Response to Referee #3

We thank Referee #3 for the helpful comments which helped to improve the manuscript significantly. We are particularly grateful for the suggestion about the hydrostatic adjustment mechanism, which clarifies our results in section 3.2.

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#3 comments

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 N^2) 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.

We are grateful for the suggestion about the hydrostatic adjustment mechanism, regarding the relation between stronger TIL and near-tropopause divergent flow from convection. We added a paragraph in section 3.2 (lines 300-307) discussing this, which improves the explanation about the sTIL relationship with divergence and makes it clearer. We link the vertical wind convergence term to convection as well, since it only has an effect on the TIL with divergent flow (convective outflow). We now suggest that vertical wind convergence is one mechanism enhancing the TIL at all latitudes, but caused by different processes: convection in the tropics and baroclinic waves in the extratropics. This interpretation is added in lines 317-322.

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.

We agree that the use of the term 'forcing' might not be the most correct, it's a good point that it shall rather be considered as an instantaneous modulation. We substituted the term 'forcing' for 'modulation' throughout the paper, also for 'signal' or 'signature' where it was most convenient. We discuss the possibility of further (more permanent) effects of the waves once they leave the tropopause (or dissipate), which could enhance the TIL as well, in lines 513-517.

Regarding the wave signature as a mere quantification of the wave itself, it shall not be viewed as an artifact of the tropopause-coordinate following. Although transient and instantaneous, there are motions associated to the wave signal that locally lift/cool/modulate the tropopause, and also warm the air aloft. Another characteristic of the waves is that they amplify next to and above the tropopause (Fig. 5), and also increase their vertical tilt (Fig. 5a, visible for Kelvin waves), which increases the wave signal in the TIL region, and also increases the area of positive N^2 anomaly above the tropopause. This is a response of the wave to the elevated N^2 values in the lowermost stratosphere, in agreement with linear theory, which in turn enhances the TIL further, working as a TIL-enhancing feedback. We discuss this in lines 480-489, while specifying that more research needs to be done to ascertain such feedback as a robust feature of the tropopause region.

We prefer not to discuss whether the TIL shall be considered part of the basic state structure for wave propagation in our manuscript since it's beyond the scope of our study, and our current results are not enough to fully support (or deny) this. Nevertheless, our results suggest that there is a response of the wave to the higher N^2 values in the lowermost stratosphere (Fig. 5a), which is predicted by theory. But again more research needs to be done in this respect in order to make a robust statement.

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.

Since the submission of the manuscript, there have been developments regarding the application of our method in the extratropics: the method is successful in quantifying the modulation and enhancement of the TIL in the extratropics by extratropical waves, and we are preparing an upcoming paper about this.

We would prefer to keep this section (with some rephrasing in lines 544-548), since it is important to state the usefulness of our method outside the equator. Also note that we do discuss about what is to be expected in the extratropics: see the discussion about baroclinic Rossby waves in lines 531-538 (which are expected to have a bigger signature than Kelvin or any equatorial wave). The role of inertia-gravity waves in enhancing the extratropical TIL is predicted from the modelling study by Kunkel et al. (2014), but is still awaiting confirmation from observations.

Minor comments:

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

We merged both paragraphs into one, slightly reducing its length where possible.

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.

We agree in that we do not show a full meteorological description of the tropical tropopause, but only TIL-relevant parameters. We erased the word 'meteorological' to make the sentence simpler.

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

We agree that Grise et al. (2010) shows a correlation of enhanced N^2 in the layer 1-3 above the tropopause and throughout the lower-mid stratosphere following the easterly phase of the QBO. However, in that paper there is no reference that this correlation creates a second N^2 maximum that is close to TIL strength. We feel that it is justified to call it a new feature.

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

In the paper by Randel et al. (2007) the term 'global' is used in a very nuanced way: they show the "global structure of the *extratropical* TIL". The tropics and the equator are not investigated in this paper. Grise et al. (2010) is the first publication to explore the TIL globally in the literal sense: covering the extratropics and tropics, therefore we would like to keep the reference as is.

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

We agree, we added this reference in the text.

line 70/71: Grise et al. show a lag-regression of N^2 to QBO index, which includes the entire lower-to-mid stratosphere

We agree and we refer to this later in section 3.1.1, but we feel that including this into a TIL-relevant introduction is unnecessary.

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)

We added the corresponding remark in lines 90-91.

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

We corrected this throughout the paper.

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

We added parentheses for both terms in the equation.

line 125: remove “empty”

Corrected.

line 176: remove “a” before “6.5%”

Corrected.

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

We added a clarification in lines 206-207: we don't use these terms for simplicity, since there's already a considerable array of wave types that we filter and constantly refer to throughout the paper.

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

We added a specification in lines 239-240 that this secondary maximum shall not be considered a second TIL. Note that throughout the paper we differentiate between TIL and this secondary maximum (see line 251, 335, 363), and that this secondary N^2 maximum is never referred to as a second TIL, only that it leads to a double-TIL-like structure in static stability (because it looks like it in the stability profile, but the second maximum is far away from the tropopause, and strictly speaking there is no temperature inversion given the background temperature lapse-rate in the stratosphere).

In the paper by Grise et al. (2010) there is reference to two distinct features in static stability (in the layers 0-1 and 1-3 km above the tropopause), not to a secondary TIL.

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)

Thank you for this suggestion, we proceeded with this change throughout the paper.

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?

We now see that this sentence was misleading in the way it was written: we wanted to imply that in the tropics the vertical convergence term is not related to relative vorticity. We erased that part of the sentence for simplicity (line 317 now), since parts of section 3.2 have been rephrased and the separation of the processes driving vertical convergence in the tropics/extratropics are clear now.

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.

We now show these parameters as anomalies (or averaged anomalies) in both figures 5 and 6. The way we interpreted these quantities was confusing in the earlier manuscript, we hope it is more straightforward now.