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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The tropical tropopause layer in reanalysis data sets" *by* Susann Tegtmeier et al.

Anonymous Referee #1

Received and published: 22 July 2019

Review of the paper:

"The tropical tropopause layer in reanalysis data sets"

written by Tegtmeyer at al.,

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

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Discussion paper

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 discus this point in your paper.

- General:
  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.

- 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..."

- P5, L17-23:
  I wonder, why SHADOZ data are not mentioned here which are for me still a very important tropical data set
- P6, L15:
  You explain "full-input" first in the line 41. Maybe you would like to reformulate
- 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.
- 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.
- P17, L4:
  I would count "volcanic" as a tropospheric 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)

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