

*Interactive comment on “Reanalysis comparisons of upper tropospheric/lower stratospheric jets and multiple tropopauses” by Gloria L. Manney et al.*

*J. A. Añel*

We thank Dr. Añel for his interest and his helpful comments. His comments are shown in blue in italics, and our responses in black.

*Major comments:*

*My main concern with this study is about the definition that the authors use for the tropopause and multiple tropopauses. Being clear about this the document to cite in the second paragraph in page 6 is the WMO definition of the tropopause (see below) and it could be that the computation of multiple tropopauses that the authors have done is wrong, as the WMO definition states that a multiple tropopause needs a vertical thermal gradient of 3 degrees to be considered as such, and not 2 degrees (Celsius not Kelvin according to the original definition to be fair). This should be double checked by the authors and figures replotted if necessary.*

We apologize for not being more explicit about the definition we use in the paper. As is described in detail in Manney et al (2011, 2014), we followed Randel et al (2007), who found that for relatively coarse vertical resolution reanalyses such as these, relaxing the definition such that the vertical thermal gradient for the secondary tropopause was only 2 C / km (or equivalently 2 K/km) resulted in multiple tropopause distributions that were more comparable with those obtained from high-resolution measurements such as those from GPS-RO or radiosondes using the more stringent definition. We will state this explicitly in the revised paper.

*Other issue is that the paper does not include a proper discussion on how the results here shown compare to the existing literature. This is specially important because this paper deals with reanalysis and previous results include radiosonde or GPS-RO data. Therefore I think that it would be really useful a section discussing the results of multiple tropopauses (at least for the well known planetary hotspots) in comparison with those obtained by Schmidt et al. (2006), Randel et al. (2007) (already cited) and Añel et al. (2008). Again I acknowledge that the focus of the paper is on the intercomparison, but maybe a good idea of doing this is to include in the discussion the spread of the reanalysis respect to the existing literature (e.g. the reanalysis with the minimum value for MTs over Japan is X with a value of Y and this is in the range (or not) of the values obtained by previous works). Maybe a table for the four hotspots of the North Hemisphere and South Hemisphere would be a good idea.*

Using the MERRA reanalysis, Manney et al (2014) provided a detailed climatology of upper tropospheric jets, multiple tropopauses, the subvortex jet, and the relationships between these features. Detailed discussion is given in that paper of the multiple tropopause distributions in relation to those in previous literature, including Randel et al (2007), Añel et al. (2008), and Peevey et al (2012) (see Manney et al, 2014, pages 3257--3259, and the paragraph spanning pages 3263 and 3264); Añel et al (2008) and, especially, Peevey et al (2012) in turn contain

detailed comparisons of multiple tropopause distributions with previous studies. Given the extensive comparisons already in the literature, including Manney et al (2014) with one of the reanalyses used here and with exactly the same methods, and the fact that the focus is on intercomparison, we do not feel it is important to include further detailed comparisons with previous studies here. It is difficult, if not impossible, to quantitatively compare these studies because of the vast differences in geographic sampling and methods (bin size, normalization, etc) used to construct the frequency distributions; Manney et al (2014) contains some discussion of spatial and temporal sampling effects that may result in some of the differences seen between results from previous studies.

A comparison of multiple tropopauses in these reanalyses with those derived from other datasets such as GPS-RO, radiosondes, or relatively high-resolution satellite datasets such as HIRDLS, if done using consistent methods across all datasets and accurately accounting for geographic sampling differences, would be a valuable study but is well beyond the scope of this paper.

We will, however, add a paragraph explicitly referring to these previous comparisons, and giving the “spread” among the reanalyses considered here.

*Also along the text and figures I have not seen a clear statement on what the authors mean by 'multiple tropopauses'. Multiple tropopauses should not be confound with double tropopauses. Right now there is a pretty clear distinction in the literature about this. From the manuscript I guess that the authors refer to double tropopauses all along the text and not multiple (e.g. triple tropopauses or above). A clear statement of what is considered as multiple tropopause should be included. For example, are you mixing cases of double tropopauses and multiple tropopauses? this could lead to inhomogeneous results because of the vertical resolution of the reanalysis. Anyway a clarification is needed.*

As discussed in Manney et al (2011, 2014) and Schwartz et al (2015), the term “multiple tropopause” is used to mean any profile with more than one tropopause, and “double tropopause” to mean profiles with exactly two tropopauses. These definitions are consistent with those that we have seen used in the extensive literature on double and multiple tropopauses. Thus, as quantified by Schwartz et al (2015), there are a very small fraction of profiles included in this analysis that have more than two tropopauses. Including or excluding these does not change our results in any significant respect. We will add a note to this effect in the methods description in the revised paper.

*Minor comments:*

*page 2, line 22: 'they'*

When a colon is used to divide independent clauses, and there are two or more sentences that follow from the clause preceding the colon (as is the case here), the word following the colon is capitalized.

*page 2, line 32: Chen et al. 2013 shows a nice case study along three field campaigns with radiosondes, combined with GOME-2 ozone data, lagrangian transport modeling of STE exchange and jet analysis that could be helpful to additionally support this view:*

Thank you for this citation -- we will include it with the others in support of this point.

*Along the text the surname 'Peña' is not well written, it would be good to write it correctly with the 'ñ'. It is just necessary copy+paste or with LaTeX to write it as 'ñ'*

We apologize for this oversight; it has been corrected.

*page 3, line 20: it would be good to put the 'th' as uppercase*

In the references I find to this reanalysis (e.g., the online information on it at NOAA), the "th" is not capitalized.

*there are some minor typos along the text, please double check them.*

We will, indeed, proofread the revised paper very carefully, and already have some corrections pointed out by the referees.