations. Subject to satisfactory modification it should become acceptable for publication. Some of the suggestions below are discetionary, in my view, but I feel acting on them would lead to a more valuable paper.

GENERAL COMMENTS

1. The paper largely focuses on interpreting the multi-model responses. While this is of course useful, it stops short of relating the new understanding to any real-world changes in SWV. Indeed, a reader could incorrectly infer (by omission) from the ab-

stract and conclusions that BC is the dominant (fast) driver of SWV changes, as no mention is made of the idealised nature or size of the perturbations.

I feel this is a significant weakness and I encourage more discussion. How much does this work help in understanding past and possible future SWV changes? Is it possible (as has been done in some of the PDRMIP papers such as Samset et al. 10.1038/s41467-018-04307-4 and the Hodnebrog paper referred to below) to make a suitable composite of the individual perturbations to derive responses that have or may in future occur? This would require some additional critical assessment of the relevance of the PDRMIP perturbations to the real world. In particular, note there has been some discussion of how the PDRMIP BC perturbations compare to observations (Allan et al, https://doi.org/10.1038/s41612-019-0073-9). I see that Allan et al's supplementary figure S5 shows the impact of BC on UTLS temperature changes in the PDRMIP models, and that needs mentioning here, in the context of the BC conclusions in this paper. Similarly, the CFC perturbations are much higher than any present or likely future changes.

2. As noted in the specific comments, I feel that there is inadequate recognition that some of the results presented here are also presented, either explicitly or implicitly, in some earlier papers from the PDRMIP group – this is particularly so for the ERFs where no reference to, or comparison with, those earlier results, is given.

SPECIFIC COMMENTS (* = more major)

14: This conclusion is specific to the TLS

16: "becomes weaker at higher altitudes and at higher latitudes below 150 hPa." This is a bit ambiguous. Does this means heights at pressures below 150 hPa or heights below the height of the 150 hPa surface. These would have opposite meanings.

47-48: There is a slight overlap between this submitted paper and the paper published in ACP by the core PDRMIP team – Hodnebrog et al: https://doi.org/10.5194/acp-

19-12887-2019, which is not cited here. There is very little discussion of stratospheric water vapor in that paper, but nevertheless effects are implicit in some figures (including in the Supplemental) and so it should be referred to at appropriate points in the paper. In addition, a reader may wonder about the tropospheric wv response in PDRMIP models, and so it would be beneficial to point to that paper for that reason.

57: Presumably the 3xCH4 experiments have no resulting change in SWV due to the oxidation of additional methane?

*90 and many other places: There are repeated statements that there is no surface temperature response in the fixed SST runs, but this is not correct, with implications for the definition of ERF. See for example the fast surface temperature response in Smith et al. 2018, the alternative definitions of ERF in Richardson et al. 2019, and other PDRMIP papers.

129: I think it is necessary that a comparison of ERFs (and the associated feedback parameter) with Richardson et al. (including for the CFCs and N2O in their supplement) is presented both to confirm they are in reasonable agreement and also to make clear that the ERFs derived here are not original work with the PDRMIP output.

139: “tend to be larger” Isn’t it clearly larger?

*148 and throughout: Rather little is said about intermodel differences. For example, on HADGEM3, more discussion of its apparent outlier status on some plots seems necessary. The text says it is “likely connected” to the larger surface warming, but it seems the climate sensitivity is about double the multi-model average but the slow SW response is around a factor of 4 larger. Is that because the TTL temperature change is 4 times higher (per unit ERF)? Another example is that apparently half the models have a slow SW response to BC of the opposite sign (Fig 1a) to the multimodel mean. Is there any obvious reason why? As far as I can see BC causes a warming in all models. One thing I miss from this study, and encourage the authors to look at if they have the resource, is the degree to which the model’s background climatology of stratospheric

water vapor or TTL temperature could explain some of the intermodel differences.

156: Is this linear regression done once across all simulations and all perturbations. If not, I am unclear which perturbations have been used for the regression.

159: This is a relatively short paper and I wondered whether the supplementary figures could be brought into the main text?

171-172: This repeats a point already made at 141-142.

201: I am sorry if I miss it, but I see very little discussion of stratospheric temperature changes in the Jain et al paper. The role of CFCs on the vertical profile of temperature can be seen in many papers such as Forster et al. https://doi.org/10.1007/s003820050182 and Forster and Joshi 10.1007/s10584-005-5955-7 (by the way, I am not Forster!)

*204: This statement on shortwave radiation is strange. There may be a small shortwave effect from the reduced reflected flux from the troposphere, but there is a long history of simulations that clearly attribute the stratospheric cooling due to increased tropospheric ozone to the decreased upwelling thermal infrared radiation. E.g. Ramaswamy and Bowen https://doi.org/10.1029/94JD01310, Berntsen et al https://doi.org/10.1029/97JD02226 and the Forster et al. paper referred to above.

206: “Tropospheric O3 is also transported”. As I understand it, ozone is imposed in the models and not advected. I don’t know what this sentence means.

212: “larger than 50%”. CAM5 and MPI-ESM look less than 50%?

*248: Returning to General Point#1, the Summary feels a very mechanical repetition of the results in the paper without any discussion of the wider implications, remaining uncertainties, or possible future avenues/priorities for improving understanding.

273: Strictly Fig 5 refers to TLS only

519-520: I think the markers are only reported when there are more than 3 contributing models?

Typo

46L “responses” -> “responds”

Throughout: This may be common usage, but the paper refers thoughout to the ensemble mean when other papers would refer to its as the multi-model mean (ensemble could refer to different runs from the same model with perturbed initial conditions or physics). I dont have a strong opinion on this.

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