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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global Tropopause Altitudes in Radiosondes and Reanalyses" *by* Tao Xian and Cameron R. Homeyer

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1 General comment

1) The fairly large bias in MERRA-2 is interesting and I was surprised that it didn'ÂĂÂŹt receive more attention by the authors (at least not in the writeup). After all, this is a (re)analysis, i.e., it includes a modern data assimilation scheme, presumably assimilating the radiosonde observations that here used as a reference. So my expectation was that all modern reanalyses essentially reproduce the tropopause. Fig. 1 furthermore stimulates suspicion: how can a reanalysis have such large temperature biases ($>$ 5 K!!) in the upper troposphere? Without labelling I would have guessed that this is a free-running model. Don't you expect all modern reanalyses to very closely agree

about temperature in the upper troposphere? This is the case between the other three products: ERA-Interim, JRA-55, CFSR. Is this simply an outlier example or do you often find such large biases in MERRA-2? Is this something that's documented in the literature? To be honest, if this is a robust bias in MERRA-2, then this product shouldn'ÂĂÂŹt be used for UTLS studies . . . in any case, this requires more discussion by the authors.

After careful re-evaluation of the MERRA-2 fields we were using for the profiles in Figure 1 (added quickly after the request during initial review before passed on to open discussion), we discovered that the wrong reference levels were used. Instead of using the pressures and altitudes in the middle of the model layers that correspond to the temperatures, we were using the pressures and altitudes of the model levels (the edges of the layers). This resulted in an artificial displacement of the profile of approximately 500 m in the UTLS. We have corrected this error in Figure 1 and the remaining analyses in the paper, for which it had little impact on the results (except for the bias analysis). The revised analyses clearly show that MERRA-2 is consistent with the remaining reanalyses in its representation of UTLS temperatures. Many thanks to the reviewer for emphasizing this point.

2) Vertical resolution is mentioned at many places to potentially explain differences between radiosondes and reanalyses. Isn'ÂĂÂŹt this easily testable? You could degrade the radiosondes to the model resolutions and see if that really explains the differences. You could even study some of the characteristics (e.g., double tropopause frequency) as a function of vertical resolution by gradually degrading the radiosonde data. Perhaps the authors have already tried this, in any case, I would strongly suggest to include corresponding results / discussion in the paper.

Thank you for the suggestion. We have degraded the radiosonde observations to the vertical grid of each model and recomputed the bias and RMS differences. Bias and RMS differences in instantaneous primary tropopause altitudes show little sensitivity, but large reductions in both are found for double tropopause frequencies. This point

has been clarified in Sections 3.1 and 4 in the revised manuscript and reflected in the revised analysis presented in new Figures 2 & 3 and Table 1.

2 Minor comments

page 2, line 16: "uncertainty that is comparable to the vertical resolution of the modelÂĂÂİ"ÂĞ this makes intuitive sense, but is this a priori clear given that you interpolate between levels for the tropopause calculation?

The value given by interpolation of the temperature profiles has been clarified in Section 2.2. The interpolation only assists in routinely satisfying the second criterion of the WMO definition and the criterion for identifying multiple tropopauses. Therfore, yes, we do expect it to be clear a priori that uncertainty should be comparable to the vertical resolution of each model.

page 2, line 19: the lapse rate is equal to minus the vertical temperature gradient

Corrected.

page 2, line 28-29: Anel ref'ÂĂÂŹs

Reference has been added at P2, L25-27 of the revision.

page 3, line 7-8: sentence doesn't work like this; how about: "PV, which is conserved . . ., is commonly used for transport studies in the extratropics and often used to define a dynamical tropopause . . ."

Done.

page 3, line 10: "threshold used varies considerably"ÂĘ seems like an exaggeration (I'd suggest to remove ÂĂÂIJ"considerably"ÂĂÂİ), note a lot of the STE studies (e.g., Wernli group and others) use 2 PVU and this value seems to be used mostly

Done.

page 5, lines 11-12: these are somewhat subjective choicesÂĘ have you checked the corresponding sensitivity? E.g., are the results sensitive to obtaining tropopause levels from the native horizontal and vertical grid, and interpolating to the 1-by-1 lon-lat grid afterwards? IâÂĂÂŹm also not sure I understand the purpose of oversampling to the 200-m grid in the vertical for tropopause identificationÂĞ please provide rationale (relevant for line 24 as well).

We have evaluated the sensitivity to these choices and it is negligible. Interpolation in the horizontal dimension has no effect on the tropopause other than reducing the level of horizontal detail (which is advantageous for apples-to-apples comparisons of the reanalyses and is how we have locally archived the data for long-term use). Some text has been added to reflect the lack of sensitivity to the choice of synoptic time here (Section 2.1). In addition, see previous response for detail on the need for vertical interpolation prior to tropopause identification.

page 6, line 24: how do you assess whether data points are roughly evenly distributed?

We checked the length of time gaps in the tropopause altitude time series for each station, and selected the stations with maximum gap duration less than 5 years (there were only 59 stations included with gaps longer than 3 years and these were manually evaluated to confirm there were no deleterious effects on the trend analysis - e.g., missing long time chunks at the beginning and end of the 35-year analysis period). This point has been clarified at P2, L33 of the revision.

page 7, line 8-9: do you do this separately for the two hemispheres? How do you then handle the equator, which in the relative coordinates "ÂĂÂIJmoves"ÂĂÂİ around?

Yes. For plotting, any data extending beyond the equator is trimmed. We have added some clarifying points in Section 2.4.

page 7, line 13-14: so here you suggest that you do use the native model grids for

tropopause calculations, in contrast to the description on page 5 please clarify

This point has been clarified at P7, L30-31 of the revision.

page 7, line 18: is this bias a function of latitude?

This bias is derived from global observation, the variation with latitude has been included in new Figures 2 and 3.

page 8, discussion of Fig. 2: have you considered normalizing these RMS differences by a measure of internal/natural variability (e.g., interannual standard deviation)? Larger RMS differences would be expected in regions with larger internal variability, so part of the latitudinal differences could be related to different internal variability.

Indeed, the large internal variability can result in large RMS error in some regions, such as the extratropics. The variability of tropopause altitude in these regions is mainly attributed to the subtropical jet shifting latitude, which is associated with north-south migration of the tropopause break. We have not attempted to normalize these RMS differences in the revision, but have expanded the bias analysis to reflect points raised by other reviewers.

page 9, line 5: over the Atlantic trends are larger at the edges of the tropics compared to the equator, which stands in contrast to the statement of "uniformly upward trends throughout"

This has been changed to "larger upward trends".

page 10, bottom (Figs. 7, 8): not sure these Figures need to be included in the paper, perhaps as supplement is enough? They don'ÂĂÂŹt look that much different from the Eulerian versions (as the authors remark) and aren'ÂĂÂŹt discussed much either.

We believe the differences between these tropopause break-relative analyses and the Eulerian analyses, though small in some respects, are important to show in the paper and to the discussion included (despite the fact that it is relatively brief).

page 11, bottom paragraph: this discussion based on differences in how O3 is handled is useful and should be extended a bit: notably, ERA-Interim and MERRA-2 are very different in this regard with ERA-Interim using a climatological O3 product in their radiative scheme and MERRA-2 using its own O3 field so the effect of O3 on the tropopause and its trends will likely be very different between these two reanalyses.

The description of differences in ozone assimilation between reanalyses has been expanded beginning at P13, L34 of the revision.

page 11, line 33: please clarify that you are referring to anomalous upwelling and downwelling (the full residual circulation is still downward over the polar latitudes)

Corrected.

page 12, line 28: awkward sentence structure (âĂĂIJSignificant trends . . . were found to be increasing . . .âĂĂİ) - please modify

This has been changed to "Significant increasing trends in double tropopause frequency were found nearly everywhere in the radiosonde observations ...".

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