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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Tropical Tropopause Inversion Layer" *by* R. Pilch Kedzierski et al.

Anonymous Referee #3

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The tropopause inversion layer (TIL) in the tropics is studied using temperatures from GPS radio occultations and dynamical fields from ERA-interim. A strong relationship of TIL strength to tropopause-level divergence is found, with stronger divergence associated with stronger TIL. The authors also analyze the modulation of the TIL strength by the QBO and equatorial waves. They find a stratification maximum located just below the transition line from easterly to westerly QBO phase, which becomes a secondary TIL when this transition approaches the tropopause. Equatorial waves are found to strongly modulate temperatures around the local tropopause (as in previous work in the literature), thereby leading to transient local TIL modulation.

As the authors remark, the tropical TIL has received relatively little attention, so this is a welcome contribution. The paper includes interesting and novel results and is overall well written and easy to follow. However, I do have some major comments (and a number of minor comments) that should be addressed before publication, see below.

Major Comments:

1) divergence-TIL relationship:

The relation of TIL strength to tropopause-level divergence is new and interesting. But what I find puzzling is that convergence apparently does not lead to a reduction in TIL strength (Fig. 3). For DJF TIL strength is independent of the strength of convergence (Div < 0), for JJA it even increases slightly for strong convergence. This seems to contradict the mechanism put forward in section 3.2 (vertical gradient of vertical velocity forcing Nˆ2) and should be discussed/interpreted somewhere in the paper.

Another question I have related to the divergence-TIL relation is: what is the impact of deep convective outflow? Strong tropopause-level divergence would be expected from organized deep convection. Deep convection is known to be associated with the "cold top" (e.g. Holloway & Neelin, 2007; or Paulik & Birner, 2012 who quantified this using COSMIC data) – a strong tropopause-level cold anomaly aloft mid-to-upper tropospheric heating, which should be associated with enhanced TIL. This signal would primarily show up for strong meso- to large-scale divergence. I wonder whether this in part explains the relationship shown in Fig. 3? For large-scale convergence the TIL may locally still be enhanced due to smaller scale dynamics (e.g. gravity waves) and the tropopause-following coordinate.

2) wave-modulation of tropopause

I found the portrayal of the wave-modulation of the tropopause and TIL somewhat confusing. Section 4 is titled "Dynamical Forcing by Equatorial Waves", but what is primarily shown is the quasi-reversible transient modulation. Any wave with a vertical temperature signature will have layers of positive temperature gradient (∼enhanced stratification) and layers of negative temperature gradient (∼reduced stratification). By definition, if the wave propagates through the tropopause, the tropopause algorithm will place the local tropopause near the wave-induced temperature minimum, which, again by definition, puts the layer of enhanced stratification (∼TIL) just above the local tropopause. From that perspective, the TIL enhancement is just a quantification of the wave itself, so cannot be considered a response to the wave (as would be implied by "forcing"). It also doesn't allow the TIL to be considered part of the basic state structure for wave propagation (see authors' motivation in 2nd paragraph of abstract and introduction).

I would urge the authors to be more careful with the wording and interpretations in section 4: what is quantified is the wave-modulation of the tropopause (incl. its TIL structure), not the wave-forcing. It is not clear how much of the analyzed signals are reversible vs. irreversible – possibly, a life-cycle analysis of certain wave types might reveal how much of the wave-modulation is left over once the wave has passed through the region.

3) discussion of applicability to extratropics:

I suggest to either expand Section 5 or remove it – it's not much of a discussion at this point, other than to simply note that there are waves in the extratropics and that a similar analysis could be performed there. The way it stands it would suffice to simply mention this in section 6. If the authors feel it's important to include this section then it should discuss in what way the findings might carry over to the extratropics (or not), given the very different dynamical constraints and physically distinct waves. But again, I don't really see the point of including such a discussion – it seems to primarily distract from the main points of the paper.

Minor comments:

Abstract: the first two paragraphs are very general/generic and can probably be condensed into one shorter paragraph.

line 16: do you mean that you approximate the meteorological situation by the 100 hPa divergence field? The divergence field certainly doesn't completely determine the meteorological situation.

line 18: "new feature": I agree that this is quantified better here, but the QBO–static stability relation was already described in Grise et al. (2010), so by itself is not new

line 36: I believe Randel et al. (2007) were the first to demonstrate this from GPS

line 52: Randel et al. (2007) were the first to suggest this mechanism

line 70/71: Grise et al. show a lag-regression of Nˆ2 to QBO index, which includes the entire lower-to-mid stratosphere

line 91: 100 m is the resolution at which the data is provided, which is not the same as the effective physical resolution – please include corresponding remark (see referenced papers on GPS data for details)

line 101: it's -> it is (and similarly at other places)

line 110: I suggest parentheses around (g/theta)

line 125: remove "empty"

line 176: remove "a" before "6.5%"

line 200 (and at other places): usually the n=0 mode is referred to as mixed-Rossby-gravity (MRG) wave (or Yanai wave) – please clarify

line 234 (and other places): referring to the QBO-associated static stability maximum as secondary TIL could be confusing, as it's not always located near the tropopause – I suggest to distinguish those; another potential issue is that Grise et al. already referred to a secondary TIL at the poleward flanks of the inner tropics, which is different from what is referred to as secondary TIL here

line 285 (and other places): I suggest "analogous" instead of "similar" for the comparison between vorticity-TIL and divergence-TIL relations (vorticity and divergence are distinct meteorological fields, so "similar" may be confusing to some readers)

line 302: "absence of Coriolis force" – I don't understand this comment, isn't this just referring to the continuity Eq., which doesn't depend on the Coriolis force?

line 364 / Figs. 5, 6: why did you decide to show the time-derivatives of T and N^2 (as opposed to just T and N^2)? This came as a surprise to me, so I'd suggest to include a brief statement motivating this choice.

References:

Holloway & Neelin (2007): The convective cold top and quasi equilibrium, J. Atmos. Sci., 64, 1467–1487.

Paulik & Birner (2012): Quantifying the deep convective temperature signal within the tropical tropopause layer (TTL), Atmos. Chem. Phys., 12, 12183–12195.

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