Response to reviewers

Alison Ming, Amanda Maycock, Peter Hitchcock and Peter Haynes

March 6, 2017

We would like to thank both reviewers for their detailed comments which have helped us to improve the paper significantly in revision. We have implemented many of the suggestions and provided detailed responses when we have not. In light of the comments from both reviewers about the length of the manuscript, we have revised the text to make the paper more concise.

Page and line numbers quoted in the responses refer to the updated text.

Below we indicate how we have dealt with each individual comment.

Reviewer 2

> Abstract: I think the abstract could be somewhat condensed, and (i) more quantitative information could be given; (ii) the text switches back and forth between amplitude, phase, ozone, water and temperature (e.g. around Lines 6-12), which is confusing.

The abstract has been revised to discuss the ozone annual cycle first then the water vapour.

> P1/L7,8: "... and out of phase with the observed annual cycle of the tracer mixing ratio."
> P1/L8: Suggest: "The ozone contribution calculated here is ..."
> P1/L9: Suggest to replace "This difference" with "The difference"; but see general comment on Abstract above.

These suggestions have been taken into account in the revised text.

> P2/L20: Suggest (the tropopause temperature is important for stratospheric water, not dehydration per se.) "Therefore the temperatures at 90hPa and 100hPa shown in Figs 1(c/d) are more directly relevant to water entering the stratosphere than the temperature at 70hPa."

This is a good point and the sentence has been changed.

P4/L26: Quantitative details for H2O missing in previous work. Fueglistaler et al. state that their calculations “show that the temperature adjustment for the annual cycle in water vapour around 70 hPa is about an order of magnitude smaller than that for ozone.” At the time of writing Fueglistaler et al., this seemed to be sufficient information. However, in the context of your ozone effect being substantially larger than theirs, it is interesting to note that their water vapor impact (around 0.2K) is also smaller than your water vapor impact (your Figure 5b gives 0.6K). I think it would be great if your paper could resolve these quantitative differences better, such that your paper can be cited as the best quantification of the radiative effects. The Fueglistaler et al. numbers are taken from the calculations with the Edwards-Slingo code, but results with the Fu-Liou code were rather similar (see Fueglistaler et al., page 3702; last paragraph). The calculations using the Fu-Liou code gave slightly larger amplitudes, but the differences were much smaller than the differences to your calculations; at the time, we decided to go with the Edwards-Slingo results as they were obtained from a software configuration that has been used in previous studies by Piers Foster. It would be great if you could resolve the questions you discuss on page 10/11 regarding the role of the radiative transfer code versus trace input data -the ozone and water vapor files used by Fueglistaler et al. for the calculations with the Edwards-Slingo and Fu-Liou code can be made available.

We have performed a number of SEFDH calculations that try to narrow down the causes of the differences between our calculation and Fueglistaler et al. and whilst we have not been able to explain the differences, the main effect is likely to be differing ozone climatologies rather than radiation code. The temperature change was more sensitive to the background ozone field than any other changes we tested. For instance, an SEFDH calculation with the RRTM radiation code and SWOOSH ozone obtained similar results to those in this manuscript.

We suggest that future studies, as far as possible, make the actual fields of ozone and water vapour used in the radiative calculation publicly available, to make comparison more straightforward.

Finally, can you quantify the impact on the global average temperature? An important point of Fueglistaler et al. is that one should not assume that the global average temperature remains constant (as e.g. implied in Yulaeve et al. 1994); it would be interesting to see the numbers from your calculation. You do discuss the role of extratropical heating rate variations on the tropical amplitude, and it appears to me it that would be fairly straightforward to also show the annual mean cycle of global average temperatures.

The plot below shows the global averaged temperature in the IGCM model calculation and ERA-Interim. This supports the point that the global average temperature is not constant with a peak to peak amplitude of about 1.4 K in the case of the ozone and water vapour perturbation. The 20° N-S averaged for this calculation was about 2.8 K.

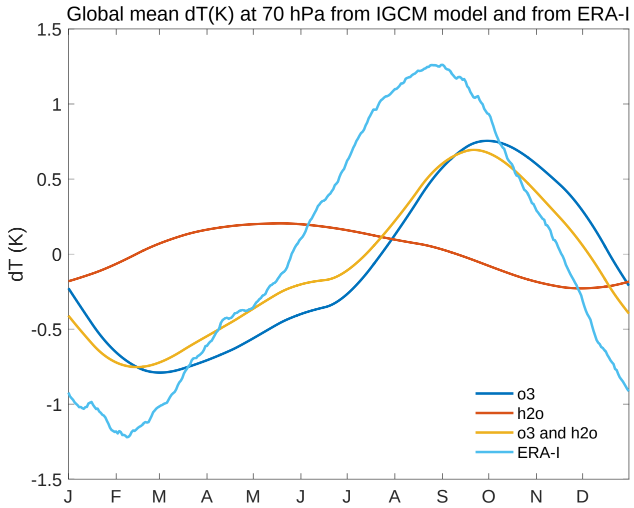

Figure 1: Globally averaged temperature at 70 hPa in the IGCM model calculations with ozone and water vapour perturbations. The ERA-Interim plot is also shown.

We have considered including a mention of the global average but having significantly reduced the length of the paper, there did not seem an appropriate location for the detailed discussion that this topic would require.