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

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Although I have not been formally invited to review this paper, as it is line with a big part of my research along the last decade I have read it carefully and I have some comments that I would like to share with the authors. They could want to include them in the final version.

First of all I would like to congratulate Xian and Homeyer for this valuable effort. This analysis brings some light on reanalysis bias at UTLS levels and updates previous results existing in the literature.

Also I would like to clarify that although I mention in my comments several of my previous works, my intention is to highlight some points that can help to support part of the

results here exposed and to bring a balance to the discussion. I miss in the text some punctual comparisons with previous works (many of them overlooked in the current version and in some way creating a misrepresentation of the topic) that I think would help us as readers to get a better picture of the research topic, instead of having to go through different papers.

More specifically:

- in page 1 line 23 I miss a citation to Añel 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;

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- 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 'tropopause' or UTLS transition according to the best fit for different studies, campaigns, etc. This does not change the reality of the complex 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 'The conventional tropopause definition' but 'The tropopause definition';

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- 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);

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- 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 1980's or the 'popular' 2 PVU value underestimate the reality of the tropopause height (obviously in extratropics and polar regions);

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- 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;

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- page 6 lines 5-10: this is a good exercise to guarantee representation with a case study. But this had already been proved by Antuña 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);

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- in subsection 2.4 you state 'the 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;

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- 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 entraintment of extratropical air to tropical regions (Wang and Polvani, 2011; Añel et al. 2012; Castanheira and Gimeno, 2011).

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- subsection 3.2, first paragraph: this is in agreement with the results for the Scenario 1 studied by Añel 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 Añel 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 Añel 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 1960's Makhover reported that this region has a special behavior in comparison with similar latitudes in this hemisphere (check Antuña 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 reason is the use of different radiosondes with very different equipment than the extended 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 90's. This kind of problems with soundings over Russian territory with frequent radiation corrections was also pointed out by Makhover (again see Antuña 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.

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- 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 Añel 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.

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- 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.

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- 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.

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- 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.

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