Atmos. Chem. Phys. Discuss.,
doi:10.5194/acp-2016-700-RC1, 2016

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Wave Modulation of the Extratropical Tropopause Inversion Layer" *by* Robin Pilch Kedziersk et al.

Anonymous Referee #1

Received and published: 12 October 2016

General comments

Influences of planetary and synoptic-scale waves on strength of the tropopause inversion layer (TIL) were investigated in the mid/high-latitude regions using GPS radio occultation data. Two-dimensional wave filtering (in longitude and time) and tropopause-based average were made to isolate wave signal in temperature and static stability (N2). Authors showed that strength of the TIL (N2 right above the tropopause) decreases significantly after applying the wave filter, and attributed it to reduction of planetary and synoptic-scale wave signature in the extratropical TIL. They also presented influences of stratospheric sudden and finial warnings on N2 of the stratosphere (and near the tropopause). Research question is clear, and the manuscript is generally well written. However, the methodology used in this study has clear limitations in isolating midlatitude-wave signature in the TIL, and physical interpretations made in the results

are controversy. I have two major questions that authors may want to consider before publication of this manuscript.

Specific comments (major)

1. For filtering method and their physical interpretation:

Authors are using filtering limits of 2-4, 4-25, 30-96 days in time and -10 to 10 wavenumber in zonal direction. The choice of filter domain is subjective, and it is difficult to make a physical connection between the filter area and mid/high latitude waves. The filter seems to cover too broad spectral range, and it may remove most of the variability within 2-96 days. Particularly, the TIL could be easily removed by variation in tropopause height, because the TIL is defined in tropopause-relative coordinate and filtering is made in altitude coordinate. Therefore, a large portion of the "tropopause-based zonal-mean TIL (shown in Figs. 5, 7, and 9)" could be filtered out, NOT because it is a part of Rossby waves BUT because Rossby waves simply change height of the tropopause. This problem could be worse with a large filter area, and authors may want to test this issue.

I agree with authors that it is difficult to define general filter area (or filtering method) for mid-latitude waves. However, some efforts are still required to define reasonable filtering area in order to overcome the limitation and get more reliable results. Careful examination on wavenumber-frequency spectrums may be helpful for figuring out Rossy wave signature in the TIL, and some results of it will also be appreciated by readership.

Minor note:

Authors mentioned that they "define a seventh filter for wavenumber zero for completeness (page 7, line 229)". It sounds like "the filter used for Figs. 5, 7 and 9 also removes wavenumber zero along with wavenumber 1-10". If this is the case, wavenumber zero should not be filtered as it is a part of mean overturning circulation (i.e., deep or shallow

branch of the Brewer-Dobson circulation).

2. Structure of temperature anomaly:

The temperature structure shown in Fig. 2 (and discussed in several parts in the manuscript) is more similar to that of gravity waves rather than that of Rossby waves. Although vertically propagating Rossby waves (with wavenumber 1, 2) have westward tilt, the tilting is not big enough to make strong N2 modulation at the tropopause (see Fig.7 of Fletcher and Kushner 2011 for example). Synoptic-scale waves could make strong modulation in temperature and N2 near the tropopause, but their temperature structure is different from that shown in Fig. 2. Based on Hoskins et al. (1985, their Fig. 15), the (potential) temperature and inferred N2 anomalies have horizontally flat structure near the tropopause. It show dense packing of isentropes over high pressure system implying strengthening of the TIL over anticyclones (this feature is constant with the idea of vertical convergence). If authors think that transient waves in 4-25-day frequency band are major contributor and Fig. 2 is their common temperature structure, it should be obtained from observation (not from conceptual figure).

I do not disagree with authors' main idea that planetary and synoptic-scale waves could have a major contribution in making strong extratropical TIL. However, I recommend that authors should test and interpret their results more carefully because the results can be sensitive to the methodology used for filtering of wave signature.

I still think that this is well-organized study and documentation. I believe authors can overcome (or minimize, at least) the limitation through careful examination of the wave spectra.

References

Fletcher, C. G., & Kushner, P. J. (2011). The Role of Linear Interference in the Annular Mode Response to Tropical SST Forcing. Journal of Climate, 24, 778–794. http://doi.org/10.1175/2010JCLI3735.1

Hoskins, B. J., McIntyre, M. E., & Robertson, a. W. (1985). On the use and significance of isentropic potential vorticity maps. Quarterly Journal of the Royal Meteorological Society, 111(470), 877–946. http://doi.org/10.1002/qj.49711147002

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