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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Normal mode Rossby waves and their effects onchemical composition in the late summer stratosphere" *by* D. Pendlebury et al.

D. Pendlebury et al.

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Response to reviewer 1 (Gloria Manney):

Major comments:

A. The authors show/discuss correlations between chemical species (CH4, N2O, O3) and dynamical variables (temperature, winds), and use these to suggest where transport and/or chemical effects may be most important. However, an explicit discussion of what types of correlations (positive, negative, lagged or simultaneous) are expected theoretically is not given, making it rather difficult to follow the reasoning given for saying particular correlations result from transport or chemistry. Instances where this is a factor are: Page 12016, lines 3-4 Page 12018, lines 1-8 Page 12019, lines 1-19 An explicit discussion of what correlations are theoretically expected given the gradients and lifetimes of the species in questions should be added, either in the introduction or at the beginning of section 3.1, to help the reader understand the following arguments.

Comments were added where appropriate to discuss the expected correlations. There is no a priori reason to expect any particular correlation between temperature and methane or nitrous oxide since this relationship will depend on the timescale of the motion and on what type of transport (i.e. vertical or horizontal) dominates. The fact that the behaviour between the dynamical fields and the chemical fields is complicated is elaborated on in the Summary and Discussion.

B. The other overall theoretical relationship that should be discussed explicitly early in the paper is the expected vertical structure of the normal mode waves in question, as well as the vertical range of the measurements taken during the MANTRA campaign. This would help the reader understand why the particular vertical ranges in question are focused on.

Measurement range of MANTRA added in the first paragraph. A brief discussion on the vertical structure of modes added in the introduction as well.

C. In Introduction and Conclusions (I believe partly because of point B), it is not clear why assimilated meteorological analyses could not/are not used for this type of analysis, especially since they are typically available in near-real-time. Of course, some of the answers to this are fairly obvious – only a few recent products (e.g., from GMAO, ECMWF) that extend above 60 km, lack of data constraint at higher altitudes, etc. But it would be nice to see (1) a brief mention of why the assimilated analyses cannot be used for this type of study in the introduction, and (2) a brief discussion in the conclusions section of whether assimilated analyses might be used for this purpose in the future and whether model studies like this might help to "validate" analyses for this purpose.

Typically meteorological analyses do not extend high enough, or if they do there would

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be an inhomogeneity around the stratopause where the data ends, which could be problematical for this kind of study. Most importantly, though, most analyses do not have chemistry, and examining the relation between dynamics and chemistry is the whole point of this exercise. Note that assimilated chemical fields should not be used for this type of study since the act of assimilating the chemistry could destroy the relationship between the chemical and dynamical fields. On the other hand, you're quite right that respecting the fidelity of these kinds of physical relationships would be an important means of validating analyses. Brief discussions in both the introduction, explaining why a CCM is better than analyses for this study, and the conclusions, explaining how these kinds of results would be useful for validating data assimilation systems, have been added.

Specific Comments:

Page 12013, line 17, what about above 50 km? A brief discussion of the vertical structure of the 5-day wave was added. Vertical information for the 10 and 16 day waves was also added.

Page 12013, line 22, it would be worth mentioning what sort of excitation mechanisms have been suggested. Added.

Page 12014, line 8, is there some reason why it is a particularly important issue during summer moreso than at other times of year? This is an issue in the summer because these modes are likely a source of planetary scale variability in the summer stratosphere. In the case of the 5-day wave, the amplitude of the wave is greatest in the summer hemisphere, albeit higher than considered here, but in later summer, as equinox approaches, all three modes become important. In the winter, stationary planetary waves dominate, and their effects would be much more important. This last point is made in the first paragraph of the introduction. A reminder is also given in the last paragraph of the introduction

Page 12014, line 21, would be nice to say what approximate lat/lon spacing T32 corre-

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sponds to. Added.

Page 12016, lines 10-11, the behavior of N2O and CH4, especially for the 5-day wave, appears to me *very* different that that of temperature, with a single maximum near 50 km rather than two maxima above and below that. So I don’t really see how you can say "Similar behavior is seen in..." here? The statement "below 50 km" was added to clarify what region is being focused on, and the fact that the secondary maxima above 70 km are not present in the methane and nitrous oxide was added. Page 12017, line 16, "...below a certain altitude..." What altitude, and how is that altitude determined? This paragraph was rewritten and now refers to Sankey and Shepherd (2003), which studied the chemical correlations in CMAM extensively. This particular phrase was removed from the paper, but the point is made that since methane and nitrous oxide are well correlated, one would expect the temperature-methane and temperature-nitrous oxide correlations to be similar. This provides a reality check on the model results, which was really the point of the original paragraph.

Page 12018, lines 4-5, aren’t the chemical lifetimes (of ozone in particular, the others are always long enough that the question is moot) latitude-dependent? This is true, but only one latitude is being considered here, and so it is the vertical dependence of the lifetimes that is important. This has been clarified in the text.

Page 12018, lines 9-15, "these altitudes" is used repeatedly in this paragraph, leaving the reader to guess that you are talking about the smae altitude range as in the previous paragraph? The first "these altitudes" has been changed to "Above 35 km to about 50 km" to specify which altitude range the paragraph is dealing with.

Page 12019, line 13, might want to put a comment (probably back in the introduction) explaining why the fields to test these relationships are not available from these runs (since in theory the model runs provide quite sufficient information to do so). The fact that this run is used by other studies for the MANTRA campaign has been added. This was an older version of the model and data cannot be regenerated since the computer

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has changed. Only certain fields were saved, and such a study (requiring high frequency sampling of the chemical fields on global scales) wasn't originally envisioned.

Page 12020, line 22 and 29, on the former you say the 5-day wave "shows the greatest variation in timing from year-to-year (hyphens should be spaces here)"; on the latter, the 10-day wave is "...showing the strongest variation in timing". Please clarify this apparent contradiction. The 10-day wave is showing the greatest variation in amplitude, but generally it is weaker at day 215 and strengthens later in the summer. For certain years, the maximum amplitude of the 10-day wave is quite large compared to the average but it always grows from a small amplitude at the beginning of August (day 215). The 5-day wave, on the other hand, shows less variation from year-to-year in terms of its maximum amplitude, but that maximum amplitude may be reached at the beginning of August, the end of August or the middle of September. A bit more discussion has been added to clarify this statement.

Technical correction: Page 12021, line 18, "respond" should be "responds". Changed.

Response to reviewer 2:

Major Comments: I feel that more detail regarding the correlations can be made from the CCM. Is it not possible to compute a budget of each chemical and explicitly determine if changes are due to chemistry or transport. I understand that once you decompose the field into wave components, then we assume a linear superposition and budget calculations may not be strictly valid, but I find the inability to make stronger statements about the contribution of transport (horizontal or vertical) and chemistry strange considering the fields are calculated in the CCM. I think the paper could be improved if more attention was placed on understanding these variations or at least discussing why these calculations are so difficult to make. More comments on this theme given in ”minor comments”; section.

While it is true in theory that all fields should be obtainable from the CMAM, the global fields for chemistry are saved at only 3-day intervals (this is standard practice for

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CMAM), which is not frequent enough to extract the 5 or even 10 day waves. The other saved fields were not sufficient to make more concrete statements. The fields saved at high frequency were saved at only one latitude, making any meridional analysis impossible, while the global fields saved at 18-hour intervals were saved at only 5 levels. Unfortunately, this version of the model cannot be rerun since the computer it was run on is no longer available. This run was used in order to be consistent with other MANTRA papers.

Each figure needs changing. Contour labels are needed (maybe not every contour for at least a few), as it is very hard (or impossible) at times to even tell the sign. Fig 1 is a good example where it’s not possible to know the values. A bold zero line would help, and shading for positive values or even the use of color with a color label could work. Ditto for Fig 2-4 and 8. Fig 6-7 are also hard to determine the exact correlations and need changes. I’m not sure how useful Fig 8 is. Perhaps a statement describing it would be sufficient.

Contour labelling and colours were added to some of the figures. In others, the gray shading was changed to red and orange so that it stands out better. In others, contour labelling was not added because it confused and crowded the plots.

Other (some minor) comments:

1. P12012: L25: Other than planetary waves might also be expected. I’d generalize this to ”other wave variability (e.g. free modes and smaller scale forced modes)”;. This was not changed since variability would have been used twice in the same sentence (it sounded awkward). We would also prefer to specify that these are large-scale waves being considered. Small-scale waves such as gravity waves are not considered in this study at all, and no comment can be made on their impact.

2. Pg. 12013, L 18: Consider rewording wave to ”wave chacteristics”;; Changed.

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3. Pg 12014, L20: I suggest you add more information the quality of model simulations with CMAM. For example, I believe CMAM was used in the latest ozone assessment (WMO 2006) and thus compared with other CCMs. A reference to this (i.e. Eyring et. al. 2006/2007) and some comment about how CMAM compared to other models would inform the reader how well this model is compared to other state-of-the-art systems. This was not the same version of the model used for the CCMVal or WMO ozone assessment. A bit more information on the model run was added, and references to other papers using the same version of the model were also added. See later comment on model details.

4. Pg 12015, L7: Consider chaning ”strongest signal”; to ”large variability in CMAM is seen at 60km”; or something like that. I expect you also see large variability in daily temperature at the surface too. Statement was changed to ’The largest variability in the CMAM data at higher altitudes…’. The tide grows with altitude and we don’t want to give the impression that its effect is somehow a maximum at a particular altitude.

5. Pg 12015, L13: Can you provide some information of how large these signals are in comparison to the total. Do these signals explain 10% of the variance or 50%? A brief sentence about the small-scale variability removed from the fields was added.

6. Pg 12015: L6: More information about the particular CMAM run is needed. What was the configuration of the simulation (what year, what forcing used). Was their spinup of the model, and what SST were used. Some more information on the nature of the run was added, but details such as SSTs and initialization procedures were not. Nothing special was done to the model to force the normal modes and so the specification of initial conditions etc, is not of great interest. In addition, this was a climate run, not a transient run, and analysis was done on years well after any spinup period (this fact was added to the paper).

7. Pg 12017: L13: Please briefly explain the statistical confidence technique employed

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so the reader does not have to look up the paper just to get an idea. A brief description was added.

8. Pg 12017, L17. It would be really nice to see the CH4/N2O relationship especially because it would allow comparison with other measurements (i.e. Haloe). It would also provide a nice tracer/tracer correlation where one does not have to consider chemistry at all. Comparisons of chemical correlations between CMAM data and observations, for the same version of CMAM, was already shown in Sankey and Shepherd (2003, JGR). This paper is referenced.

9. Pg 12017, L25: Work by Nathan et. al., (GRL 2004) show that wave ozone heating affect the phase and amplitude of free modes in the summer stratosphere. I expect you are familiar with this work and that at least you should mention the relationship between free modes and ozone. I expect this is one of the reasons for the different phase relationships between the tracers (CH4/N2O) and ozone in the lower stratosphere. You didn’t mention this, but if they were all behaving purely as long lived tracers, then you would expect their phase relationships to be similar, but they are not as seen in Fig 6 and 7. The paper we found is Nathan et al (GRL 1994), which deals with ozone heating and the effect on travelling waves in the summer hemisphere (this heating can compensate the damping effect of Newtonian cooling) in the troposphere and lower stratosphere which can lead to generation of waves which can then propagate into the stratosphere. The phase of the ozone perturbation with respect to the meridional velocity depends on the growth rate of the wave (being 90 deg out of phase for a stable wave). The effect seems to maximize around 20 km and depends crucially on the background zonal wind profile. Clearly, the relationship between temperature and ozone is more complicated than the relationship between temperature and methane/nitrous oxide, and this is due to the role of ozone in the radiative balance, but it is not clear how the results from Nathan et al (1994) apply to the correlations here, especially since they do not mention the phase relationship between temperature and ozone. The significant correlations in figure 6 are of the same sense below 30 km for all three species.

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10. Pg 12019, L7. CCM models allow budget calculations of all chemistry fields to be made, and thus I would expect one should be able to know if it’s meridional or vertical transport with more certainty. Certainly, for the full field a budget calculation should be possible, although for the wave components, this may not work as well. The bottom line is to understand if the correlation is positive, then what is causing this correlation. If indeed that cannot be done in your analysis, then what type of dataset would be needed to answer this question? See above comments about the saved fields. In addition, budget calculations of model fields are non-trivial. Frequent saves of the full-chemistry fields are needed and fluxes must be calculated in precisely the same way as in the model to close the budgets. 3-day saves of chemical fields are standard for CMAM, due to disk space limitations.

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