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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Increase of upper troposphere/lower stratosphere wave baroclinicity during the second half of the 20th century" *by* J. M. Castanheira et al.

Anonymous Referee #2

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Trends in the energy of vertical normal modes based on NCEP/NCAR reanalyses data are presented. A positive trend for the deep modes (m<5) is found for the period 1958-2006 and attributed to increased UTLS baroclinicity. This positive trend is supported by positive trends in available potential energy (APE) in the subtropical UTLS based on ERA40, NCEP2, and (to some degree) JRA-25 reanalyses. Further, a positive trend in the frequency of occurrence of double tropopauses for the period 1970-2006 is documented and linked to increased UTLS baroclinicity.

Overall I find the material represents an interesting and somewhat novel view on dynamical changes in the UTLS. It fits well within the scopes of ACP and the paper is generally well written. However, I do have a couple of major comments that should be addressed before publication.

Major Comments

1) The authors should motivate better why they think a vertical normal mode analysis is useful in the present context. For example, the fact that the atmosphere does not exhibit a well defined upper boundary (unlike e.g. the ocean) renders atmospheric vertical normal modes somewhat artificial. Further, the NCEP/NCAR data set only extends about half way into the stratosphere (note, the 10 hPa level constitutes the first level below the model top). Also, the two different parts of the study - normal mode analysis and tropopause analysis - need to be connected better. The link between double tropopause events and baroclinicity is currently not clear.

2) Wave baroclinicity needs to be specified more precisely. Wind shear changes sign at the level of jet maximum, so it seems necessary to distinguish between upper tropospheric (positive shear) and lower stratospheric (negative shear) baroclinicity. It's not clear to me, though, whether this distinction is possible with the normal mode analysis.

3) The detailed attribution of certain vertical modes to UTLS anomalies (Figs. 1 & A1) seems somewhat problematic. For example, it is clear from Fig. A1 that m=3 contains contributions from both the middle stratospheric and the UTLS anomalies. Likewise, m=6 contains contributions from both the UTLS and the middle tropospheric anomalies. Further, it is conceivable that a slight shift up or down of the UTLS anomaly will additionally modify this balance, i.e. there seems sensitivity to the precise location of the maximum of the UTLS anomaly. Another complication comes from the fact that the normal modes in Fig. 1 represent the whole globe, i.e. the layer between 200-100 hPa includes contributions from both the troposphere (in the tropics) and the stratosphere (extratropics). These shortcomings need to be mentioned and elaborated on.

4) Given the vertical structure functions (Fig. 1): isn't it possible that the trends in the

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energy of modes m<5 (Fig. 3) is simply related to tropopause rise (such that there's a related tendency for the max energy to shift from mode 5 toward 4 - consistent with Fig. 3)? If I understand correctly the structure functions are held fixed (i.e. not allowed to be a function of time)?

5) I agree that it makes sense to include other reanalysis data sets. However, the way this is done appears somewhat inconsistent. Wouldn't it be more sensible to perform both the normal mode analysis and APE analysis with all data sets (NCEP/NCAR, NCEP2, ERA40, IRA-25)? At a minimum the NCEP/NCAR data set should be included in the APE analysis to check the consistency of the results across all data sets used in this study.

6) The two lower panels in Fig. 4 to me show clear signatures of a discontinuity in 1979 (related to the inhomogeneity in assimilated data with the start of the satellite era). It seems as though trends evaluated on either side of this discontinuity would be much smaller (possibly insignificant). The negative trend for modes m>5 - one of the claims of the study - therefore seems unsupported. This is a good place where other data sets would be helpful.

Minor Comments:

- Fig. 1 (left) seems to be identical to Fig. 1 in Liberato et al. (2007) (core information seems already included in Fig. 1 in Castanheira et al. (2002, JAS)) - needs to be stated

- The reason for the decomposition into all frequencies and high frequencies (Figs. 3,4) is not clear. There isn't much elaboration why this decomposition is important in the present context.

L25-28: I've seen this argument being made before, but it simply doesn't work (at least not the way it is presented here): the temperature gradient between tropical upper troposphere and extratropical lower stratosphere is positive(!), corresponding to negative wind shear; i.e. tropical tropospheric warming together with extratropical stratospheric

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cooling leads to a reduction(!) of the meridional temperature gradient at those altitudes; different story below the core of the jet: stronger tropospheric warming in the tropics compared to the extratropical upper troposphere leads to stronger temperature gradient, i.e. larger wind shear

L104: aren't geopotential and wind fields coupled?

L172: largest _absolute_ trend

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