Response to interactive comments on "The tropical tropopause layer in reanalysis data sets" by Tegtmeier et al.

We thank the reviewer for his/her comments which have helped us to improve the paper in revision. Comments are reproduced below, followed by our responses in *italics*.

Anonymous Referee #1

General:
This a very important and well-written paper. To understand long-term changes in the stratosphere, the tropical tropopause layer (TTL) is the most crucial region. Meteorological reanalyses are best estimates of the true state of the whole atmosphere in the past. As such, they are widely used to examine the atmospheric processes and to detect changes in the climate system. This paper gives important insights into the representation of the TTL in all relevant reanalysis products. Thus, I would like to recommend this paper for publishing in ACP with only few minor comments and some remarks.

General:
I think, this is a very important statement that all reanalyses temperatures at the cold point tropopause (or at the lapse rate tropopause) show warm bias if compared to the observations because of the vertical resolution problem. Interestingly, you also show that the height of the cold point tropopause in all reanalyses is always below that derived from the observations (up to 0.4 km, Fig 6). This is an important point in the current discussion if the (tropical) deep convection is able to cross the tropopause. In many studies, water vapor and ice observations are compared with the position of the cold point tropopause derived from the reanalyses data. Because of a systematic bias of the tropopause position in the reanalyses, the observed enhanced ice/water vapor values can be erroneously attributed to transport across the tropopause. Maybe you would like to discuss this point in your paper.

Thanks for pointing this out. This is indeed an interesting implication of the tropopause altitude comparison. We have added a statement to the summary.

In your discussion of the inter-annual variability you quantify the contribution of the QBO, volcanic eruption and linear trends. However, you do not quantify the contribution of ENSO which is also a "major player" in such variabilities. Is it because you use a zonally averaged picture and to quantify ENSO, the zonally-resolved picture would be more appropriate? If this is the case, I would recommend to state this point more clearly.

Yes, including ENSO in the zonally averaged multilinear regression study does not allow for conclusive results as the zonally varying ENSO signals cancel each other out in the zonal mean analyses. We have also conducted multilinear regression of the zonally resolved temperature fields that will be discussed in a follow up publication, currently in preparation. The manuscript contains a statement explaining this '… The influence of ENSO on TTL temperatures shows large longitudinal variations with positive anomalies over the Maritime Continent and West Pacific and negative anomalies over the East Pacific. While the zonally resolved response patterns agree well between observations and reanalyses (not shown here), the zonal mean responses are not significant (not shown here) …'.

P4 L22: Maybe you would like to mention also more recent papers for "off-line chemistry model applications", like Tao et al., 2019, ACP "Multitimescale variations..."

We have added the reference to the manuscript.

P5, L17-23: I wonder, why SHADOZ data are not mentioned here which are for me still a very important tropical data set

We have not used SHADOZ data, as this record (starting in 1999) is not long enough for the comparison of interannual variability and long-term changes evaluated here for the S-RIP core time period (1980–2010). For the zonal mean climatological analyses of the time period after 2000 we decided to use the GNSS-RO data as their uniform horizontal coverage allows to include tropical and zonal mean comparisons.

P6, L15: You explain "full-input" first in the line 41. Maybe you would like to reformulate

We use the term "full-input" reanalyses here for systems that assimilate surface and upper-air conventional and satellite data (compared to systems that only assimilate surface observations). This information is given in line 15. We slightly reformulated the sentence to make this clearer.

P8, L12-15: "monthly-mean field have a warm bias of 0.5 K compared to 6-hourly data" this is not surprising. I would remove this type of motivation.

We have removed this sentence from the manuscript.

P9, L22-23: "the averaged maxima and minima values" - so you count all minima and maxima and divide it by its number? How do you define a local maximum or minimum? Maybe reformulate. In any case, this procedure is important to understand Fig. 11.

We have added the following information to the manuscript '… For each QBO cycle of this time series, the absolute temperature maximum and minimum are selected. In a second step, the means over all such temperature maxima and minima are calculated to give the averaged maximum and minimum values, respectively …'

P17, L4: I would count "volcanic" as a tropospheric variability

As the positive temperature anomalies in the upper TTL associated with volcanic eruptions are related to volcanic stratospheric aerosols, we have decided to list volcanic here under stratospheric variability.

P19, L5-6: "During the first 15 years" - or you mean during the last 15 years (higher altitude and lower pressure - I would expect the other way around)

Thanks for pointing this out. We refer here to the first 15 years and the wording was mixed up. The sentence has been corrected in the manuscript.