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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.

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Review of "Normal mode Rossby waves and their effects on chemical composition in the late summer stratosphere" by Pendlebury et al.

Recommendation: Publish in ACP after some clarifications

General Comments:

This paper uses dynamical (winds, temperature) and chemical fields from a GCM extending through the middle atmosphere to study wave motions in summer and to assess their potential influence on single-station measurements taken during the

MANTRA campaigns at the end of summer. The results presented are to the best of my knowledge, new and unique, and thus will be of value both to people taking measurements under these conditions and to the broader research community. The paper should be published in ACP after a few clarifications that are needed to make it a stronger paper:

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.

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.

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

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future and whether model studies like this might help to "validate" analyses for this purpose.

Specific Comments:

Page 12013, line 17, what about above 50 km?

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

Page 12014, line 8, is there some reason why it is a particularly important issue during summer moreso than at other times of year?

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

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?

Page 12017, line 16, "...below a certain altitude..." What altitude, and how is that altitude determined?

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?

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?

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).

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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.

Technical correction:

Page 12021, line 18, "respond" should be "responds".

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 12011, 2007.