

Interactive comment on “The Tropical Tropopause Inversion Layer” by R. Pilch Kedzierski et al.

Anonymous Referee #1

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General comment

The paper investigates the tropical tropopause inversion layer (TIL). Although this has been the subject of previous studies, the present work provides new knowledge by using a more detailed data set and by doing a more detailed analysis. The authors clearly state in their text what is new and what has been known before, so I think the work is put into context very well. Overall the paper is concise and well written. Nevertheless I have a few comment. These should help to produce a revised version, which I am sure would be a valuable contribution to ACP.

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Major issues

1. I am not an expert on reanalysis data, but as far as I know the quality of reanalysis wind data in the tropics is not as good as one would wish them to be. So the question is: how much can you trust the upper tropospheric horizontal divergence in the tropics? The authors should at least address this issue and try to convince the reader that the quality of the data is sufficient for their purpose.
2. The authors could clarify the role of tropospheric vertical motion and upper tropospheric horizontal divergence for tropical TIL formation, e.g. in their section 3.2. Assuming that a tropospheric wave produces regional upwelling with horizontal divergence right at the tropopause level, this would yield a higher and sharper than normal tropopause (corresponding to a stronger than normal TIL) — essentially by pushing upward the tropopause and thereby making the lowermost stratosphere somewhat colder. In this simple scenario there is *no warming* involved at any point: the TIL forms because the *cooling* ($\partial T/\partial t < 0$) has some vertical structure decaying with altitude. On the other hand, composite plots like Figure 6a indicate actually some *warming* in the lowermost stratosphere. Does this mean that the equatorial waves are associated with *downwelling* in the lowermost stratosphere (right above the tropospheric upwelling), or does this possibly imply diabatic warming?

Minor issues

1. Line 166: What is an e-fold function? A Gaussian?
2. Line 172: How are the profiles shifted in altitude? By how much? For what purpose?

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3. If I recall right, an important point in the work of Wheeler and Kiladis (1999) is the removal of the background spectrum. How is this dealt with in the present work?
4. As a standard reference for the seasonal cycle of the tropical tropopause one should add the paper by Yulaeva *et al.* (1994).
5. Line 258, "... temperature inversion is added to this background profile...": For me, "temperature inversion" means that the temperature increases (rather than decreases) with altitude. It seems that this term should only be used for full temperature profiles, not for perturbations or "additions". So I have a difficulty with the expression "adding a temperature inversion to the background profile".
6. Line 259: "skyrocket" appears too colloquial and not quite fitting here.
7. Line 260, "the N_{max}^2 is very narrow": strictly speaking this is not true. The peak containing N_{max}^2 may be very narrow, not the N_{max}^2 itself.
8. Line 336: How is the significance of the difference between the curves assessed? As far as I know, the significance of the difference in the mean between two distributions is measured by the *standard error* (Press *et al.*, 1992), not by the standard deviation.
9. Line 364: How was the longitude chosen for the plots in figure 5?
10. Line 374, "... tend to be aligned...": Well, this seems to be at least partly wishful thinking, I find that it is sometimes true, but sometimes not.
11. Line 378, "... cooling and/or warming...": this is not clear to me.
12. Line 396 and line 401: Figure 5 shows anomalies of $\partial N^2/\partial t$, not anomalies of T !
13. Line 457, "... a small part...": how do you know that this part is small? Could it be a substantial part?

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14. Line 485, should read: "... would be suited to...".
15. Line 525, "... is rather marginal...": "marginal" may not be the right term here. True, it is smaller than in the corresponding figure 3, but it may yet be significant!

References

- Press, W. H., B. P. Flannery, S. A. Teukolsky, and W. T. Vetterling 1992. *Numerical Recipes. The Art of Scientific Computing*. Cambridge University Press, 2nd edition, 818 pp.
- Yulaeva, E., J. R. Holton, and J. M. Wallace 1994. On the cause of the annual cycle in tropical lower-stratospheric temperatures. *J. Atmos. Sci.* **51**, 169–174.

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