Review of the manuscript "Wave Modulation of the Extratropical Tropopause Inversion Layer" by R. P. Kedzierski, K. Matthes and K. Bumke, submitted for publication to ACP

Recommendation: Significant revisions

This paper investigates the impact of synoptic- and planetary-scale waves on the static stability structure of the extratropical tropopause region. This is done by using high-resolution temperature profiles from satellite measurements and applying wavenumber-frequency filtering to them. The technique is similar to the method applied previously by the authors to the tropical tropopause layer. In the present analysis, the extratropical Tropopause Inversion Layer (TIL) appears as largely due to these waves. In polar latitudes, the part of the TIL which cannot be associated with the waves is interpreted as due to either the residual circulation or to radiative effects. These results seem to be consistent with results from previous work.

The paper is generally well written. I have a few major and a number of minor comments which the authors should account for in revising the manuscript. In particular regarding their main premise of how waves affect the TIL, I have a few questions which should be clarified in order to make the argument consistent.

Major comments

1. A key aspect of this whole paper is the recognition that the purely adiabatic impact of a wave on static stability in the tropopause region and the concomitant modification of the tropopause height may lead to a systematic TIL enhancement. Figure 2 serves as a motivation and explanation. However, I did not quite understand the related description in lines 258–260, which seems to present a key argument in this context. Please clarify.

   Several times in the text (e.g. bottom of page 9) the authors stress that they typically expect a westward phase tilt with height corresponding to upward propagation of waves. This may be OK for planetary waves, but synoptic-scale waves tend to be evanescent in the stratosphere, so I would not expect much of a phase tilt in the lower stratosphere. In addition, wave modes which represent baroclinic instability (like in the Eady model) do have a westward tilt with altitude regarding perturbation geopotential or the perturbation streamfunction, but the associated tilt of the temperature anomaly actually has an *eastward* tilt with height (see Fig. 8.10 in the textbook of Holton 2004). Also, it has been argued in the past that some gravity wave activity is actually generated in and, hence, emanates from the tropopause region (O'Sullivan and Dunkerton, 1995), and the phase tilt of such waves seems less clear or may be insignificant. How does this affect the arguments presented in the paper, which evoke a westward tilt with altitude?

2. Fig. 3 shows a few selected cases. Are these random picks, or are these cases where the wave signal at the tropopause level can be seen best. If the latter is true, this would mean that often the wave signal may be rather incoherent and hard to interpret?!

3. In their interpretation sections, the authors could tie their results more comprehensively to earlier studies, including model studies, in particular the one by Miyazaki *et al.* (2010), who systematically quantified different mechanisms for TIL formation in a GCM including their hemispheric and seasonal behavior. Also, to what extent does the currently analysis (implicitly or explicitly) include the traveling cyclones and anticyclones which have been argued before to play a role for TIL formation, and to what extent are the current results consistent with the mechanisms suggested earlier? authors choose to leave this as a remaining question (see Page 12), but it could at least be formulated explicitly as a question.

4. I do not like the use of the terms "warming" and "cooling". The authors consider band-with filtered wave signals, in other words anomalies from the zonal mean. If a plot shows a local warm or cold anomaly, this does not necessarily imply that there is/was warming or cooling. It could just as well be the result of horizontal advection, i.e. the original air was replaced by warmer or colder air, but this does not imply any warming or cooling (neither diabatic nor adiabatic). Using more precise terminology would make the discussion of the processes more lucid.

Minor issues

1. Line 86: replace "high amounts of ...." by a more idiomatic expression.
2. Line 110: the 100 m vertical resolution of GPS-RO temperature profiles, is this really comparable to the vertical resolution of radiosondes? I thought that the latter have even significantly higher resolution. To be sure, I believe that 100 m resolution is sufficient for the purpose of the current paper.
3. Line 126: replace "grid with 10° separation" by a better expression (grid points may have a separation, but not the grid itself).
4. Line 128: replace "exponentially-folding function" by "Gaussian function"
5. Line 153: replace "+- 1 longitude grids" by a better expression (and same with time-discretization)
6. Line 157: replace "in the equator" by "on/at the equator"
7. Line 160: replace "lower bottom of the vertical scale" by "lower bound of the vertical scale"
8. Line 180: replace "remaining of this section" by "remainder of this section"
9. Line 203: this dispersion relation seems to be the version for the shallow water system, right? If so, this should explicitly be stated, and it should also be motivated to what extent this can be used in the current context. After all, the paper deals with the vertically stratified atmosphere, *not* with the shallow water model. The term "equivalent depth" should then be explained, and also in what sense the term f^2/gh can be considered as an "approximation to account for vertial propagation".

10. Bottom of page 7: I find it misleading to talk here about "different wave types". As the authors show quite clearly, there is a broad spectrum of waves, which *cannot* be classified into different *types* as in the tropics. This is, by the way, exactly what the authors say themselves on page 10 ("unable to differentiate particular wave types"). Maybe one should simply refer to "different parts of the spectrum".

11. Line 236: replace "is outstanding" by a more idiomatic expression.

12. Line 249: replace "associated to" by "associated with".

13. Line 267: replace "would remain the same despite the presence of wave anomalies" by "are unaffected by the wave anomalies"

14. The longitude-height sections of Fig. 3 could be improved by actually ploting all the way from -180 ° to +180 ° .

15. Line 360: In what sense it $s = 0$ a wave?

16. Line 511: I would try to avoid the abbreviation "mSSW".

17. Line 611: replace "humbler" by "smaller"

18. Figure 2: put axes labels, i.e. longitude (or λ) and altitude (or z).

19. In figure 5 (and all corresponding figures showig time versus altitude) one might consider ploting only up to 30 km, without changing the vertical extent of the plot. This would give somewhat better resolution in the vertical, putting more emphasis on the features around the tropopause region, which is relevant here. On the other hand, the information way up in the stratosphere is not very relevant for this paper (except where discussing stratospheric warmings).

References

Holton, J. R. 2004. *An Introduction to Dynamical Meteorology.* Elsevier Academic Press, 529 pp., fourth edition.

Miyazaki, K., S. Watanabe, Y. Kawatani, Y. Tomikawa, M. Takahashi, and K. Sato 2010. Transport and mixing in the extratropical tropopause region in a high-vertical-resolution GCM: Part I: Potential vorticity and heat budget analysis. *J. Atmos. Sci.* **63**, 1293–1314.

O'Sullivan, D., and T. J. Dunkerton 1995. Generation of inertia-gravity waves in a simulated life cycle of baroclinic instability. *J. Atmos. Sci.* **52**, 3695–3716.