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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Characteristics of the tropical tropopause inversion layer using high-resolution temperature profiles retrieved from COSMIC GNSS Radio Occultation" *by* Noersomadi Noersomadi et al.

Anonymous Referee #1

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This is a welcome study about the tropical TIL (a region where TIL literature is relatively sparse) and how ENSO and the MJO influence it. The manuscript is well organized and well written, presenting several novel results. I only see one important weakness before it can be published (see major comments 1-3), related to the amount of detail when the authors discuss their results, and how they fit/compare to previous works. Once this has been overcome, it will definitely be worthy of publication in ACP.

I hope the comments included below are helpful for this purpose.

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Discussion paper

Major comments

1

Amount of detail in the result sections.

The text within your first result sections (3.1, 3.2, 3.3) could be shortened. Most of the results presented there basically agree with previous findings by Grise et al. (2010), Son et al. (2011), Kim and Son (2012), Pilch Kedzierski et al. (2016) or Randel et al. (2007) in their latitude or zonal structures and their seasonality. There are very little main results in these subsections that are really new, so one can move on quicker.

Things get way more interesting in sections 3.4 and 3.5, I see lots of novel material. However all is discussed in a hurry compared to the previous result sections. The discussion of the results in sections 3.4 and 3.5 should be extended, because this is the most important and novel part of your study. Related to this, see Major comment 2.

2

Referencing and highlighting what's new. Throughout the results sections I too often don't see what exactly is new and what agrees with previous studies. I'll go section by section here.

-Introduction:

p.2 l.11: more appropriate references are Birner (2006) and Grise et al. (2010), in the sense that they look at the TIL in a global sense, including the tropics. Perhaps also keep Birner et al. (2002) as the first one about the TIL, but the most relevant to introduce your study are the two from above.

p.2 l.15-20: Randel and Wu (2005) studied Kelvin waves from GPS-RO and how they affect the zonal structures of tropopause height and the surrounding T structures. Should be included among Tsuda et al. (1994).

p.2 l.40: I really miss references to Grise et al. (2010) within this paragraph.

-Paragraph in p.4 l.30, and paragraph in p.6 l.30: Grise et al. (2010) did a comparison of LRT and CPT - relative N2 profiles (Their Fig. 2). You should discuss and compare your results to theirs.

-Paragraph in p.6 l.40: I miss a discussion with Randel and Wu (2005), Kim and Son (2012) and Pilch Kedzierski et al. (2016) comparing your results to the modulation of the tropopause by Kelvin waves, MJO and other equatorial waves presented in those studies.

-p.7 l.30: you should compare your S-ab histograms to the ones of TP sharpness in Pilch Kedzierski et al. (2016). Although they use N2max there, this measure is comparable to yours since N2min below TP is always very low and N2max would dominate your distribution of S-ab.

-Section 3.3: discussion with Grise et al. (2010), Pilch Kedzierski et al. (2016), Son et al. (2011), Kim and Son (2012)... is completely missing. These studies show horizontal structures of TP sharpness and its seasonality. Also, note that S-ab is centered around the Equator, while your convective activity by OLR is not, so how can you leave out modulation by equatorial waves out of the discussion? (convectively coupled or not, the amplitudes of eq. waves by definition maximize there)

-Section 3.5: discuss your results comparing to Zeng et al. (2012), Kim and Son (2012) and Pilch Kedzierski et al. (2016), who all showed how MJO modulates the tropopause zonal structures, sharpness or T structure within the TTL with the use of COSMIC GPS-RO.

3

TIL sharpening by convection. Throughout the manuscript I find that discussions could be improved about how convection may sharpen the TIL. See Holloway and Neelin (2007), Paulik and Birner (2012) and Kim et al. (2018) for a detailed mechanism for tropopause cooling/sharpening by convection. A reference to these should be included

in your manuscript.

Also, I suggest to make a plot, for both PO and MC regions, showing a diagram of OLR versus S-ab of individual collocated RO profiles (e.g. in the same grid and day), similar to the diagram of Randel and Wu (2007) with rel. vorticity -vs- TP sharpness. In principle it should show increased S-ab with lower OLR values, at any region. With this you could link the convective influence on the TIL across different timescales (seasonal, MJO, ENSO) and it would be a great complement to figures 10-15 which are climatologies or monthly means.

Minor and technical corrections ## ## ##

Use of 'GNSS' throughout the manuscript: The term 'GNSS' is being used for recent satellite missions such as Metop (A, B and C), and for planning future missions. GNSS is the more general term which includes navigation satellites from all countries, while GPS is the American part. The idea is that GNSS receivers are able to capture signals from more satellites and yield more occultation profiles. Now, the Metop/GRAS instrument stands for 'GNSS Receiver for Atmospheric Sounding' while the IGOR instruments onboard COSMIC stand for 'Integrated GPS Occultation Receiver'. In Anthes et al. (2008) it is always referred to as GPS, the same in subsequent publications. So, for consistency, I see no reason not to use GPS in your manuscript.

Title: It's too general and needs to be more specific. I suggest to somehow highlight zonal structures and the influence of ENSO and the MJO in the title already. This is not the first manuscript to study the tropical TIL globally.

Abstract: First paragraph can be shortened: details about the resolution of your RO profiles or the definitions (S-ab, dH and so on) belong to the Data and Methods section. You can elaborate some more within the second paragraph, and simply use TIL sharpness and thickness instead of the acronyms there.

p.2

l. 12: I think you mean '... very low temperature in the TTL...'

l. 23-25: This sentence is vague and difficult to follow. I suggest to formulate it this way: how ENSO modulates TTL temperature anomalies or wave activity.

l. 30: '(i.e. the sharpness)' doesn't fit there in the sentence. Rewrite.

p.3

l. 4: also mention the MJO and QBO in this sentence, their influence on the tropical TIL was analyzed in this study as well.

l. 6: also mention in this sentence that the real resolution of RO measurements increases in regions of increased refractivity gradients (such as inversion layers above the boundary layer or the tropopause), where it's most needed.

l.27: in 2015 and 2016 the number of profiles is significantly less than that.

p.4 l.1: include a webpage here or within the Acknowledgements.

p.5

l. 6-10: is it a simple mean, or do you apply any kind of weighing to get the grid's value?

l. 38: I'm confused by this sentence, wasn't your dataset always 0.1 km vertically resolved? Then how can your LRT be sensitive to vertical resolution?

p.6 l. 16: I think you mean 'In agreement with previous studies'. Also, refer to those studies.

p.8 l.1-2: I'd erase this sentence, it's too vague.

p.10 l.10: this sentence is too speculative. Could as well be related to ENSO amplitudes within your study's time period, of only one decade.

p.11

l. 21: I think statistics like 'x percent of values of this parameter are within y range'

are unnecessary in the Concluding remarks section, maybe even throughout the manuscript. I suggest instead to use a structure like: 'maxN2 is typically located within 0.5 km above the CPT' or 'typical dH values range within...' and refer to the corresponding figures, so that the important numbers are easier to digest for the reader.

l. 39: what is meant with 'from the new definitions'?

l. 40: as it reads now, this paragraph fits better in the introduction section as motivation or for discussions within your result sections. I suggest to remove it.

I also noticed some errors in your reference list: - First one is Andrews et al. (the 's' is currently missing throughout the manuscript) - The Anthes et al. reference for the COSMIC mission should rather be the one from 2008. - p.13 l.6: Kedzierski –> 'Pilch Kedzierski' (also throughout the manuscript)

Fig. 4: change colour of the blue line to something that contrasts more with the lower N2 values which are also blue.

Figs. 5, 6, 7, and 8: I really need to zoom a lot to see the features you're describing in the text. I suggest keeping the lower boundary at -1 km instead of -3, and removing the contour lines to leave only the color shading for better visibility of the values reached within the TIL.

Figs. 12 and 13: why are two repeated seasonal cycles displayed instead of one?

Fig. 14: could the authors provide a regression coefficient for these plots within the text?

References:

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