

## **Response to Referee #1**

We thank Referee #1 for the helpful comments which helped to improve the manuscript significantly.

In the following, we first explain general changes made in the manuscript, and continue with the point-by-point responses to the reviewer's comments. The referee's comments are in blue font, and our replies are in normal font. Every change made in the revised manuscript is highlighted (please find the highlighted version in the Author Response).

### **General comments:**

#### New subsection 4.3 and Figure 7

Motivated by the specific comments 2 and 3 by Joowan Kim in his review, we added subsection 4.3 to the manuscript in order to discuss how much of the TIL is left without the equatorial wave signal, other mechanisms that could enhance the remaining TIL, and the forcing of the secondary  $N^2$  maximum. Figure 7 compares the time evolution of the equatorial  $N^2$  structure with and without the equatorial wave signal (Thomas Birner asked about this during the SHARP2016 workshop, and we found that making this kind of plot would be the best fit for the purposes of section 4.3).

In Fig. 7 the difference in the TIL region when the equatorial wave signal is subtracted is clear, but the secondary  $N^2$  maximum below the descending westerly QBO phase remains the same, and therefore is not directly modulated by Kelvin waves, as we were suggesting in the discussion manuscript version. Since proven untrue, the paragraphs that discussed the forcing of the secondary  $N^2$  maximum by the filtered Kelvin waves have been erased (now missing from lines 368, 403, 479 and 563), and now we discuss possible forcings in lines 518-527. We still suggest an indirect effect of Kelvin waves (T signal from wave dissipation), but this cannot be captured by our wavenumber-frequency domain filters once the wave dissipates.

#### New Appendix C

We added a caveat about the filtering of waves with periods of less than 2 days from our daily dataset. Spectral ringing can be an issue with these settings, and could leave a spurious signal in our results (Figure 6), but we checked that the contribution of these periods to the calculated equatorial wave signature of inertia-gravity waves is zero, and therefore doesn't affect our results at all.

## Point-by-point responses to Ref#1 comments

### 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.

We agree in that upper-tropospheric winds in ERA-Interim in the tropics are somewhat less accurate than in the extratropics, but we don't think the difference is enough to make tropical 100hPa divergence unreliable for the following reasons:

1) Globally, the performance of ERA-Interim at the 100hPa level is comparable to the operational weather forecast system from ECMWF in terms of root mean squared (RMS) error relative to radiosondes (see Figure 1a from Dee et al., 2011). Note that this figure compares RMS of ERA-Interim 1979 analyses (well before GPS-RO was available), to RMS in operational forecasts in 2007. The 100hPa level in ERA-Interim is as good as one can get from state-of-the-art NWP systems.

2) In the extratropics, the wind difference between in-situ observations and ERA-Interim reanalysis has a 1 standard deviation of about 3m/s for both zonal and meridional winds. In the tropics, this difference at 100hPa is of about 4m/s, meaning that the extratropics have about 75% of the inaccuracy found in tropical upper-tropospheric winds (see Figures 17 and 18 from Poli et al., 2010). Also, the tropical winds at 100hPa don't have the worst performance, since the levels between 120-200hPa in the tropics have a higher 1std difference of 4.5m/s. In addition, the assimilation of GPS-RO observations slightly reduces this differences about everywhere.

3) In-situ observations, radiosondes, have uncertainties as well: several m/s of standard error can be observed applying different tracking techniques, and the errors highly depend on the wind regime, shear and rate of vertical ascent. Also, high-resolution radiosondes include small-scale variations of winds (also up to a few m/s) that cannot be resolved by the model's vertical grid. A thorough description of these issues with wind observations can be found at the "GUIDE TO METEOROLOGICAL INSTRUMENTS AND METHODS OF OBSERVATION" (WMO-No. 8), Part I, chapter 13.

We feel there is no need to discuss this issue in the manuscript, but we added a short sentence in lines 102-104 about it.

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 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?

Regarding divergence, we connect it to convection and tropopause cooling by the hydrostatic adjustment mechanism. Here the suggestions of Joowan Kim were helpful in providing references for a clearer explanation for the sharper TIL with divergent flow (see specific comment 3 of his review). It has to be noted that this mechanism and our results with divergence from Figures 2 and 3 are independent of equatorial wave activity: deep convection (and near-tropopause divergence) can be coupled to an equatorial wave or not, and is represented either way in the diagram of sTIL versus divergence in Fig. 3. We added a new paragraph discussing this within section 3.2, lines 300-307.

The equatorial wave signature in Figure 6a comes entirely from making a tropopause-based mean of the different wave anomalies: it appears because the tropopause is adjusted to the wave anomalies – a ground based mean gives zero. The reason for this is that a Fast Fourier Transform separates a field into a sum of harmonics, which are deviations from the zonal mean. The constant (ground-based zonal mean) term is not included in the wave signals obtained by the filtering method, and the ground-based sum of the positive-negative parts of each harmonic is zero. It is the tropopause undulations and the tropopause-based averaging that enable the signature in Fig. 6a to appear, and we make this clear throughout section 4.2 now.

Our method is suited to compare the signal of the different equatorial wave types on the TIL: therefore the tropopause-based averaging of the temperature and  $N^2$  profiles while creating a gridded dataset, and the tropopause-based averaging of the wave anomalies.

However, conclusions about vertical motion cannot be inferred from Figure 6: the observations we work with are temperatures from GPS-RO and the filtered wave anomalies, and vertical motion is a derived, indirect quantity that can be obtained from models, whose vertical resolution is not enough to enable a study of the relation of upwelling and the small-scale filtered anomalies.

In a scenario of zonal-mean ascent in the upper troposphere, a wave would consist of a harmonic of upwelling and downwelling *anomalies* from this zonal mean: there would be a local cooling effect (to which the tropopause would be lifted by the extra upwelling, therefore adjusting to the anomaly), and a local warm anomaly somewhere else, which would fall above the tropopause since it's not necessarily been lifted there. Thus, the tropopause-based zonal mean would show the dipole of tropopause cooling and warming aloft. Once the wave has a vertical phase tilt (a more realistic scenario, e.g. Figure 5a) this dipole can be present in the same place, otherwise the cooling/warming are in different regions. The warm anomaly doesn't imply downwelling per se, it may as well be less upwelling. In this scenario, the existence of the wave doesn't affect the zonal-mean ascent: only the tropopause horizontal structure and the TIL. Non-linear interactions are needed for a wave to change the zonal-mean flow (e.g. wave breaking), these can be complex and are beyond the scope of our study.

Our method and corresponding results in section 4 were specifically designed to target TIL forcing, and they don't give conclusions about vertical motion related to equatorial wave activity. In section 3.2, divergence is related to vertical wind convergence, which doesn't give information about the actual rate of ascent, just its gradient at that level independently of wave activity. For these reasons, we find that a discussion about vertical motion in sections 3.2 and 4 is very difficult to link to our results while not adding insight about the TIL.

## Minor issues

### 1. Line 166: What is an e-fold function? A Gaussian?

We renamed the function into 'exponentially-folding' (l. 169) for better clarity. Note that the mathematical expression of the weighting function is in line 171.

### 2. Line 172: How are the profiles shifted in altitude? By how much? For what purpose?

The profiles are shifted from a tropopause-based scale onto a ground-based one. We rephrased lines 175-176 to clarify this. The purpose of making tropopause-based averages while gridding GPS-RO profiles is to smooth the TIL as little as possible (l. 190). The filtering has to be done at ground-based levels, since we know the tropopause undulates, adjusts to the equatorial wave signal and is not a constant reference level.

### 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?

In Wheeler and Kiladis (1999) the background power spectrum is calculated to discern which regions of the wavenumber-frequency domain have a spectral signature significantly above the background (Fig. 3 of their paper). We don't present such diagrams in our study. While filtering, the inclusion of background noise is unavoidable, but it appears as a continuum of small amplitude fluctuations: please see the beginning of section 4 in Wheeler and Kiladis (1999). The background spectrum doesn't need to be removed since the filtered wave anomalies appear as bursts of high amplitude compared to it.

### 4. As a standard reference for the seasonal cycle of the tropical tropopause one should add the paper by Yulaeva et al. (1994).

Agreed, this reference was added in line 229.

### 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".

We changed the term 'temperature inversion' for 'dipole of tropopause cooling and warming aloft' in the sentence, see l. 265.

### 6. Line 259: "skyrocket" appears too colloquial and not quite fitting here.

We changed this term for 'increases dramatically' in the sentence, see l.266.

### 7. Line 260, "the $N^2_{\max}$ is very narrow": strictly speaking this is not true. The peak containing $N^2_{\max}$ may be very narrow, not the $N^2_{\max}$ itself.

Agreed, the sentence was changed accordingly (see l. 268).

### 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.

The purpose of the sentence was to infer that a significance test is not needed: the two means are separated by 30 standard deviations, which is really far apart. A common way to assess the significance of the difference in the mean of two distributions is a t-test. The difference between the

Easterly-Westerly QBO  $N^2_{\max}$  distributions is well beyond the 99.9% significance level, as we now explain in lines 356-359. We prefer not to use the term 'standard error' since both distributions are true.

9. Line 364: How was the longitude chosen for the plots in figure 5?

We found that the word 'sections' might have been misleading in the sentence. We changed it for 'snapshots' (l. 380 now). There is no longitude limitation in the plots in Figure 5, note they go from -180 to 180 degE.

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.

We rephrased the paragraph so it immediately specifies that the tropopause adjustment happens where the wave anomalies are large (see lines 393-395).

11. Line 378, "... cooling and/or warming...": this is not clear to me.

We changed this expression for 'dipole of TP cooling and warming aloft' while rephrasing lines 393-395. We hope that the paragraph involved in points 10 and 11 is more straightforward now.

12. Line 396 and line 401: Figure 5 shows anomalies of  $\delta N^2/\delta t$ , not anomalies of T!

Thank you for finding this mistake, the terms were corrected accordingly (now in line 407), and the reference to Fig. 5 in the next paragraph was erased. In lines 411-413 now we refer to our method for clarity, since we do filter both T and  $N^2$  fields. Also note that we do not use time derivatives any more in figures 5 and 6, but anomalies (and averaged anomalies), since our earlier interpretation of these quantities was confusing.

13. Line 457, "... a small part...": how do you know that this part is small? Could it be a substantial part?

We erased the term 'small' from this sentence (now in l. 467). We expect the radiative contribution to be small in that equatorial waves are not radiatively driven and their propagation is explained by dry dynamics. We added this explanation in lines 469-471.

14. Line 485, should read: "... would be suited to....".

Thank you for finding this mistake, it's been corrected.

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!

We agree. The term 'marginal' was changed for 'very small' (l. 590 and also 298).

## References

Dee, D. P., and coauthors,(2011): The ERA-Interim reanalysis: configuration and performance of the data assimilation system, Quarterly Journal of the Royal Meteorological Society, 137, 553-597, doi:10.1002/qj.828.

Poli, P., S. B. Healy, and D. P. Dee (2010): Assimilation of Global Positioning System radio occultation data in the ECMWF ERA-Interim reanalysis, Quarterly Journal of the Royal Meteorological Society, 136, 1972-1990, doi:10.1002/qj.722.

GUIDE TO METEOROLOGICAL INSTRUMENTS AND METHODS OF OBSERVATION (WMO-No. 8) PROVISIONAL 2014 EDITION FOR CIMO-16 APPROVAL. Part I, chapter 13: Measurement of upper wind

[http://www.wmo.int/pages/prog/www/IMOP/publications/CIMO-Guide/Provis2014Ed/Provisional2014Ed\\_P-I\\_Ch-13.pdf](http://www.wmo.int/pages/prog/www/IMOP/publications/CIMO-Guide/Provis2014Ed/Provisional2014Ed_P-I_Ch-13.pdf)