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

Recommendation: Some further revisions

The authors have carefully revised the manuscript, and many of my issues have been clarified in a satisfactory manner.

Yet, clarification had also the effect that some new issues surfaced, which were not evident to me from the original version. Most of my new issues refer to interpretation and terminology, so it should be possible to address them by appropriate rewording of specific sections. Just to be sure: the work done is valuable and the paper should be published eventually.

Major issues

1. I welcome the new discussion (section 5 in the revised manuscript), because amongst others it puts the work into the context of previous work. The authors say that their proposed "mechanism" is not a "completely separate dynamical feature" [a somewhat awkward language], but that rather "earlier proposed mechanisms are integrated in their mechanism" (lines 660 ff). In my view this somehow conflicts with the claim that they "introduced a new mechanism" (line 626, also line 20 "our wave modulation mechanism").

   Maybe I start here to quibble about terminology, but what is a "mechanism" versus or a "feature" in the authors' opinion?

   To me, what the authors show boils down to conservative reversible dynamics on (typically) synoptic time-scales and its effect on the zonally averaged TIL in a tropopause-based coordinate system. This is what the method sees, because the method considers spatio-temporal deviations from an average (i.e. the wavenumber-frequency filtering). The authors call this "wave modulation". I find this somewhat misleading, because obviously the method applied cannot see anything but waves; even a very localized anomaly (a delta function) shows up as a superposition of waves (maybe this is alluded to on lines 291–295, but I did not understand this paragraph). In other words, a very local displacement of the tropopause (say, through an isolated anticyclone) does produce the effect studied in the paper, but in my view "wave modulation" is not a good term to describe the associated mechanism. If I played the devil's adocate I would say that the authors "merely" study an old mechanism with a new diagnostic approach and new data (which, after all, I consider to be very valuable!).

   What *is* important in this paper is the recognition that local (in space and time) reversible modulation of the tropopause region ("adiabatic transient dynamical effect.. ", line 635) creates a TIL in the tropopause-based average, in a certain sense by purely "kinematic" reasons. To be sure, this is implicitly contained in some of the earlier work, but the current analysis brings out the point quite clearly and provides an observational foundation. I think this is the most original part of the paper, and this should be pointed out more clearly.

2. An important part of the argument is contained in figure 5, where panel (a) shows the "full" signal and panel (b) shows the same except with "the daily extratropical wave signal subtracted". What is the "daily extratropical wave signal" and how does this relate to the different spectral subspaces defined earlier? This must be very clearly stated, because it is the basis of the authors' claim that in midlatitudes the TIL is mostly due to "the wave modulation mechanism".

   Related to this issue: what is "instantaneous modulation" in line 484? After all, the maximum temporal resolution in this analysis is one cycle per day, so the analysis is blind to anything operating on a faster scale. It, therefore, seems difficult to make statements about "instantaneous" effects.

3. Is the conclusion on lines 679–681 correct, namely that "the zonal mean seasonal TIL is dominated by waves of synoptic and planetary scales"? After all, the current method is blind regarding fast (sub-daily) gravity waves. How can one then say with confidence that the zonal-mean seasonal TIL is dominated by synoptic and planetary scales? Wouldn't such a statement require a method that includes both types of waves and then allows one to quantitatively estimate their relative importance?

Minor issues

1. I am still not happy with the uncommented use of the shallow water dispersion relation. In their reply the authors state that "quasi-geostrophic theory is an approximation that uses the shallow water equations". I do not agree with this statement. What the authors probably mean is that for linear wave solutions one usually separates the vertical dimension from the horizontal plus time dimension, which after separation of variables effectively leads to a shallow water equation for each vertical mode.

2. The argument in line 218 seems nonlogical: the authors say that if there is no meridional propagation (as they assume), one can set $l = 0$. However, this is not strictly true: there may well be situations with no meridional propagation but still $l \neq 0$, e.g. when $\psi' \propto \cos(ly)$. Of course the reverse is true: if $l = 0$, there is no meridional propagation.

3. Line 273, "extratropical waves have vertical tilts in their temperature structures": I do not think this is generally true. How about synoptic scale waves which are trapped in the troposphere? Shouldn't they be characterized by zero vertical tilt at the tropopause level?

4. Line 307, "commonest": shouldn't this read "most common"?

5. Line 295, what does "filtered out" mean? Does it mean that this is left out (i.e. omitted) by the authors methodology and, therefore, not present in the remaining analysis? Or does it mean that it is accounted for by the filter (i.e. included in the filter) used and, thus, present in the remaining analysis?

6. I do not find the new term “spectrum region” very well chosen. How about “spectral region” or “spectral subspace” or “part of the spectrum”?!