Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2018-945-AC2, 2019

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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Received and published: 7 February 2019

1 Major Comments

My major comment is that the authors should have included detailed analysis for both bias and RMS while validating. While the authors tend to emphasize the RMS statistics, I hope they know that the RMS, as a loss function, gives a relatively high weight on large errors since it is more sensitive to extreme values on long tails/outliers due to the fact that the errors are squared before they are averaged. So, sometimes very few extreme values can completely change the statistics, which is not desirable for this analysis.

In contrast, the bias is sometimes more intuitive because it tells us how much of abso-

lute differences between the radiosonde and the reanalysis. However, the bias analysis is not perfect because the positive and negative biases will cancel out. I would separate the bias analysis into positive and negative, with positive means the reanalysis primary tropopause showing at a higher altitude, and negative means the reanalysis primary tropopause showing at a lower altitude. Meanwhile, adding the frequency (with respect to total samples considered) of positive and negative bias. In this way, we know that on average how frequent and how much the reanalysis would overestimate/underestimate the tropopause height. I think this detailed analysis is more meaningful to the community.

In this sense, it is fair to always include both bias and RMS error analysis. I think Fig. 2 should include bias on the first panel and RMS error on the second panel. For each panel, different shapes represent different months, but please do include one more statistics for all-season averages. Then, add another figure that repeats a similar analysis for the double tropopause. Having the easy visualization of the statistics, still, keep Table 1 of detailed numbers for easy reference.

Thank you for these comments. We now separate the bias analysis into positive and negative, which is listed in the new Table 1. Bias analysis is also included for primary tropopause altitudes and double tropopause frequency within different latitude bands, with statistics for all-season averages added (new Figures 2 and 3). Necessary changes have been made in Section 3.1 in the revised manuscript.

Another major comment is on the accuracy of IGRA data, and its ability to precisely document the lapse-rate tropopause is crucial for this study. It helps if the authors could iterate in more details on how the $<=$ 50m vertical resolution of raw observation are eventually reported in only 1.5-2.5 km vertical resolution at the UTLS (although in the revised manuscript the authors changed to $>$ 1km). The "$>$ 1 km" is still less desirable for studying the vertical variability of temperature records - it will miss effects of both gravity waves and the Rossby waves acting on the temperatures. Given the reported resolution, why not using GPS/COSMIC temperature records that has a better coverage? The focus of the paper is from 5-20 km, in which COSMIC is totally capable of seeing waves on temperatures.

If I understand it correctly, Figs. 1-2 and Table 1 are the only places that the authors performed apple-to-apple comparison by collocating the reanalysis to the radiosonde locations. For all other analysis, the authors just reported trends inferred from gridded results at each latitude-longitude box, so the results could be biased by sampling sizes. So, the first part of validation is more meaningful to my sense. That said, personally I am not interested in the trends reported. For example, what does a trend of +/- 50 m/decade in primary tropopause mean? What does a positive trend of double tropopause frequency mean in specific reanalysis? Unless you can elucidate the possible cause of trends with proof, I donâÂĂÂŹt think the trend numbers themselves have significant meaning. On the positive side, the fact that different reanalyses showing different trends is meaningful in that they imply how unreliable the reanalyses are as to the tropopause analysis. This makes me wondering if it is necessary to include the trend analysis, especially in such a large portion of the paper. If I were the authors I would report the bias and errors in more details to help the community to understand the different performances of the reanalyses.

There is perhaps some confusion on the typical resolution of the IGRA profiles here. In the discussion paper, it is stated that reduction of the radiosonde profiles to mandatory and significant levels only *can* result in vertical resolution larger than 1 km, but this is a worst case scenario. As Figure 1 demonstrates, the vertical resolution of the IGRA data is often finer than this. We do not believe additional detail on the process of reporting mandatory and significant levels beyond what is provided in Section 2.2 is necessary, but we have clarified a few points there in the revision.

As for the suggestion to use GPS/COSMIC temperature profiles to investigate tropopause characteristics, we would like to do that in the future. However, an obvious shortcoming of the GPS/COSMIC temperature records is the limited temporal coverage, which is not suitable for studying the long-term changes in tropopause char-

acteristics (the primary of the focus of this paper). We have added acknowledging this on P5, L21 of the revision.

Finally, the purpose of focusing on tropopause trends is motivated by the review given in the Introduction. Namely, tropopause altitude trends are believed to be an indicator of climate change as increases in tropopause altitudes often occur with increases in tropospheric temperatures. In addition, double tropopause occurrences provide a physical perspective of UTLS dynamics (most notably STE). Evaluating long-term trends provides a unique insight into these processes. Comparing model trends with observed trends are also another method of model validation, so we believe analysis of trends is well-justified and relevant to the scientific community (as also evidenced by the remaining reviews of the paper).

As for elucidating the sources of the trends, we have drawn on complementary results from other recent efforts in Section 4. We have also expanded some of the discussion on these trends there by considering potential physical and dynamical sources (most on P13 of the revision).

A last comment is that I do hope the authors could put more emphasis on the physical meaning/causes of the (large) differences among different reanalysis. So, beyond the vertical resolution, could there be any other reasons that caused the discrepancies? The current version seems to be less scientific and more like a technical report.

We have added text and additional analysis evaluating the effects of vertical resolution (see Tables 1 and Figures 2 & 3 of the revision). Apart from vertical resolution, it is quite difficult and beyond the scope of this study to identify the reasons for oftentimes subtle differences between the tropopause trends in the reanalyses. The tropopause reflects the combined impacts of a long list of choices in model design and assimilated data, so elucidating the role of each in controlling trends in tropopause characteristics is a daunting task. Vertical grid spacing is an ideal target for initial evaluation, given that the tropopause definition depends greatly on it. Thus, we have limited our detailed evaluation to this single source in the revision. To examine the role of other aspects of the model design, comprehensive sensitivity studies using a single modeling system are likely required.

2 Minor wording comments

1. P1L8, attributed > attributable

Corrected.

2. P1L9: observations

Done.

3. P1L9: and reanalyses > and the reanalyses

Done.

4. P1L9: analysis period

analysis period has been clarified.

5. P2L10: > the UTLS composition

Not changed.

6. P1L13-15: this sentence doesn'ÂĂÂŹt make sense

Revised to improve clarity (P1, L12-14 of the revision).

7. P3L6-7: this makes sense because of the existence of the ozone layer, but can you be more specific about it?

This point has been clarified (P3, L4-8 of the revision).

8. P4L1: I donâĂÂĂÂŹt understand the logic here. If the radiosonde data is so lim-

ited, why bothering using them instead of COSMIC data? Plus, this part sounds like belonging to the discussion part.

See previous response to similar comments.

9. P5L10-12: all reanalyses are reported in sigma or eta coordinates. From conversion you might get temperatures on pressure levels easily, but how did you get them in altitude coordinates? Did you use simultaneous geopotential heights to interpolate the data? Be more specific about how you preprocessed the data.

This point has been clarified at P5, L12 of the revision.

10. P5L16: > quality-controlled

Corrected.

11. P5L23-24: one comment is that this linear interpolation doesn't change the shape of the profile, at all. So, you typically end up with the same value without doing interpolation.

An interpolation is needed to verify the second criterion of the WMO definition and the criterion for identifying multiple tropopauses. We have clarified the value provided by interpolation in Section 2.2.

12. P7L18: tropopause altitudes in MERRA-2 > primary tropopause altitudes in MERRA- 2.

Done.

13. P8L16: is maximum > its maximum

Done.

14. P13L11-12: how did you reach this conclusion?

Similar to Rossby wave breaking leading to transport of tropical UT air into the extratropical LS, double tropopauses can be formed by equatorward transport of extratropical LS air into the tropical UT during wave breaking events. We have added a relevant citation to this conclusion [Liu and Barnes (2018)].

References:

Liu, C., and Barnes, E. A. (2018). Synoptic formation of double tropopauses. Journal of Geophysical Research: Atmospheres, 123, 693–ÂĂÂŞ707.

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