Review of "Determination of Tropical Belt Widening Using Multiple GNSS Radio Occultation Measurements" by Darrag et al.

General
In this paper, the authors presented a study of tropical belt widening using satellite measurements. In the first part, they used two sets of tropopause definitions to examine the widening based on the GNSS radio occultation measurements. They also compared the results with those from AIRS and reanalysis data (ERA5). In the second part, they attempted to explain the widening from different aspects, including surface temperature and precipitation, greenhouse gases, and tropospheric column ozone. The studied issue is interesting and meaningful. The topic is suitable for this journal. The presentation is in good quality.

The following are some comments for the authors to consider when revising this paper.

The authors did a good job in the first part. However, the second part is not well done. The authors may just focus on the first part. If they are also interested in exploring the underlying mechanisms for the tropical belt widening, a much in-depth study is required.

In the first part, the authors only presented the results from GNSS RO satellite, AIRS, and ERA5. There is lack of in-depth discussion on these results. For example, why the results are different, which one is more reliable, what the advantages and limitations for each dataset are, how your results compare with literature…

In the second part, the authors presented time series for a variable of interested, and its PC1 time series and spatial distribution. They also calculated correlation coefficients between tropopause height and that variable. However, it is unclear how these analyses related to the tropical belt widening. The discussion is rather superficial. It is hard to understand what the authors' points. For example, how precipitation is connected to the tropical belt widening?

In section 3.3, the authors stated that LRT temperature shows an increasing trend in both hemisphere during the study period. This is strange for the northern hemisphere where tropopause height shows an increasing trend. As known, if tropopause height increases, tropopause temperature generally decreases.

In section 3.5, the authors stated that the correlation coefficient between the surface temperature and the GNSS RO LRT tropopause height is 0.81. Is this high correlation possible? How is this value calculated? How is this value compared with that in the literature?

Specific
L33, replace "pole ward" to "poleward", the same as for the remaining manuscript.

L37-38, please add a statement on what this means to TEL.

L50, TEL first appears in the text.

L64, it should be in Staten et al. (2018) and Adm et al. (2018). The same format applies for the remaining text.

L70, RS first appears in the text.

L90, LEO first appears here and is defined in L97.

L98 and L113, remove ", and ", respectively.

L218-219, what is this function? Is it an area weighted average?

L275, "figure 4" should be "Figure 4". The same applies for the text throughout. For example, in L318, it should be Figure 6 and Table 2.

L321, "On the other side"? this is used a few times, it may not be a correct expression.

L322, "capture" is not a suitable word to use here.

L408, "Precipitation" should be "precipitation", the same for L410.

L473, L476, L478, "For the subjective method", "for the AIRS data,", "In case of objective method", … These are not good expressions. The authors may use different expressions; for example, "based on the subjective method" is better.

Fig. 7, use different letters for the subplots. Two figures (c) are too small.