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Interactive comment

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

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- in page 1 line 23 I miss a citation to Anel et al. (2006). This work also deals with the trends from radiosonde data and indeed it will be useful to discuss some issues later in the paper;

Thank you for the suggestion. The citation has been added.

- in page 2, after line 17: usually there is some confusion on the issue of definition of the tropopause. Words have meanings and being fair it only exists one definition for the tropopause, the one established in 1957 by the WMO. Others are criteria to approach the behavior of the ÂĂÂŹtropopauseÂĂÂŹ or UTLS transition according to the best fit for different studies, campaigns, etc. This does not change the reality of the complex

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atmospheric behavior, but using the right words is useful for those not so familiar with the topic that could waste time looking for formal definitions that do not exist anywhere. Therefore in line 18 it is not âÂĂÂŹThe conventional tropopause definitionÂĂÂŹ but ÂĂÂŹThe tropopause definition;

This has been changed to "The *original* tropopause definition" to retain useful context for discussing alternative definitions in the remainder of this paragraph (beginning P2, L13 of the revision).

- in page 2, line 22: in some way linked with the previous issue, I do not think that it is correct to say that there are exceptions to performance. Simply there are regions of the Earth where the UTLS structure is so complex that there is not a tropopause or transition troposphere/stratosphere as such. You mention one case where this behavior is mostly driven by the very specific tropospheric radiative balance during the austral winter. But it is not the only case. The same happens in the third-pole (the Tibetan Plateau) but because of dynamical reasons. There unstable mix of air can make impossible to get a troposphere-stratosphere distinction because of the high altitude of the plateau and its radiative balance (see Chen et al. 2013 and Chen et al. 2016);

Mentions of "performance" were removed and complex, layered stability structures were also acknowledged (P2, L18 of the revision).

- page 3 line 13: indeed fifteen years before Hoinka et al. (1998) had clearly established that the usual values of 1.6 PVU introduced in a campaing in the 1980âÂĂÂŹs or the âÂĂÂŹpopularâÂĂÂŹ 2 PVU value underestimate the reality of the tropopause height (obviously in extratropics and polar regions);

The references have been cited, and text has been changed a bit at P3, L13 of the revision.

- subsection 2.1 "Reanalysis output": for the purpose of this work, more relevant than this information (vertical levels and top) is to know the distribution of levels (or dz) between 200 hPa and 50 hPa. I would recommend to the authors to focus the description here on this layer. This will enable them to simplify the understanding and discussion of results later, for example in section 3;

The information of model vertical resolution in the UTLS has been added in Section 2.1, as well as a more direct reference to the Fujiwara et al reanalysis comparison paper.

- page 6 lines 5-10: this is a good exercise to guarantee representation with a case study. But this had already been proved by Antuna et al. (2006) using other station at a quite similar geographical location. I recommend to cite the work to add extra support and to include in the text the coordinates for Corpus Christi (unless I have missed them);

The coordinates for Corpus Christi have been added to the text (P6, L13 of the revision). Citation has not been added because this illustration is dataset specific (i.e., showing the level of detail between full-resolution data and reduced resolution data in IGRA) and the Antuna paper focuses only on mandatory-level radiosondes and the impacts of missing mandatory-level data for climatological analyses.

- in subsection 2.4 you state ÂĂÂŹthe 35-year analysis period. I have not got clearly what is the period of study: 1979-2015? This is 37 years. 1981-2015?. Please, clarify it;

We have added a parenthetical reference to the time period analyzed here (1981-2015) to remind the reader (P6, L28 of the revision).

- page 8, lines 15-16: there is another basis for this (one of them briefly mentioned in the paper), the competing phenomena of tropical widening where the tropical tropopause overlaps the extratropical one and the horizontal meridional entrainment of extratropical air to tropical regions (Wang and Polvani, 2011; AÃ±el et al. 2012; Castanheira and Gimeno, 2011).

Text modified by also acknowledging double tropopause seasonality (P9, L1-3 of the revision).

- subsection 3.2, first paragraph: this is in agreement with the results for the Scenario 1 studied by AÃ±el et al. (2006). That is, raw series without data homogenization. Thought IGRA solved several of the problems that existed in CARDS, here you do not perform any change-point detection technique and this restricts the validity of your results. I think that the issue of not undergoing change-point detection deserves to be mentioned here and that a comparison in the text with the values obtained by AÃ±el et al. (2006) and Santer et al. (2003a,b) would be good as it would enable readers to get a more complete picture of the state-of-the-art.

The point on Siberia deserves special attention in my view: this is also in agreement for with part of the Scenario 1, and with Scenarios 2 and 3 of Anel et al. (2006). Here I would point out two different issues:

1. some of the radiosonde series in this region show up to a 1% significant correlation with the Northern Annular Mode, this could explain partial regional trends. But as soon as in the 1960âÂĂÂŹs Makhover reported that this region has a special behavior in comparison with similar latitudes in this hemisphere (check Antuna et al. 2009 or the original Russian books cited therein);

2. no doubt it deserves a deeper analysis with data homogenization techniques, but there is a potential reason that could explain bias (be aware that I talk about bias not changes in trends) over the region corresponding to the former Soviet Union. This rea- son is the use of different radiosondes with very different equipment than the ex- tended Vaisala RS80/RS90 radiosondes for other parts of the world. A quick check of the metadata in IGRA shows how some stations over the period 1980-1990 there was up to 4 or 5 changes of radiosonde model (changes, not simple updates) and in some of them radiation corrections in 90âÂĂÂŹs. This kind of problems with sound- ings over Russian territory with frequent radiation corrections was also pointed out by

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Makhover (again see AntuÃ±a et al. 2009). This could have an impact on any trend computed. Therefore any statement on trends without change point detection and data homogenization should be accompanied of one on the limitations of the data analysis.

Thank you for these comments. A comparison in the text with the values obtained by Anel et al. (2006) and Santer et al. (2003a,b) has been added at P9, L17-19 of the revision. In addition, the limitations of not using change point detection or data homogenization have been acknowledged at P7, L21-25 of the revision.

- subsection 3.3, last sentence: I think that it could exist a partial explanation for this behavior in Fig.4 for CFSR. This is my hypothesis: as it has been proved by Anel et al. (2008) in presence of multiple tropopauses the first lapse rate tropopause (LRT1) is lower than when a single tropopause exist and multiple tropopauses are not present. As Xian and Homeyer show CFSR has lower bias and increased resolution at UTLS levels. This enables this dataset to better represent a bigger number of multiple tropopause events. Having more multiple tropopause events means that an increasing proportion of lower LRT1 cases should be found. This should be more clear in critical regions for the detection, such as subtropics. Therefore the positive trend in the frequency of multiple tropopauses and lower bias of CFSR would be driven an increased frequency of lower LRT1.

Nothing changed. Comparing tropopause altitude trends to the double tropopause trends in CFSR, there is no significant increasing trend in double tropopause frequency in the extratropics where decreasing tropopause altitude was found. Thus, the connection between the decreasing primary tropopause altitudes and increasing double tropopause events mentioned by the referee is not robust in the extratropics for CFSR. Moreover, the remaining reanalyses show increasing double tropopause frequencies and increasing tropopause altitude in most regions.

- page 10, lines 9-15: this is exactly what is stated in Castanheira et al. (2009) (Fig. 8) using IGRA data and a probable consequence of the energetic modes at UTLS levels.

I think that the numbers here obtained should be compared to their ones and the work cited.

Thank you for bringing this work to our attention as we were not aware of this double tropopause trend analysis. We cited the work and compared our results to their trends of double tropopause frequency in two latitude-bands (30-60N and 30-60S) at P11, L11-13 of the revision.

- page 13, line 14: I do not think that "found" is the right word here. To be fair beyond the useful contribution on comparison between state-of-the-art reanalysis, the other results here presented only confirm previous findings existing in the literature and it should be acknowledge in this way.

Replaced "found" with "shown" here (P15, L8 of the revision).

- Table 1: I understand that values in this table are computed using all the stations, independently of the hemisphere. This could provide a sense of average changes, but if you present the results for months representative of seasons, what is the point on mixing NH and SH stations?. Doing such thing does not let to appreciate the true seasonal change. In my view exposing only the values for extratropical regions of one of the hemispheres would be the right way of doing it, as there is no point on including the tropics because of the lack of seasonal variability. Moreover Double tropopauses are a phenomenon with strong seasonal dependence associated to extratropical wintertime UTLS baroclinicity (Castanheira et al. 2009) and therefore the same reasoning applies.

Good points. We have removed the seasonality from Table 1 and shown the total evaluation numbers only. The new Figures 2 and 3 summarize the results for season and location (extratropics, subtropics, and tropics).

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