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

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General comments

The article under discussion provides a compelling demonstration of the role of synoptic and planetary wave activity in the (strength of the) extratropical TIL. However, the article's abstract and conclusions leave the overall impression that the extratropical TIL is unrelated to radiation. I think the authors would agree that their conclusion that the zonal-mean extratropical TIL is modulated over time by large-scale waves does not exclude the radiative forcing of the TIL; it just identifies the main wave-dynamical effect on the zonal-mean state of the TIL.

(In the following, literature citations refer to the paper reference list, with the exception of the two references given below.)

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Discussion paper

FDH radiative calculations showed that the extratropical TIL, on average, depends strongly on radiative cooling of the tropopause by water vapor, or on the distribution of water vapor near the tropopause, in midlatitudes (Randel et al., 2007; Kunz et al., 2009) and in polar latitudes (Randel and Wu, 2010).

Dry synoptic-scale dynamics in adiabatic-inviscid flow models accounts for the formation of the extratropical TIL, without simulating its actual strength (Wirth, 2003; Wirth and Szabo, 2007; Son and Polvani, 2007; Erler and Wirth, 2011). Moreover, as the authors probably know, the role of moisture and radiation (mostly the vertical gradient of specific humidity) was eventually recognized to be important in synoptic-scale modeling of the TIL strength; see ref. [1]

Somewhat in between the dynamical and radiative views, another study showed that both thermal radiation and large-scale dynamics are important to the seasonal-mean midlatitude TIL; see ref. [2]. Aiming to understand the persistency of the extratropical TIL, that study not only explained how the vertical gradients of water vapor around the tropopause are key for the average seasonal TIL, but also showed that the midlatitude TIL strength depends not less on local dynamical warming.

Considering the increasing evidence from previous studies, in my opinion, the present article would benefit from adding as a caveat that radiation is likely to play its own role in the extratropical TIL, even if coupled (by some process to be determined by future research) with the synoptic- and planetary-scale waves that modulate the zonal-mean TIL; the problem is that the concluding section only remarks the radiative cooling of the polar summer tropopause. Small improvements concerning the Abstract are suggested below in the specific comments.

Additionally, the literature citations, at least in the introductory section, might be up to date.

[1] Kunkel, D., Hoor, P., and Wirth, V.: The tropopause inversion layer in baroclinic life-cycle experiments: the role of diabatic processes, Atmos. Chem. Phys., 16, 541-560,

doi:10.5194/acp-16-541-2016, 2016. // First published in Jan 2016

[2] Ferreira, A. P., Castanheira, J. M. and Gimeno, L. (2016), Water vapour stratification and dynamical warming behind the sharpness of the Earth's midlatitude tropopause. Q.J.R. Meteorol. Soc., 142: 957–970. doi:10.1002/qj.2697. // First published in Dec 2015

Specific comments

Line 4: the phrase "it also puts other TIL enhancing mechanisms into context" should read as "it also puts other TIL enhancing dynamical mechanisms into context", since radiative effects are not addressed in the study.

Lines 10-11: instead of "The instantaneous modulation by planetary and synoptic-scale waves is almost entirely responsible for the TIL in mid-latitudes", it would be more accurate to say: "Planetary and synoptic-scale waves are almost entirely responsible for the instantaneous modulation of the TIL in mid-latitudes". In this way, the role of radiation is not left out, while keeping the authors' main finding (wave modulation of the mid-latitude TIL).

Lines 18-20: "After many modelling studies (. . .) in the last decade, our study finally identifies which processes dominate the extratropical TIL strength and their relative contribution, by analyzing observations only." The sense of this sentence is questionable because the present and previous works on the extratropical TIL have dealt with different time and/or spatial scales, so they are not easily comparable; besides the FDH sensitivity tests supporting the radiative hypothesis are a mix of observations and radiative modelling. I am sure that the authors do not want to say that the previous theories are all marginal to the subject. Then, it would be more constructive to write something like, "In addition to the TIL enhancing mechanisms proposed by modeling studies in the last decade, our study now identifies which dynamical processes dominate the zonal-mean extratropical TIL strength and their relative contribution, by analyzing observations only."

Lines 363-364: it would be helpful to the reader to clarify the expression “TIL enhancement” (from the values given in the text, it refers to the increase of N^2_max). Since it is used more than once, it should be explained in the introductory part of section 4.

Lines 380-381: when saying that the TIL is ”almost completely gone” after subtracting the extratropical wave signal, the authors meant to say that N^2_max is greatly reduced. This might be clarified in parenthesis.

Lines 501-502: “The only other mechanism restricted to polar summer that could enhance the TIL is water vapor radiative cooling of the tropopause”. The word “restrictive” seems misleading here. I would suggest rephrasing in this way: “The only other mechanism that could enhance the polar summer TIL is water vapor radiative cooling of the tropopause”.

Lines 559-560: Since the text refers specifically to “the remaining TIL”, I would suggest to replace “is enhanced by” –> “is due to”

Figures 5, 7, 9: in Figure 5, we expected to see N^2 values in the range 1-2 x10^-4 s^-2 in the upper troposphere. So, I don’t understand why N^2 is shown in white (or is not shown) below the tropopause. The same applies to Figures 7 and 9, even if the high-latitude upper-troposphere static-stability values are different. I guess this was done to enhance the color map in the lower stratosphere. Anyhow, values not shown in the plots should be mentioned at some point (e.g., figure caption of Figure 5).

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