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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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I found this study quite interesting for the ongoing research on the tropopause region in a climate perspective. It gives considerable consistency to previous evidences indicating a positive decadal trend both in the global tropopause height and in the extratropical double-tropopause frequency – at the expense of stressing the discrepancies between the results inferred from reanalysis products and RAOB, as well as showing differences among different reanalysis models.

I also think that the paper could be improved and hope that the comments below can be valuable in the eyes of the authors.

Main aspects:

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1) Concerning the reproducibility of results, the paper lacks information about the radiosonde data used in the study: how were IGRA stations selected in the first place? The number of selected stations (317) and the corresponding amount of observations for 1985-2015 are given later in the results section, with their approximate locations shown in the Figures. But IGRA (version 2 released in 2016) contains temperature data from 800–900 radiosonde stations within the studied period. Nothing, however, is said about the choice of stations, concerning the homogeneity of time-series in terms of temporal and vertical features (i.e., leaving aside the much more difficult problem of instrument biases): temporal regularity and continuity; vertical resolution around the tropopause.

2) A linear interpolation to a 200-m regular vertical grid was applied prior both to radiosonde and reanalysis temperature data before tropopause identification. The authors claim this was done "in order to enable reliable tropopause identification". This phrase is potentially confusing to the reader. Evidently, an interpolation is needed to verify the second condition of WMO's definition of first tropopause, as well as to look for a second tropopause. But a linear interpolation simply does not change the lapse rate between the known data points. So, the estimation of the first and second tropopause levels is essentially limited by the resolution of data – as the authors in fact recognize in other parts of the paper. The gain resulting from the interpolation scheme should be explained to make this point clear.

3) Radiosonde data were analyzed at the principal synoptic hours, 0000UT and 1200UT, whereas reanalysis data were analyzed only at 0000UT. This means that half of the time-zones on the global reanalysis fields of temperature (at latitudes outside of the polar regions, after averaging over one or more years) is represented by daylight times, while the other half is represented by nocturnal times. In this respect, in Figs. 3-6 it is not clear why some radiosonde stations show 0000UT average values while others show 0012UT values, since reanalysis-derived values refer always to 0000UT. Also, considering the diurnal variations of the tropopause height, it should be explained

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how the radiosonde–reanalysis tropopause differences listed in Table 1 were exactly calculated.

4) Although not obligatory, to be more informative Table 1 should depict hemispheric seasons. Or perhaps individual months, but then restricting to North Hemisphere, where the amount of radiosonde data (used as reference to errors) is much larger there than in the South Hemisphere.

5) The calculation of tropopause altitude needs a bit of clarification: is moisture included in the hypsometric equation? 'Tropopause altitude' refers to geometric altitude or geopotential altitude?

6) Maybe the large discrepancies between the results obtained from CFSR and the other reanalysis models (seen in all plots) deserve a slight explanation.

Secondary aspects:

P2, L19. Where it reads "(. . .) (also known as vertical temperature gradient) (. . .) " it correctly should read "(. . .) (negative of the vertical temperature gradient) (. . .)"

P4, L1. "(. . .) since they are only launched from land masses". Considering the radiosondes launched on whether ships and 'ships of opportunity' (even if not used in the study) it should be better to write "(. . .) since they are mostly launched from landmasses".

P4, L6. "Reanalyses assimilate global high-quality observations (. . .)". Do not forget to mention other observation platforms besides radiosondes. Moreover, I doubt that all observations assimilated in reanalysis models are of "high-quality". A meteorological reanalysis is supposed to deal with inaccurate and incomplete observations to some degree. "Quality-controlled" is closer to reality.

P4, L 30. I don't understand the words "a physical perspective of the UTLS'. I suppose that the authors' point is that their paper provides an evaluation of reanalysis-model performance regarding the UTLS temperature structure.

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P6, L12. “Thus, we are confident that IGRA data are suitable for tropopause analyses following the methods employed here.” How can you tell, from a demonstration with two random soundings from one site? The study uses nearly 10ˆ5 soundings from over 300 radiosonde stations! The above assertion is not acceptable. Although Fig. 1 serves the purpose of illustration of the idea, paradoxically, expressing here some uncertainty would give more confidence to the reader.

P7, L14-16. It’s not totally clear whether Fig. 3 (and so on) uses only four months per year or not.

P13, L16. “(. . .) increases in primary tropopause altitude are associated with a warming climate (. . .)”. The suggested connection is supported by a very few modeling experiments until now. I’d replace “are” by something less assertive like “is probably” or “is believed to be”.

Fig. 6 and Fig. 8. If possible, the color scale legend “Double tropopause frequency” should be changed to “Double tropopause trend”.

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