Response to reviewers

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We are grateful to the reviewer for the additional comments and suggestions. We have revised the wording of the text in many places to be more precise and have addressed the various comments as follows:

> 1) In the response document, there are now statements about the IGCM calculation having potential differences 'arising from implementation rather than dynamical adjustment'. This was not mentioned in the original manuscript. What causes potential differences in 'implementation? Does this remain a potential source of error in the revised draft, and how well quantified is it?

The differences in implementation were found after careful comparison of the SEFDH and IGCM calculations that was prompted by the reviewer's previous comments. We therefore made changes to the implementation of the radiation code so it was directly comparable to the SEFDH version. One such change was to calculate the diurnally averaged shortwave in the same way in the two calculations and another was to change the way in which the trace gas concentrations were provided to the radiation code in the IGCM. As we explained in our previous responses, Figs 10c and 10e were included in the revised version to demonstrate explicitly that the differences between the dynamical calculation and the SEFDH calculations could be explained by the effect of dynamical heating, rather than being the result of differences in the detailed implementation of the two calculations. There are small differences between the two figures but the overall agreement is very good. Therefore this does not remain a potential source of error.

The revised paper explained this and contained the sentence

"The resulting temperature response shown in Fig. 10(e) is a very good match to the difference in temperature in Fig. 10(c)."

– for clarity we have further modified this to (page 19, line 8)

"The resulting temperature response shown in Fig. 10(e) is a very good match to the difference in temperature in Fig. 10(c) and reassures us that the difference between SEFDH and dynamical calculations can indeed be interpreted as resulting from the effect of dynamical heating and is not due to differences of detail in the implementation of the two calculations."

Once again we are grateful to the reviewer for focusing our attention on this point in her comments on the ACPD version of the paper.

> 2) In the response document and in the manuscript, it is stated that since the results reproduce the annual cycle then the net effect of clouds must be small. The response document refers to 'rough quantitative estimates to back this up, but that is quite loose language. How rough? How quantitative? It is later

stated that no results regarding this concern will be given in the draft because 'the problem is sufficiently complicated that a brief explanation could not do it justice and the scientific uncertainties are large'. So the reviewer is first presented with an assertion that the effect of clouds must be a small one (because the seasonal cycle looks good without it), and second a statement that the uncertainties are large. This is not an appropriate response. Getting the 'right' answer for the annual cycle can happen for wrong reasons, and the assertion that clouds are not important even though uncertainties are large is speculation. Particularly given the potential for cloud feedbacks in a dynamical calculation to affect radiation (e.g., through changes in cirrus in regions of altered upwelling, although that is only one example among many), I think further information is required beyond the statements made that this is 'beyond the scope of this work'. The paper needs to be revised to address this more clearly.

We have removed the sentence that suggests "the net effect of clouds must be small". Instead we have added the following paragraph to the discussion to clearly state that a full quantification of the cloud effect will require further work (page 23, line 8):

"All of the calculations make use of a clear-sky assumption. A rough SEFDH calculation taking into account an estimate of the annual mean climatological high cloud cover shows that the peak-to-peak annual cycle temperature change due to ozone at 70 hPa decreases by 5-10% at all latitudes between 20° N and 20° S. The effect on the water vapour annual cycle at the same level is negligible. The clouds lead primarily to a reduction in the amount of upwelling longwave radiation reaching 70 hPa of about $0.05\,\mathrm{K\,day^{-1}}$ which in turn decreases the ozone temperature response. A full assessment of the cloud effect is beyond the scope of this work and further work is needed to establish their precise contribution."

3) I did not find the authors response to my question 7) to be sufficient. I am asking about the mean meridional circulation in this representation of the model (as implemented, which could differ from other uses of the IGCM that may be discussed in the literature). Since the paper seeks to calculate thermal responses associated with imposed constituent changes, it seems to me to be quite important to determine whether or not the mean meridional circulation that is found in this representation of the model prior to imposing the perturbation is accurate. One approach that can be used is tracer transport tests, for example. I dont find the statement in the paper regarding the difference of 2K from a control run in the tropical stratosphere to be proof that the reader should be confident that an accurate balance exists in this representation of the IGCM model between radiation and vertical motion near the tropical tropopause. Both of these terms are known to be important drivers of temperature seasonal cycles in the region of interest, and 2K would be a very important change if it occurs close to the tropical tropopause. A 4 year long 'control run' is carried out and results for the perturbed versus control are shown for a 5th year, but the rationale for a 4-year

control is not given when it is introduced, and there is no discussion of how the model behaves over the 'spin-up' period in this configuration compared to IGCM 3.1. If the spin-up produces a mean circulation that is not realistic, and differs from the parent IGCM, that raises concerns about the experimental design. My concern here has to do with more clarity on how the models 2-D representation performs, particularly given the focus on specific latitudinal structures that are highlighted. These may well be correct, but the paper needs more clarity on its underlying methodology.

To recap, in this work we are considering how part of the dynamical feedback (the part represented by zonally symmetric dynamics) affects the temperature response to the radiative effects of imposed annual cycles in trace species. We do this by comparing a control configuration, in which there is no imposed annual cycle in trace species, with a perturbed configuration, in which the imposed annual cycle has been added. The reviewers questions have in essence focused on the response we deduce and how our conclusions depend on the control configuration of the model. The key issue here is not the control configuration itself, but whether differences in the control configuration (for example, the 2K difference between the control configuration described in the paper and ERA-I annual temperatures) lead to substantial differences in the response.

We had investigated this sensitivity previously – statements such as 'differences from the ERA-Interim annual mean are small (less than 2K in the tropical stratosphere) and do not affect the results presented below' included in previous versions of the paper were based on results from sensitivity experiments, and we have investigated it further at this stage by including a more realistic upwelling in the model control state. Once again our finding is that differences in the temperature response are small, e.g., a maximum of 0.2K, and that our characterisation of the latitudinal structure of the response continues to apply.

To ensure that our approach and the assumptions within are as clear as possible we have further revised the text of the paper (page 17, line 31) and we hope that this further addresses the reviewers concerns (e.g., over the need for a spin-up period in the control and perturbed simulations).

4) I appreciate the added appendix C but remain concerned about the discussion of uncertainties in SWOOSH ozone, and the authors' response to my criticism. The authors use the interannual standard deviation of SWOOSH values in providing tests in Appendix C. But the variability from year to year is not a measure of absolute accuracy. This should be explicitly stated. Further, the authors quote Tummon et al. (page 29, line 30) regarding the spread among satellites in the lower stratosphere. But it is clear that the uncertainties in the region of interest here, the lowermost stratosphere in the tropics, are considerably greater than global average uncertainties, in part because ozone is so much less abundant in the tropical lower stratosphere than globally. In the response to my comment, the authors assert an uncertainty of 10% at 70 hPa

and 30-40% by 130 hPa and refer to Davis et al. I could find no such statement for the tropics in the Davis et al. paper. Again, I request the authors to clarify where specifically this estimate comes from, whether it is a value for the tropics or simply a global average and therefore probably too generous in the tropics. Please give uncertainties appropriate to tropical latitudes and say where they came from.

Two sets of values presented in the paper are intended to give the reader a sense both the precision and the accuracy of the dataset and are appropriate to tropical latitudes. Appendices B and C have been edited to add details to clarify the calculation being done.

Precision: The 10% at 70 hPa and 30-40% by 130 hPa is not in the Davis et al paper but inferred from the SWOOSH dataset following the treatment of uncertainties as described in Davis et al. and provided by the SWOOSH dataset. The method by which these are calculated are detailed in Davis et al. (2016). This is an estimate of the precision of the dataset and is in line with similar estimates of precision at these levels in AURA MLS satellite data in the tropics (Livesey et al., 2017).

Accuracy: The tests in Appendix C are illustrative calculations and this is mentioned in the text. These values are taken as the interannual standard deviation of SWOOSH values to obtain a meaningful ozone perturbation in a way such that the calculations are reproducible. The variability is not an measure of accuracy but the range of values used reflects the spread in the accuracy of the SWOOSH dataset. This is comparable to the AURA MLS estimate of the accuracy compared to tropical sonde data given in Table 3.18.1 of Livesey et al. (2017) (Also, Sean Davis, pers. comm.).

5) page 5 and elsewhere. Temperatures are taken from ERA for 1991-2010, but ozone and water vapor from SWOOSH are for 1984-2015. Why the inconsistency? Why not use 1991-2010 from SWOOSH?

The time series is long enough to produce reasonable climatologies. E.g., the maximum differences between using a climatology from 1991 to 2010 for both ozone and water vapour from SWOOSH and the 1984 to 2015 SWOOSH climatology are of an order of magnitude smaller than the amplitude of the annual cycle perturbations in the region of interest.

6) Page 5, lines 28-31. The assertion that reproducing the annual cycle means cloud effects must be small should be removed for the reasons discussed above.

This comment has been addressed together with point 2) above.

> 7) page 6, line 12. What is meant by 'accurate convergence' and how was this determined? Loose language detracts from the paper in several places; this is an example but I suggest that the authors try to tighten up throughout.

The meaning of 'Accurate convergence' is explained in Appendix A and a reference to this appendix has been added to the main text (page 6, line 5).

> 8) page 8, line 12-16. I think 10% is far smaller than can be defended as a 2-sigma absolute (not interannual) uncertainty for tropical lower stratospheric ozone. This sentence is not useful or needed, just delete it.

We are arguing, together with the comment above to point 4), that 10% can be defended as the 2-sigma precision of the dataset for the annual cycle variation in ozone at 70hPa. We are not attempting to quantify exactly the absolute uncertainty of the dataset but an indication of this can be obtained from comparisons to the accuracy of the various satellite instruments. To address this, we have instead presented a range of calculations in Appendix C.

> 9) page 8, line 23. You quote 60% from one region and 30% from another. Where is the remaining 10%? Is there a 40% total contribution when additional higher levels are considered?

There is an 8% contribution from the region between 30 to 50 hPa region with the remainder coming from the remaining region. The text has been edited to add this (page 8, line 15).

> 10) Pages 6-10. The paper uses loose and inconsistent language in deciding when there is a 'significant' non-local contribution for water vapor versus ozone. In the case of water vapor, on page 10, line 7, an 0.4K non-local contribution for water vapor out of 1.1K (i.e., 36%) is deemed 'significant', while on page 8, line 22, a 30% contribution from ozone from higher altitudes is simply dismissed (with 10% unaccounted for), and it is stated here and elsewhere that

> the ozone response is primarily local. Please do account for the missing 10% by examining your calculations rather than leaving this hanging. In any case, even without the extra 10%, it looks to me like the two are comparable, and this should be stated. I suggest that the word 'significant' may be best reserved for things tested statistically, not those where the authors view is expressed. The paper should avoid loose judgments on what is 'significant' and what is not, and make consistent comparisons throughout. The question of non-local effects for both ozone and water vapor comes up in several other places in the paper, and needs consistent propagation in all its places of occurrence.

The 30% contribution to the temperature at 90hPa from the ozone annual cycle in the region between 80 to 50 hPa is indeed significant and has not been dismissed. The remaining contribution has now been accounted for in the edited text. It is expected that there will be a substantial contribution from the region above 90hPa to the temperature change there since there is a large annual cycle of ozone in this region.

The distinction being made between ozone and water vapour relates to the regions where these trace gases cause their largest temperature response. For ozone this region is located around 70hPa and for this level, 80% of the temperature annual cycle due to ozone comes from ozone variations local to this region. In contrast, temperature due to water vapour maximise around 90hPa and only 60% of this comes from the water vapour in the 100 to 80hPa region. This has been clarified in the discussion (page 22, line 10).

When comparisons are made, we have also replaced the word 'significant' with the word 'substantial'.

> 11) Page 11, line 6. You dont compare your findings regarding non-local effects to Gilford and Solomon. That should be done here, since it was a key focus of that paper.

We have added a comparison to the GS2017 results and added the following to the text (page 11, line 25):

"Gilford and Solomon (2017) find a response to water vapour changes with a peak-to-peak amplitude of 0.6 K at 70 hPa, 0.9 K at 85 hPa and 0.5 K at 100 hPa. These values are smaller than the amplitudes (respectively 0.9 K, 1.1 K – for 90 hPa and 1.0 K) we report above, particularly at 100 hPa but the difference may be in part explained by the fact that our calculations include water vapour variations down to 130 hPa. When, following Gilford and Solomon (2017), we include water vapour variations only above 117 hPa, we obtain peak-to-peak amplitudes of 0.8 K, 1 K (for 85 hPa) and 0.8 K respectively, closer to their results."

12) Page 14, lines 9-13. This statement first critiques Randel, then speculates as to why his results may differ, then admits to substantial uncertainties underlying that criticism. Avoid loose speculation in critiquing other work, delete this.

The statement is not aimed at critiquing Randel, rather we are trying to compare our calculation to theirs and point out physical reasons why our results differ. We identify in this section the importance of a tropospheric constraint, the effects of which were implicitly included but not explicitly identified by the Randel et al. paper. The text has been modified as follows to make this clearer (page 14, line 8):

"Randel et al. (2002) inferred damping time scales from the cross-correlation between the annual components of analysed $\overline{T}$ and $\overline{w}^*$, which implicitly includes non-local effects such as those of non-radiative processes operating in the upper troposphere. This is also true of the supporting radiative calculations they performed on the basis of observed temperature anomalies. As demonstrated below, the tropospheric processes have a substantial effect on the relaxation of temperature anomalies even in the lower stratosphere, in part because of the strong dependence of radiative timescales on the vertical scale of the imposed temperature perturbation (Fels, 1982)."

13) Page 15 and elsewhere. This section is interesting, but the paper is too vague regarding the outcome. What is the origin of the implied "upper tropospheric constrain"? Could it be clouds? Something else? Are there any insights to be had from the full IGCM? This isnt very useful unless it can at least clarify what may be meant here.

The main message of this section is to show the various factors that determine the vertical structure of the temperature annual cycle. The 'upper tropospheric constraint' is justified as being necessary from the small observed annual cycle below 130hPa in ERA-Interim. We did not explore what causes this constraint (and we said that) but we have presented clear arguments as to how the existence of the constraint has been deduced. We see this material as important precisely because it raises interesting questions that cannot be answered and we have retained it.

For clarification we have added the following to the text on Page 23, line 19:

"We have not attempted to provide an explanation for the inferred upper-tropospheric constraint and highlight this as an area for further study."

14) page 22, lines 23-26. Ozone values are stated for 70 hPa, while H2O values are stated for 90 hPa. Consistent comparisons should be made. Page 8, lines 21-24 state that 30% of the ozone effect at 90 mbar is non-local, (perhaps 40%, when the missing 10% is accounted for). Therefore, at 90 hPa your results suggest similar magnitudes for non-local effects of both species.

The text has been edited and this comparison has been clarified together with point 10) above.

15) Please comment on the differences between Fig 1 (ERA-interim) and the combined effects of ozone and water in the IGCM shown in Fig 12. Do the residuals seem plausible? What do you think accounts for them? At 90 hPa in particular, there seem to be some large remaining terms if this model is correct. What do you think they could be?

As explained at various places in the paper – e.g., on Page 17, Line 3, the dynamical model (IGCM) calculation shown in Fig 12 is not intended to reproduce the full ERA-Interim temperature change, since the model calculates only the zonally symmetric dynamical adjustment to the ozone and water vapour radiative heating without a full treatment of the role of wave induced forces driving the annual cycle. (The implications of this are discussed in the final Discussion section.)

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