Review of the resivsed manuscript "The tropical tropopause inversion layer: variability and modulation by equatorial waves " by R. P. Kedzierski, K. Matthes and K. Bumke, submitted for publication to ACP

The authors have thorougly revised their manusript. Overall this lead to a significant improvement; in particular, one misinterpretation was detected (by one of the other reviewers) and rectified in the revised manuscript. The authors have addressed my original concerns in some way.

Having said this, I suggest that the authors still could put in some effort in order to improve the text. Below are my concerns and suggestions. Since some of my remarks are new, I leave it up to the authors and/or the editor to decide to what extent these suggestions should be taken in to accout upon revision.

1. A key concept in this paper is "the divergence". The authors should at some point state clearly that they refer to the divergence of the horizontal wind field in this context. To what extent is this "divergence" different from "vertical wind convergence", which is used somewhere else in the text?

2. In my original review I formulated some concern regarding the suitability/accuracy of reanalysis data for the purpose of the paper. I did not find the authors' reply completely satisfying. At issue is not the typical magnitude of the absolute wind error, but rather how accurately the divergence of the upper troposheric horizontal wind field can be calculated. In the tropics this divergence can be expected to be ralated to some extent to convection and tropospheric diabatic heating, and to my knowledge the forecast models used to be somewhat deficient in this respect.

   I suspect that this is not a big issue on the (rather large) spatial scales which the authors consider, and this is also suggested by the consistency of the results. Yet, the authors could provide a short discussion somewhat more to this point, because the usability of the reanalysis data is very important for the results of this paper.

3. I am not really happy yet with the use of the terms "warming" and "cooling". The authors typically consider band-with filtered wave signals, in other words anomalies from the zonal mean. If a plot shows local warm or cold anomalies, this does not necessarily imply that there is/was warming or cooling. It could just as well be the result of horizontal advection, i.e. that the original air was replaced by warmer or colder air, and this would not imply any warming or cooling (neither diabatic nor adiabatic). Using more precise terminology could make the discussion of the processes more lucid.

4. Following on the previous item, the authors should at some point define what Δ means, e.g. in the axis labels of their figure 6; similarly for the use of the term "anomaly", e.g. in line 419; similarly ΔN^2 in the color bar of figure 5.

Minor issues

1. Is "expontially folding function" really a good terminology in order to refer to a Gaussian-shape function?

2. I would like to see a short phrase explaining the "hydrostatic adjustment mechanism" (line 301) such that the reader can understand this mechanism without consulting the reference Holloway and Neelin (2007).

3. Line 333: "correlation of enhanced N^2....": ... with what other variable? (Usually one correlates variable A with variable B)

4. In the paragraph on lines 452–457 the authors talk about heating and cooling rates (K/day) and rates of change of N^2. On the other hand, the figures they discuss in this paragraph (figures 5 and 6) show anomalies Δ owing to the wave. How does this go together?

5. I do not find the statement on line 470 very logical: true, tropical waves may primarily be of dynamic origin, but does this necessarily imply that the radiative signal is small? ... small in what sense?

6. Line 598: should read "As shown....".