

Response to reviewers

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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.

In response to the comments of Reviewer 1 concerning the differences between SEFDH and dynamical calculations we have reexamined the IGCM calculation and modified it slightly to minimise any differences between it and the SEFDH calculation arising from implementation rather than dynamical adjustment.

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

Reviewer 1

What was assumed in this study regarding clouds? Are the results sensitive to clouds, both in the region from 100-130 mbar and below that level?

All calculations in the paper assume clear-sky conditions as stated on page 4, line 15. While it is possible that cloud effects could modify the TTL temperature responses to ozone and water vapour, and as such a careful exploration of this effect would be a valuable contribution, we note that our calculations accounting for the effects of both clear-sky radiative and dynamical heating reproduce well the observed annual cycle in TTL temperature, suggesting that the net effects of clouds (other than those included indirectly in the dynamical heating) are small, justifying this choice of focus.

Nevertheless, in revising the paper we have thought carefully about cloud effects and have performed additional SEFDH calculations to give an indication of the order of magnitude of cloud effects. These calculations show the effect of including annual mean climatological cloud cover is to decrease the peak-to-peak annual cycle temperature change due to ozone at 70 hPa 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. For this estimate, we used the high, medium and low annual mean cloud fractions from the ISCCP dataset (1983 to 2006). The clouds lead primarily to a reduction in the amount of upwelling longwave radiation reaching 70 hPa of about 0.05 K day^{-1} which in turn decreases the ozone temperature response. Clouds also have an annual cycle in radiative heating. One estimate, at a single location in the tropics, from Fueglistaler and Fu (2006) shows that the annual cycle in radiative heating from clouds is about 0.05 K day^{-1} (peak to peak) at 70 hPa but these values are at present poorly constrained. On the basis of these rough quantitative estimates we consider it unlikely that including cloud effects will invalidate our main arguments and conclusions.

However, we do not feel that it is practical or appropriate to present these estimates in the revised version of the paper – the problem is sufficiently complicated that a brief explanation could not do it justice and the scientific uncertainties are large.

b) How sensitive are the conclusions regarding the role of water vapor in the 100-130 mbar region to the background values of water vapor adopted, both in that region and below? The paper should discuss sensitivities to both the assumed water vapor background values and to cloudiness. If either or both are important, then what does that imply for the robustness of the results presented?

The results are not sensitive to the background concentrations of water vapour within the ranges of values in the SWOOSH dataset (page 16 Line 1-3). The main sensitivity is to background concentrations of ozone which we have now discussed in more detail with supporting calculations added in Appendix C.

2) The paper is quite long. It would greatly benefit from shortening with a particular eye to focusing on what is new here, and how robust it is. Results that are not robust to uncertainties should be edited, including where they are used to highlight differences relative to other studies. For example, the heavy emphasis on differences in ozone responses compared to Fueglistaler et al. on page 9 isn't warranted given the strong sensitivity to ozone climatology later stated on page 10. If the results are that sensitive, this doesn't merit a page of discussion. This occurs in several other places, and the paper would benefit from tightening throughout.

The paper has been considerably shortened. The main text (excluding references and appendices) has been reduced from 28 pages to 24 pages during revision. In particular, the review of current literature in the introduction has been made more concise and the discussion of the differences compared to Fueglistaler et al. (2011) is now shorter.

3) In many places, the review of past literature that is not new here could usefully be shortened. To give one example among many, potential reasons advanced in prior work as to why ozone varies between hemispheres don't need to be discussed in detail. This work is not about the reasons for ozone variations. Its focus is on their radiative effects.

This point has been addressed together with the comment above whilst shortening the paper.

4) Differences in the “smoothness” between SEFDH calculations and a 2-D dynamical model are heavily emphasized, but the computed changes are mainly in a few limited regions. It is useful and very helpful that the authors show that there is essentially no difference in the tropical mean. But its also true that over much of the region from 20N- 20S, Figures 10a,b and 11 show differences of less than 20% between the SEFDH and 2-D dynamical model calculations; i.e., Figures 10 and 11 show that the SEFDH and 2-D model agree quite well in more places and times than they disagree. Highlighting local differences that occur only in limited places doesnt provide a balanced representation of the findings. Language should be changed throughout the paper to avoid over-emphasizing spatially limited changes, and a more balanced account should be provided.

We have added Figure 10(c) which shows explicitly the difference between the SEFDH and dynamical temperature responses.

The validity of assuming fixed dynamical heating within the tropics has been questioned by a number of important studies on this topic. Fels et al. (1980): “Supporting auxiliary calculations using purely radiative models are also presented. One of these, in which the thermal sensitivity is computed using the assumption that heating by dynamical processes is unaffected by changed composition, gives results that are generally in excellent agreement with the GCM. Exceptions to this occur in the ozone reduction experiment at the tropical tropopause...” – i.e., Fels et al. (1980) are saying that (SE)FDH may give a poor estimate of the temperature response in the tropical tropopause region to changes in ozone in this region. Garcia (1987): “This implies that tropical heating will tend to be balanced by a mean meridional circulation rather than by radiative relaxation, in agreement with the conclusions of Fels et al. (1980).” If this statement by Garcia (1987) was taken as a rule of thumb then it would imply that a zeroth-order approach to considering the effect of changing ozone in the tropical tropopause region should be to assume that all the anomalous heating is balanced by a change in dynamical heating – i.e. a kind of anti-SEFDH. In practice, the extent to which the temperature response to an ozone perturbation is correctly predicted by SEFDH comes down to details, in particular the latitudinal structure in the ozone perturbation. The heating anomalies associated with broad features (more than 20 degrees of latitude for this problem, as argued in the paper) give rise to a broad temperature change as predicted by SEFDH. The heating anomalies associated with narrow features are primary balanced by changes in dynamical heating and the temperature response is small. So simply giving the size of local differences does not seem an effective description of how things work.

We believe that this characterization in terms of latitudinal scale is the most useful and general way to describe the effectiveness (or otherwise) of the SEFDH calculation, rather than focusing on whether or not numerical values are in agreement in particular locations. Nonetheless, in revision we have taken account of this comment from the reviewer by trying to ensure that our description is as clear and quantitative as possible (see also Point 6 below). As such, we have explicitly shown the differences between the SEFDH and dynamical adjustment calculations.

5) a) Are your statements about differences in the SEFDH and 2D calculations robust to uncertainties in the adopted constituent distributions? Given the strong sensitivity of the results to the background climatologies in ozone, how might errors in the SWOOSH datasets background climatologies of ozone as a function of latitude affect your results on this point? What about water vapor gradients? I would expect dynamical responses to a radiative perturbation to depend upon the background climatological gradients and was surprised that there was no discussion of that.

As explained in the paper and noted above, the differences in SEFDH and 2D dynamical calculations are largest when latitudinal gradients in the anomalous heating implied by the change in constituents are large. Therefore, broadly speaking, errors in the SWOOSH dataset on small horizontal scales will have a greater effect on the differences (and will be manifested in small horizontal scales), while errors on larger scales will have a less significant effect on the difference.

In a similar vein, the effect of errors in the background climatologies will largely be determined by how those errors modulate the anomalous heating associated with the annual variations in ozone and water vapour. If errors in the climatologies tend to reduce the small horizontal scale features in that anomalous heating, then the corresponding SEFDH vs. dynamical differences will be smaller. If background errors tend to increase the small horizontal scale features, then the differences will be larger. There does not seem to be any particular reason why errors in the background field should systematically reduce or increase the small horizontal scale features in the anomalous heating. Furthermore, irrespective of the effect of such errors, the same broad principle applies – large horizontal scale features in SEFDH calculated temperature response are likely to be robust, small horizontal scale features are not.

6) The paper does not present a clear case for what causes the decrease in 'smoothness' for the SEFDH versus IGCM, which is the central key point in the paper. The comparison of changes in vertical velocity in Figure 10c and the difference between the SEFDH and IGCM calculations suggests as many mismatches as it does matches, so this on its own doesn't serve to convince the reader. It is suggested in the text on the bottom of page 22, top of page 23 that there is a balance involving Q_{rad} , the time rate of change of temperature, and dynamical heating, but the paper doesn't demonstrate a balance. To be publishable, the paper needs to show exactly how these (or other) factors change in the model to produce the results shown. It would be appropriate to do that for a few of the key places where there are larger local differences between the SEFDH and the IGCM, and a few of the places where there are no such large changes.

In addressing this important comment (some aspects of which are addressed by our replies above to 4 and 5), we have first made some minor modifications to the implementation of the radiative code in the IGCM calculation to ensure that the SEFDH and IGCM calculations are directly comparable. These changes have only a minor effect in the updated Figure 10 (see below), but this verifies that differences between the SEFDH and dynamical adjustment calculations do not arise simply from differences in implementation.

The following text has been added:

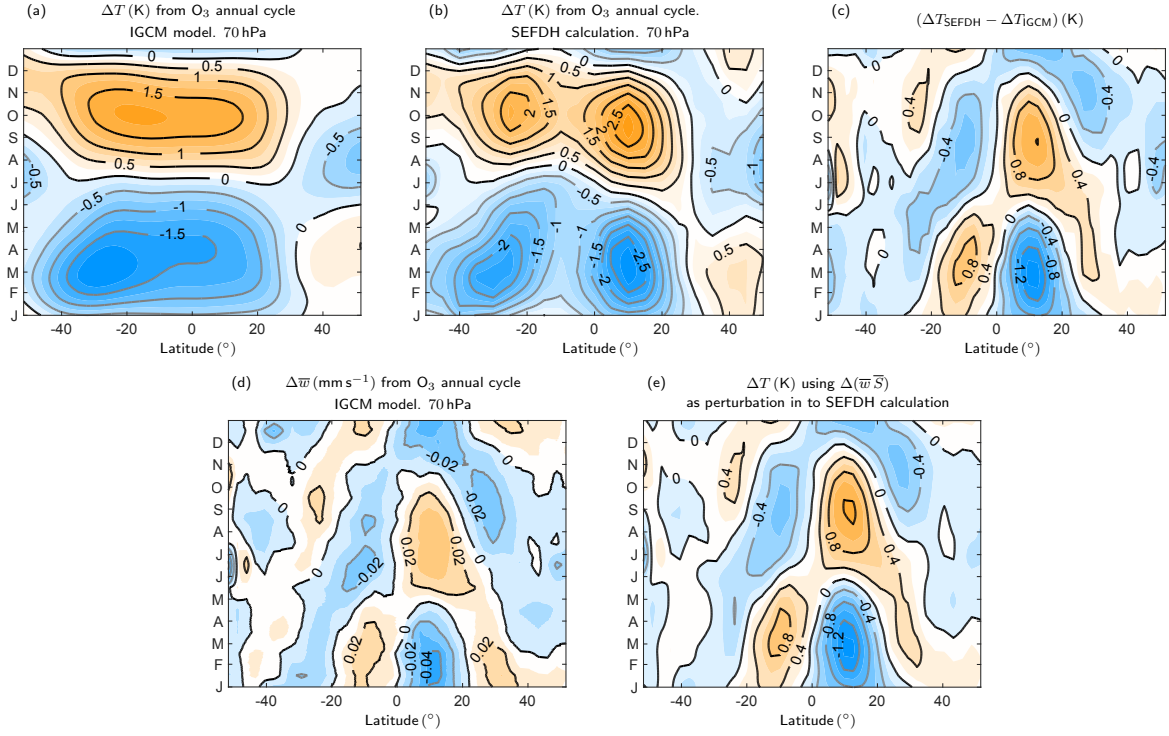


Figure 10: (a) Monthly temperature changes showing the annual cycle at 70 hPa calculated using the idealised dynamical model (IGCM) with an annual cycle in ozone. (b) Figure 3(b) is reproduced here for comparison and shows the corresponding SEFDH calculation at 70 hPa. (c) Difference in temperature change (K) between SEFDH calculation, (b), and the IGCM calculation, (a). (d) Change in upwelling in idealised dynamical model. (e) Temperature change at 70 hPa calculated by imposing the term $\Delta(\bar{w}\bar{S})$ from the dynamical model as a perturbation to the SEFDH calculation. See main text for more details.

“Figures 10(a) and (b) compare the temperature change at 70 hPa caused by the annual cycle in ozone in the dynamical model and in the SEFDH calculation, respectively (Fig. 10(b) is identical to Fig. 3(b), but is included here for ease of comparison). Fig. 10(c) shows the difference between the two. The figures show the importance of including the dynamical adjustment, which tends to broaden the temperature response in latitude in the tropical region, making it more symmetric about the Equator. Note, in particular, the effect on the off-equatorial maximum at about 10° N in the SEFDH calculation, which is no longer a distinct isolated feature in the dynamical calculation.”

We have further added Figure 10(e), which shows the temperature response induced by the dynamical heating from the change in upwelling shown in Figure 10(d). The temperature response

in Figure 10(e) closely matches the SEFDH - dynamical adjustment temperature differences in Figure 10(c), thus demonstrating quantitatively that the temperature differences are explained by the dynamical response. We then include a discussion of the criterion that determines the range of latitudes over which dynamical heating response will dominate.

7) It would be helpful to have more information here on how the 2-D dynamical model performs. 2-D models obviously have many limitations, and the references cited generally focus more on broad dynamical phenomena than on quantitative performance. Does the 2-D model generate accurate seasonal and latitudinal climatologies of temperature, winds, and circulation from apriori information? Is the model mean circulation or background temperature distribution tuned? How much confidence is there in the models ability to simulate the strength of the Brewer-Dobson circulation (critical here), and how has it been tested? More discussion of confidence in the models quantitative performance is needed, since the papers key findings rest on a robust quantitative simulation of meridional circulation perturbations from a model of reduced dimensionality.

The dynamical model is being used to calculate responses to the changes in constituents, not calculate 'background quantities such as the time-average Brewer-Dobson circulation. There is no sense in which the dynamical model is tuned. It simply contains the basic ingredients of the 2-D dynamical equations, plus radiative relaxation as represented by a radiative calculation considering the temperature response as an anomaly relative to a specified background temperature field. We noted in the original text that tests had shown that the calculated response was insensitive to plausible changes in that background temperature field – e.g., changing it from annual average to solstice distributions.

We have made it clear that the dynamical model does not include the effects of change in eddy forcing and we have carefully discussed the limitations of that.

8) The paper doesnt state what years were used to define the background climatologies for H₂O and O₃ against which seasonal anomalies were evaluated from among the range of 1984-2015 available in SWOOSH. This is particularly concerning for ozone, where it is clear that there have been long-term trends in the tropics. Decadal variability in tropical H₂O has also been established in the scientific literature. What years were used? How much do the years chosen matter, both in terms of background climatologies and amplitudes of the responses you are interested in, both for ozone and water vapor?

This information is given on page 5 line 13. The seasonal cycle is provided by SWOOSH using data from 1984 to 2015. The interannual variability is incorporated into the uncertainty estimates for both ozone and water vapour (see Appendix B).

Detailed comments: 1, Lines 8-9. Given the high sensitivity to the adopted background ozone values, as you show in the text, it isn't clear that your results being high deserves such emphasis here.

We were intending here to put our results into the context of other literature on this topic. However, taking our results together with those of Gilford and Solomon (2017) and Chae and Sherwood (2007), all of which show a larger amplitude for the ozone annual cycle effect than Fueglistaler et al. (2011), we have removed this clause from the abstract. A discussion of the quantitative magnitudes and how these compare to other literature is still given in the main text (Pages 6 to 9).

1, Lines 12-13. Is the non-local result robust to assumptions about background water vapor and background and seasonality of cloudiness? Clarify or delete. tropical upwelling?

The non-local radiative result arises from the properties of the radiative transfer for water vapour in the TTL and is robust to background water vapour [tested and reported in the paper, page 11, line 5] and tropospheric cloudiness [tested and not explicitly reported in the paper]. Neither the effects of background cirrus nor the seasonality in cloudiness have been considered, but this is clearly stated as outside the scope of the paper.

4, Lines 10-13. Should tropical gravity waves be explicitly noted as a possible factor in tropical upwelling?

Tropical gravity waves are possibly relevant but whilst condensing the review of past literature, as recommended by the referee, this paragraph has been modified to

“Many studies have focused on the role of wave-induced forces in driving the annual cycle in temperature through their effects on upwelling in the TTL and hence on $\overline{Q}_{\text{dyn}}$ in Eq. 1, although uncertainty remains about what types of waves are the most important (Randel and Jensen, 2013, and references therein).”

6, Lines 18-19. What is meant by “a three-point Gaussian is used to account for the diurnal variation in solar zenith angle”? Please clarify what you assume regarding diurnal changes in heating rates.

The Morcrette/Zhong and Haigh radiation code incorporates a common method of calculating a diurnal average which accounts for the variation in solar zenith angle over the day. Note that how we compute these diurnal averages does not affect the results in this paper so we have revised this sentence to “Shortwave heating rates are calculated as diurnal averages”.

7, Lines 1-2. Are you sure that the temperature changes would necessarily be zonally uniform if, for example, there are zonal asymmetries in potentially thick clouds?

This is a statement about our calculation, in which all quantities are assumed to be independent of longitude, rather than a statement that ‘if TTL temperatures and constituents are zonally uniform then the temperature response will be zonally uniform’. We have amended the text to clarify this as well as to emphasise the fact that the zonal mean adjustment would smooth regional changes in temperature that would be predicted by an SEFDH calculation.

9, Lines 21-23. Why is it necessary to presume this rather than determining whether this is actually true in your calculations?

This sentence has been edited to remove the word “presumably”. This claim is essentially verified by Fig. 3; the sentence has accordingly been strengthened.

Page 10, line 13. The claim of a 10% accuracy in the SWOOSH dataset is remarkable. Is this 2-sigma? Does it apply for local values across the full range of latitudes of interest here, all the way down to 130 mbar? What is this claim based on? I would not expect 2-sigma absolute uncertainties in tropical ozone in SWOOSH to be better than 20 or 30%, at best.

A consideration of the uncertainties as far as they have been characterized for the SWOOSH dataset (see Davis et al., 2016) suggests that 10% for climatological values is reasonable at 70 hPa

although this uncertainty increases to 30-40% by 130 hPa. We emphasise to the referee that these calculations are included in the manuscript to highlight the sensitivity of the calculations to the background ozone profile, and as such the 10% values quoted are intended to represent plausible ranges of ozone values rather than a precise assessment of uncertainty, for which a more detailed explanation for the SWOOSH dataset is given in Davis et al. (2016).

We have also added an Appendix C with additional calculations to describe the effect of varying background ozone values which is the primary source of sensitivity in our results. These calculations are intended as illustrative calculations rather than a precise assessment of uncertainty.

Page 11, line 17. Why ‘perhaps’? Please provide a clear statement based on your quantitative results as to whether it is or isn't.

The word ‘perhaps’ has been removed.

Page 13, line 8. Garbled sentence.

This sentence was too long and has been revised to:

“In each case, the latitude of the maximum response is further north than the latitude of the maximum amplitude in the water vapour mixing ratios at that level. The fact that there is no simple relation between the latitude-time structure of the SEFDH-predicted annual cycle in temperature at a given level and the latitude-time structure of the water vapour mixing ratios at that level is further evidence for important non-local contributions in the vertical from water vapour to the temperature variations.”

Page 14, line 18. A secondary

It is not clear from this comment what the reviewer’s concern here was; however the relevant text has been modified in the course of shortening the paper.

Page 20, line 2. Why ‘maximum possible relevance ... to the real atmosphere’? A full 3-D model, and a complete line-by-line radiative code would have more relevance to the real world, so this statement should be deleted.

The text has been amended.

Sections 5.2, 5.3, 5.4, and 6 require revision to deal with the above comments. I will not repeat those remarks here on a line-by-line basis but they occur in many places.

These Sections have been revised. See responses to previous comments.

Page 25, lines 2-4. Gilford and Solomons paper has been accepted in J. Climate. Your statement that your consideration of different vertical layers, and water vapor, is different from that paper is not correct. Gilford and Solomon did consider water vapor, as well as concentration perturbations in different layers. Please revise to accurately quote what Gilford and Solomon did.

We have modified our discussion of Gilford and Solomon (2017) accordingly.

Page 28, lines 15-21. 'should not be taken too seriously' is not clear and its not scientific language. Please provide a quantitative statement that is specific, and balanced across regions of agreement and disagreement and considers robustness of your results as noted above in the major comments section.

The text has been amended to improve clarity and exploit the new Fig. 10. As noted in earlier replies, the key and robust point about the difference between SEFDH temperature response and dynamical temperature response is that this is largest at small horizontal scales. As can be seen in the revised Fig. 10(c), local differences are zero for some latitudes and dates but for others, these are up to about 1.5 K, i.e., about 50% of the maximum SEFDH 'signal'. Again, simply giving the size of local differences does not seem an effective description of how things work.

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