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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The radiative role of ozone and water vapour in the temperature annual cycle in the tropical tropopause layer" *by* Alison Ming et al.

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Review of the paper by Ming et al.

Major comments

1) a) 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? 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?

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.

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.

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 it's 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 doesn't 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.

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 dataset's 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.

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 Qrad, 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.

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 model's ability to simulate the strength of the Brewer-Dobson circulation (critical here), and how has it been tested? More discussion of confidence in the model's quantitative performance is needed, since the paper's key findings rest on a robust quantitative simulation of meridional circulation perturbations from a model of reduced dimensionality.

8) The paper doesn't 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?

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.

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.

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

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.

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?

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

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.

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

Page 13, line 8. Garbled sentence.

Page 14, line 18. A secondary

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.

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.

Page 25, lines 2-4. Gilford and Solomon’s 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.

Page 28, lines 15-21. ‘should not be taken too seriously’ is not clear and it’s 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.

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